सत्यमेव जयते

INDIAN AGRICULTURAL
RESEARCH INSTITUTE, NEW DELHI.

I. A. R. I. 6.
MGIPC—S1—[illegible]R/54—7-7-54—10,000.

THE
SCIENTIFIC MONTHLY

44438
IARI

VOLUME LXVII
JULY TO DECEMBER

PUBLISHED BY THE
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
1515 MASSACHUSETTS AVENUE, N.W., WASHINGTON 5, D. C.
1948

Copyright 1948 by
AMERICAN ASSOCIATION FOR
THE ADVANCEMENT OF SCIENCE

BUSINESS PRESS, INC.
LANCASTER, PENNSYLVANIA, U.S.A.

THE
SCIENTIFIC MONTHLY

JULY 1948

TWO TELESCOPES AND THE NEW UNIVERSE

R. S. UNDERWOOD

Professor Underwood (Ph.D., Chicago, 1930) is professor of mathematics at Texas Technological College, where he also teaches some elementary astronomy, the field of his original interest. He writes about astronomy only as an observer—with more than the usual amount of technical knowledge. Dr. Underwood is the author of Jaunts Into Space *and of numerous articles and textbooks*

THE year 1948 marks the beginning of the reign of the new king of instruments on the throne of Palomar Mountain, California. Meanwhile, the royal predecessor, trapped on Mount Wilson near Los Angeles but still carrying on bravely over a flood of harrying light spilled out from the city, has already written into the history of human thought one of its major chapters. In its off-the-world affairs and speculations, the twentieth century is being dominated by two telescopes; and in my opinion the newest of these will be fortunate indeed to match the other by the yardstick of over-all accomplishment.

Nevertheless, the astronomical age now starting is one of justified optimism. Even the more thoughtful members of the human race, deadened as they are to the scientifically marvelous by having seen, in pictures at least, the very pillar of flame that dramatized the birth of the atomic age, await with calmness the first authentic report about an unexplored region some seven times larger than the present known universe. Surely this should be news of more than passing moment, even on such a crowded and self-centered planet as ours!

The time seems propitious, therefore, to set the stage for the interested onlookers. If we are to be prepared properly for the astronomical revelations likely to be forthcoming, we must have at least a rough understanding of those that have gone before. As befits the importance of the occasion, several articles dealing with some aspects of the unfolding story have already appeared. None of these, naturally, has exhausted the subject; and most of them seem to have plunged into the what-to-expect part of the discussion before the lay reader has had time to adjust his mental sights to the cosmic picture that is already before him. It will be a major aim of this article to convey to the reader some conception not only of the universe as it now stands revealed, but also of the tremendous change in outlook that has taken place between two world wars. And perhaps those last two words will explain in part the strange fact that few among the well-read people of this earth have the faintest idea why 1924 is one of the most important dates in the history of science.

Our discussion will be divided into three parts: The first will deal with the new telescope itself; the second and largest division will be concerned with the developing picture of the universe, which, incidentally, has swelled to new dimensions in my own lifetime and has culminated in the great instrument now almost ready to make the next enlargement; the third and final part will discuss the typically cautious predictions, made by the qualified astronomers who will carry on the work, with reference to the nature of the new things the telescope may reveal. I reserve the privilege, however, of supplementing these reasonably reliable forecasts with some less trustworthy speculations of my own (duly labeled as such) made in the light of my personal version of the major facts in hand.

Mount Wilson Observatory

THE 100-INCH TELESCOPE AT MOUNT WILSON

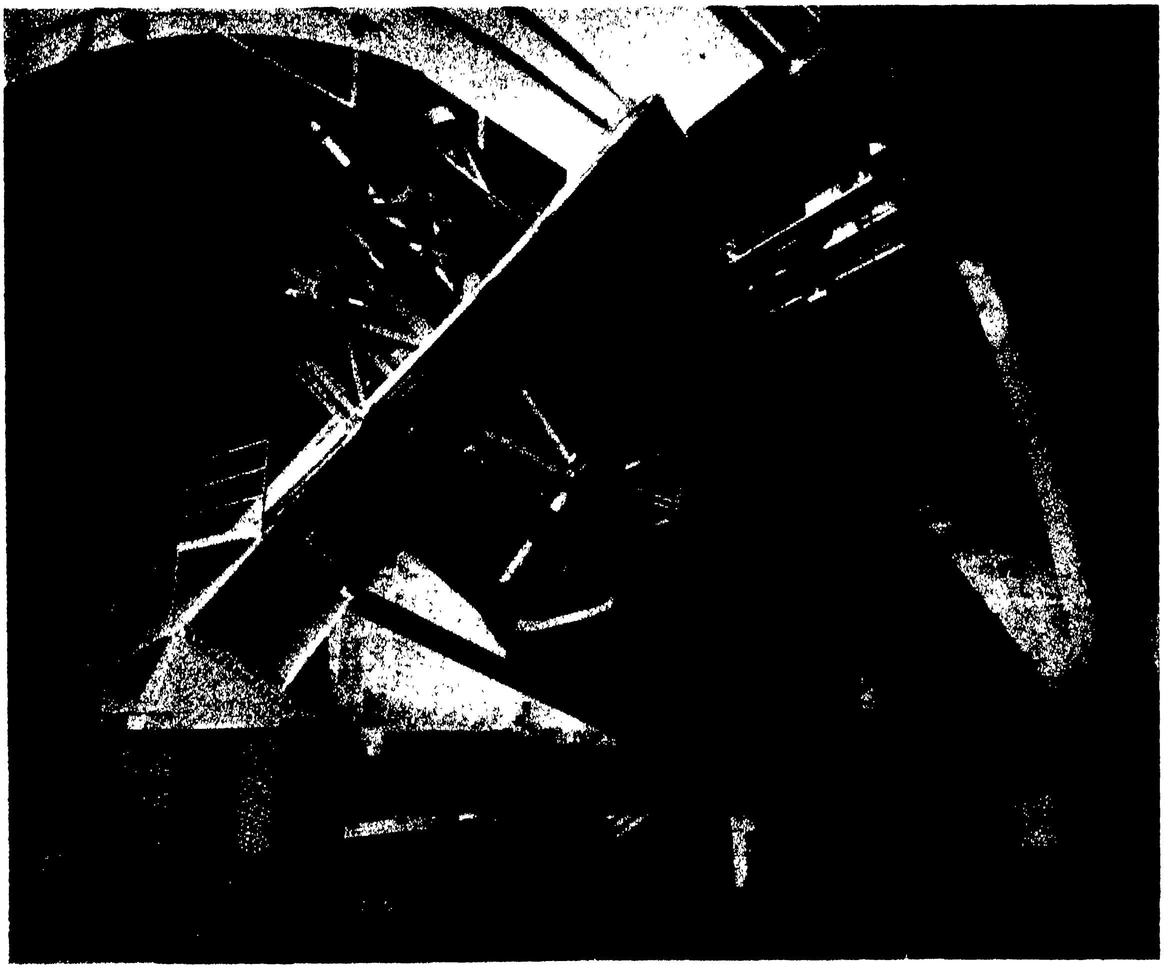
UNTIL 1948, THIS WAS THE LARGEST REFLECTING TELESCOPE IN THE WORLD.

THE spectacular success of the 100-inch telescope (of which we shall have more to say) resulted not only in a revolutionary new outlook on the universe, but also in an idea in the mind of one man that may prove to be of equal or greater importance. That man was the astronomer George Ellery Hale, and the idea was that of a telescope that would dwarf even the giant reflector on Mount Wilson. In 1928 the idea bore its first tangible fruit in the form of a six-million-dollar grant to the California Institute of Technology by the General Education Board of the Rockefeller Foundation. The grant was for the construction of a reflecting telescope with an aperture of 200 inches, to be built somewhere in the West Coast area.

The very conception of this plan, as a matter of fact, was more daring than it may appear in offhand retrospect. For the intricate engineering problems involved in the details of mounting and operating an instrument which combines the massiveness of a locomotive with the delicacy of a wrist watch are such as to make the addition of a few inches in the diameter of a projected telescopic mirror a matter for concern, long study, and technical ingenuity. But here the step-up was not of inches, but of yards! Here was the first call, with details as yet to be supplied, for a mirror with four times the light-gathering power of the greatest optical eye then existing—one able to reach galaxies roughly twice as far away as the most distant ones yet recorded, and therefore to bring into the realm of the explorable an approximate sphere of space about seven times as large as that now known to man.

It is clear, then, that the task of designing and building a telescope of such unprecedented proportions was one not to be taken lightly. In fact, the very length of time involved in making the dream come true bears testimony to the unexpected problems that were met and overcome in the twenty-year-long epic of trial and error and vexing delay. Of course, a world war and the consequent suspension of work on the project were added reasons for the excessive time consumption; and it probably is not true that at any time the final fulfilment of long-deferred hopes seemed even to border on the impossible. Given the necessary financial resources, it was what might be called a routine task of extraordinary difficulty which was eventually completed, just as those who were involved expected it to be. Nor was the delay an unqualified misfortune. Important advances of various kinds, particularly in photography, have called for modifications of the evolving design which take full advantage of new information and new techniques, so that the completed instrument is practically up to date in every respect.

With regard to personnel, Dr. John A. Ander-



NEW MONARCH OF THE SKY

SHOWING PRINCIPAL FEATURES OF THE NEW 200-INCH TELESCOPE, MINUS THE GREAT MIRROR, WHICH HAD NOT YET BEEN INSTALLED. AT THE UPPER LEFT IS THE CYLINDRICAL TUBE CONTAINING THE PRIME FOCUS, IN WHICH, FOR THE FIRST TIME, THE OBSERVER RIDES WITH THE TELESCOPE. *Left center*: MOVING PLATFORM FROM WHICH HE CLIMBS TO HIS POST.

Mount Wilson Observatory

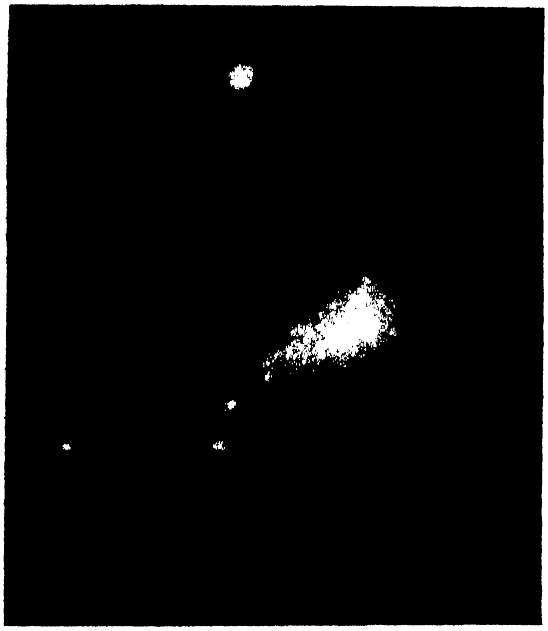
CORONA BOREALIS CLUSTER

A FEW OF THE HUNDRED MILLION GALAXIES THAT THINLY PRICK THE BLACKNESS IN THE PART OF SPACE THAT IS NOW REACHED BY THE 100-INCH TELESCOPE AT MOUNT WILSON.

son, of the Mount Wilson staff, has been in charge of the project from the start, and there has been excellent cooperation among the scientists concerned. In fact, it has been agreed by the directors of Carnegie Institution, which sponsored the bold adventure in applied science at Mount Wilson, and of "Cal Tech," which received the Rockefeller grant, that the operation of the two great observatories should be a joint venture in research under a common director. That highly responsible position has now been filled by Dr. Ira Sprague Bowen of Cal Tech, a physicist and astronomer whose special field of "extreme ultraviolet spectroscopy and nebular spectra" carries a strong hint of the type of investigation that will most certainly be stressed.

There is not space here for the whole lengthy story of trial and frustration; of how, six years after the project started, at a time when it was to have been well along toward completion, the huge glass disc was finally poured at Corning, New York, only to break in its mold. Two years later the successful Pyrex slab, weighing 14.5 tons in spite of its ribbed and hollow construction, began its carefully routed journey to the laboratory of Cal Tech. There it was ground, tested, polished, retested, and repolished, in what seemed to be an interminable quest for perfection. From 1942 to 1945 the war called a complete halt, and then the polishing began anew. Finally, in the late months of 1947, the mirror was pronounced satisfactory, which meant that it missed parabolic perfection, in the roughest spots, by something less than a millionth of an inch.

In the meantime, the long-sought site for the telescope and housing observatory had been agreed upon. Long before the last soft strokes were applied to the oversized jewel in Pasadena, preparations for its setting were complete on Palomar Mountain, which is a huge, flat-topped geologic block some 127 miles to the southeast. A two-million-dollar highway, 6.5 miles long, had been built along the side to the summit under the supervision of experts from San Diego, about 45 airline miles away. Now the beautiful dome of the observatory proper was complete, and so was the colossal telescope, except that a concrete slab still did balancing duty for the piece of glass on whose performance rested the whole fate and fame of the Palomar project. It is true that other astronomical work with other instruments was carried on in the far-from-idle community, which, incidentally, required a dormitory and seven cottages to house its personnel; but to the interested public, at least, nothing that mattered much could happen in advance of the Great Moment. That moment, of course, as envisioned by the more naïve, would occur when, with the great mirror in its appointed place, the slit of the dome would open majestically and some wide-eyed scientist, Adam's apple aquiver, would press his eye to the magic circle and shout excitedly at the marvels there revealed.

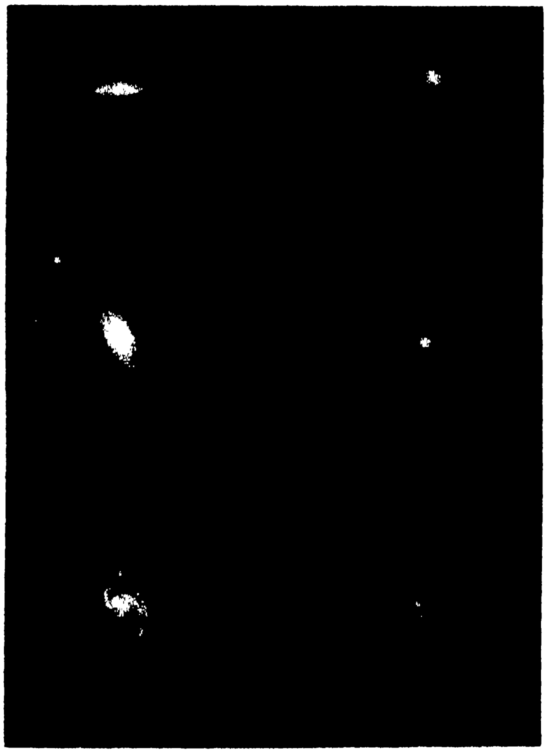


Boyden Station, Harvard College Observatory

SMALL MAGELLANIC CLOUD

NAMED AFTER THE EXPLORER, THIS SEEMINGLY DETACHED PORTION OF THE MILKY WAY IS EASILY VISIBLE TO THE NAKED EYE FOR OBSERVERS IN THE SOUTHERN HEMISPHERE. IT IS, HOWEVER, ABOUT 80,000 LIGHT-YEARS AWAY.

It is perhaps a pity that such a dramatic climax, after the long years of preparation, could not conveniently be arranged. Long before the dedication ceremonies the epoch-making First Glance was probably diluted and deglamorized in the course of several months of routine adjustments. Doubtless many have read that the 200-inch telescope is actually just a huge camera through which the working astronomer will seldom glance directly, and that its precious time will be devoted primarily to the study of distant galaxies. This is true, and yet not the whole truth. There is no optical law that will bar the use of the simple observing eyepiece, and there is probably no ethical code for astronomers and opticians that will prevent someone from taking an unofficial peek at even such an unworthy object as the moon. Perhaps, therefore, we can save a smudged corner of the romantic picture after all. Probably the First Peek, spiced with a bit of well-concealed excitement, has already gone into the side lights of history; and it even seems likely that some strategically placed feminine savant has learned by now what the New Look really means.



Mount Wilson Observatory

TYPES OF NORMAL AND BARRED SPIRALS

THESE ARE THE LARGEST KNOWN STRUCTURAL UNITS, EACH ONE CONSISTING OF MILLIONS OF SUNS. ABOUT ONE HUNDRED MILLION OF THEM ARE SCATTERED THINLY IN THE PART OF SPACE REACHED BY THE 100-INCH TELESCOPE.

Boyden Station, Harvard College Observatory

LARGE MAGELLANIC CLOUD

THOUGH SMALLER THAN OUR GALAXY AND NEAR ENOUGH TO BE CALLED "SATELLITES," BOTH MAGELLANIC CLOUDS ARE PROBABLY INDEPENDENT GALAXIES. THEY ARE OF THE SO-CALLED IRREGULAR TYPE, THOUGH A HINT OF THE RELATIVELY RARE BARRED SPIRAL FORM MAY BE DETECTED HERE.

And now, before we turn to the second phase of our story, let us take a peek of our own at the site of the unfolding drama, and at the mighty instrument that will play the leading role.

Why Palomar? In the first place, the mountain is not too far away from Pasadena and the interested scientists at Cal Tech and Mount Wilson. Of course, the site must be relatively high and in a predominantly dry, clear, calm atmosphere. Actually, the base of the telescope is 5,600 feet above sea level; and the air, which tends to cloud up and swirl about a sharp peak, is quiet on the broad, tree-covered top of Palomar. Then, too, the single mountain block is off to one side of the geologic fault where earthquakes are most common. Finally, it was desirable that the unreachable cone in space that surrounds the south celestial pole for observers in northern latitudes be reduced in size as much as possible; for that reason it was fortunate that this ideal location was rather far to the south.

As for the telescope itself, the bald figures involved are probably feeble substitutes for the actual sight of it. If, however, one tries to visualize a disc of glass which, with the sheathing around it, towers vertically across two stories of a six-story building, and then pictures this as the moving end of a swinging, steel-girdered tube which, when upright, reaches to the top of this building, he may get a little of the feel of the proportions involved. Tonnage means little, but it may be worth recording that the telescope weighs 1,000,000 pounds altogether, of which some 280,000 pounds, or the weight equivalent of a healthy locomotive, are balanced so delicately that they move under gentle pressure. As in all large telescopes, the mounting is of the so-called equatorial type, which permits smooth, automatic, clock-driven following of a given celestial object. A less usual feature, however, is the horseshoe-shaped construction of the north end of the polar axis. This axis runs parallel to that of the earth, and in the case of the 100-inch it bars access to a limited region around the north celestial pole. The 200-inch, swinging northward inside the huge horseshoe, can point at the pole star and its neighbors at will. Thus, the new giant will have the advantage of a greater northern as well as southern reach in available area for study as compared with its nearest rival, in addition to its well-known advantage in the matter of space penetration. And when we consider the mighty deeds of the deposed champion eye of the world, made without the technical refinements of the past three decades, the prospects for new conquests by the favored successor look bright indeed.

THE accelerated tempo of events on earth, in the span of years between two world wars, has been more than matched in the realm of ideas. In physics and chemistry, the universe of the minute has been unlocked for all to marvel at, and the whole world knows of the atomic bomb. In astronomy, the steps have been equally gigantic; but the advertising department has fallen down on the job, and the man in the street—even the supposedly well-informed one—has had little inkling of the things that were going on inside the nonpractical heads of the stargazers. He would be surprised, and probably skeptical, if he were told that in his own lifetime, and in part at least through the agency of the 100-inch telescope, an Age of Revelation has torn away a major veil from the face of the universe itself.

He knows about the intellectual revolution symbolized by Copernicus the thinker and Columbus the actor some four and a half centuries ago, when to an ever-swelling group of knowing ones the stationary earth rounded into a spinning ball, and the near-by stars, now stilled in their "crystalline sphere," moved backward and outward into space. Those were the stirring days when the early giants of modern thought, from Galileo to Newton, began to roil the waters of complacent stupidity with such mighty splashes that even the slumber of the masses was disturbed. This is history; and this is moderately well known. But how many realize that a mere quarter of a century ago, in the age of flappers, gangsters, and diamond-studded pocket flasks—during, in fact, the course of a single year or two centering about 1924—quiet announcements were being made that deserved to ring out in the world of science like the notes of a cosmic bell. One of the two or three great hours of human awakening came and passed; and then Coolidge did not choose to run, the stock market crashed, and the dying echoes of a scientific rumor, little heeded at any time, faded away at the sound of footsteps in the bread lines of the world.

Perhaps this sounds like journalistic exaggeration rather than an attempt to evaluate the event described in careful and precise language. It is true that there has been no restraint, and that I have used all the emphasis at my command to call attention to one of the greater moments in the history of human thought. Is this an unwarranted pose—an overrating of one discovery among a dozen others of like importance? Let us see.

At this point the injection of a personal note may help to bring full realization of the change of outlook that has taken place in one lifetime. To make it clear, however, that my judgments are impersonal and objective, let me say at once that I have been merely an interested nonprofessional observer in this field, and am speaking as a reporter rather than as one who has had even the tiniest of roles in the drama itself.

In 1916, when as an undergraduate at the University of Minnesota I was absorbing the current picture of the universe about me, the scene was vast even then, though perhaps a bit dismal. Well do I remember the stirring feel of distance and emptiness and a far-flung universe of suns—of remote, solitary, and majestic orbs which were like the sands of Sahara in number, but which faded in the farther zones to sparks in silhouette against the blackness of engulfing space. For here was the central fact of the universe as it then was known: the ranks of the stars were thinning as men plumbed the outer reaches of space. Black holes in the Milky Way served as windows through which we could look outward and into the void. It was not altogether a pretty picture.

These were the facts of observation, though it is not to be inferred that no one suspected the grander view that was to come. Already a number of fine photographs of the so-called spiral nebulae had been made, particularly by Keeler; and there were those who argued that the spirals were themselves great systems of suns like the stellar horde that we see from the inside as the Milky Way. The matchless modern observer, E. E. Barnard, who photographed the Milky Way and described it beautifully, believed correctly that the apparent holes were not windows at all, but dark patches of intervening cosmic dust which blotted out most of the light from the stars beyond. But these were still just theories, and for all anyone knew the spiral nebulae were like the other huge blobs of rocks and dust in which some of the stars are suspended. In fact, the very name "nebula," or "cosmic cloud," testifies to this point of view. They could be of the order of size of the Great Nebula in Orion, which is only ten light-years, or some sixty millions of millions of miles, in diameter. They could be mixed with, and lighted by, some of the stars in the one observed system, though certain peculiarities in their distribution made this conclusion seem doubtful to some. Nevertheless, the one great generalization of the day, which came out of patient observation and keen deduction, was that of the ultimate exhaustion of the starry horde in the distance. This, in the larger aspects, was the state of astronomical science in the first two decades of the twentieth century.

What happened then? We must skip most of the details; but one significant event was the building of a reflecting telescope with an aperture of 100 inches, at Mount Wilson, California. Another was the more or less casual observation that a certain type of variable star called a "Cepheid" (so named from the constellation in which one appears) gives away its distance as compared with fellow-Cepheids in a very simple manner. This particular type of star changes in brightness at regular intervals, and the longer the interval between "winks," the greater is the actual or intrinsic brightness of the star. This observation was made by Miss Henrietta Leavitt, of Harvard Observatory, with regard to the Cepheids in the smaller Magellanic Cloud, a seemingly detached portion of the Milky Way, visible in far southern skies, whose stars are all roughly the same distance away. Later, this very important "period-luminosity relation" was confirmed and extended in a program which involved many observatories and astronomers, and for which Dr. Harlow Shapley deserves much of the credit. The suns involved were giant ones that could be seen as individuals at tremendous distances. The links in the chain of evidence were being forged. And then, late in 1924, the pieces of the jigsaw puzzle came together and the new cosmic picture stood revealed, consistent in outline, grand in contour, and surprisingly indisputable, once it was seen.

Mount Wilson Observatory

TWO VIEWS OF JUPITER

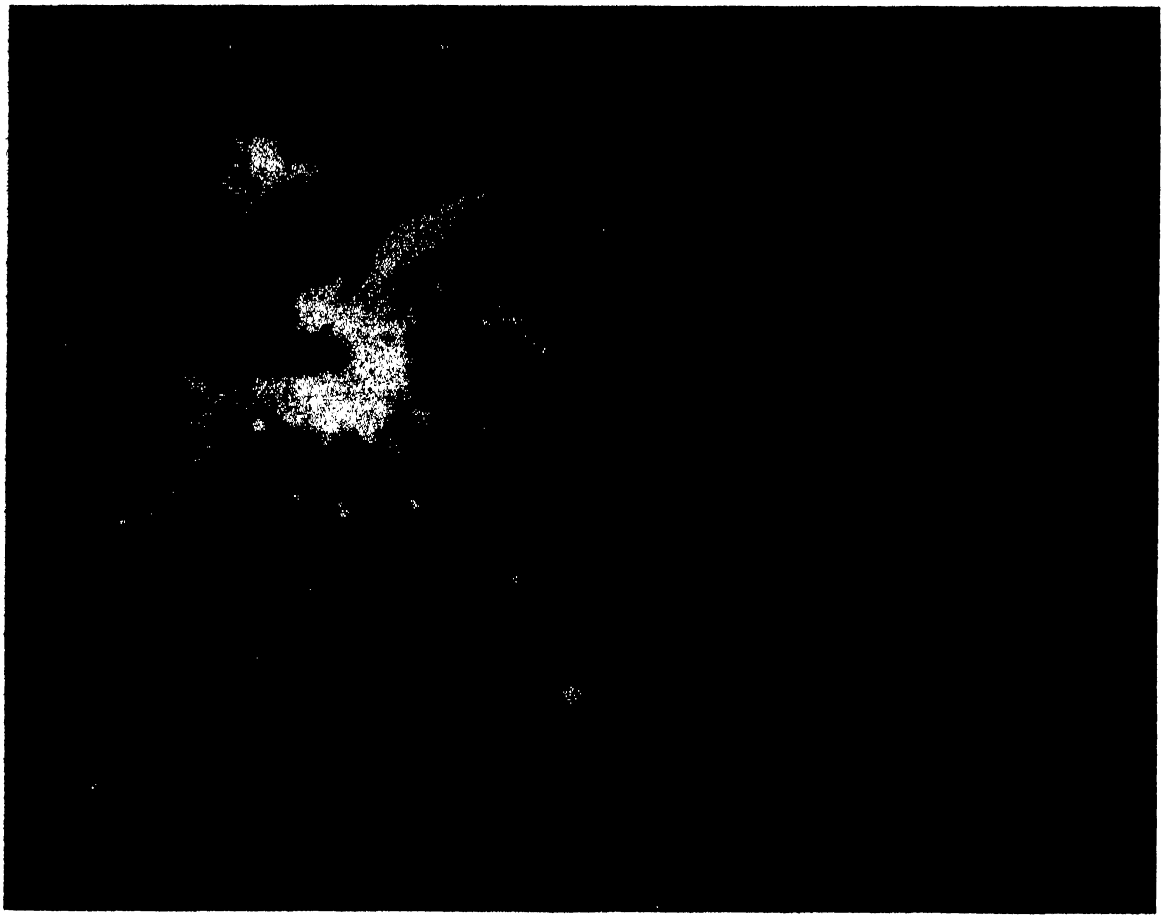
Top: MARCH 15, 1921, WITH GANYMEDE AND SHADOW; *bottom:* MAY 29, 1922. (100-INCH HOOKER REFLECTOR).

The deciding factor was the new high in resolving power reached by the then recently completed 100-inch telescope. This technical term, incidentally, means the measure of a telescope's ability to show as separate individuals two stars that are optically close together. The larger the diameter of a telescope, the greater its resolving power. On a Mount Wilson photograph of one of the larger spirals in area, and therefore presumably a nearer one, some individual stars were resolved in the fringes away from the center. Some of these in turn were Cepheid variables whose distances could be determined. The cat was out of the bag. They were nearly a million light-years away! The Great Nebula in Andromeda differed from the gaseous cloud in Orion as a forest fire differs from the blaze of a match; all by itself it was a vast stellar system like the only one we had known. The island-universe theory was now demonstrated truth, and some hundred million galaxies comparable with our Milky Way stretched outward to the borders of telescopic sight. The universe according to man's understanding had exploded into a new order of size.

Have I then exaggerated the importance of the intellectual revolution of 1924? It does not seem so to me. Remembering that the 200-inch telescope will give us only eight times as much observable space for study as we had before, we should be warned not to take for granted new revelations of the order of those that already have come in our lifetime; and at the same time we should be made somehow to realize that it was the 100-inch telescope that played a crucial part in bringing to pass the modern age of miracles in the field of science. Certainly only one other development of our time is comparable at all with the discovery of 1924. In practical consequences, the release of atomic power is of course much more important; but from the point of view of pure science and of sudden access of knowledge, the two great events are probably of the same order of magnitude.

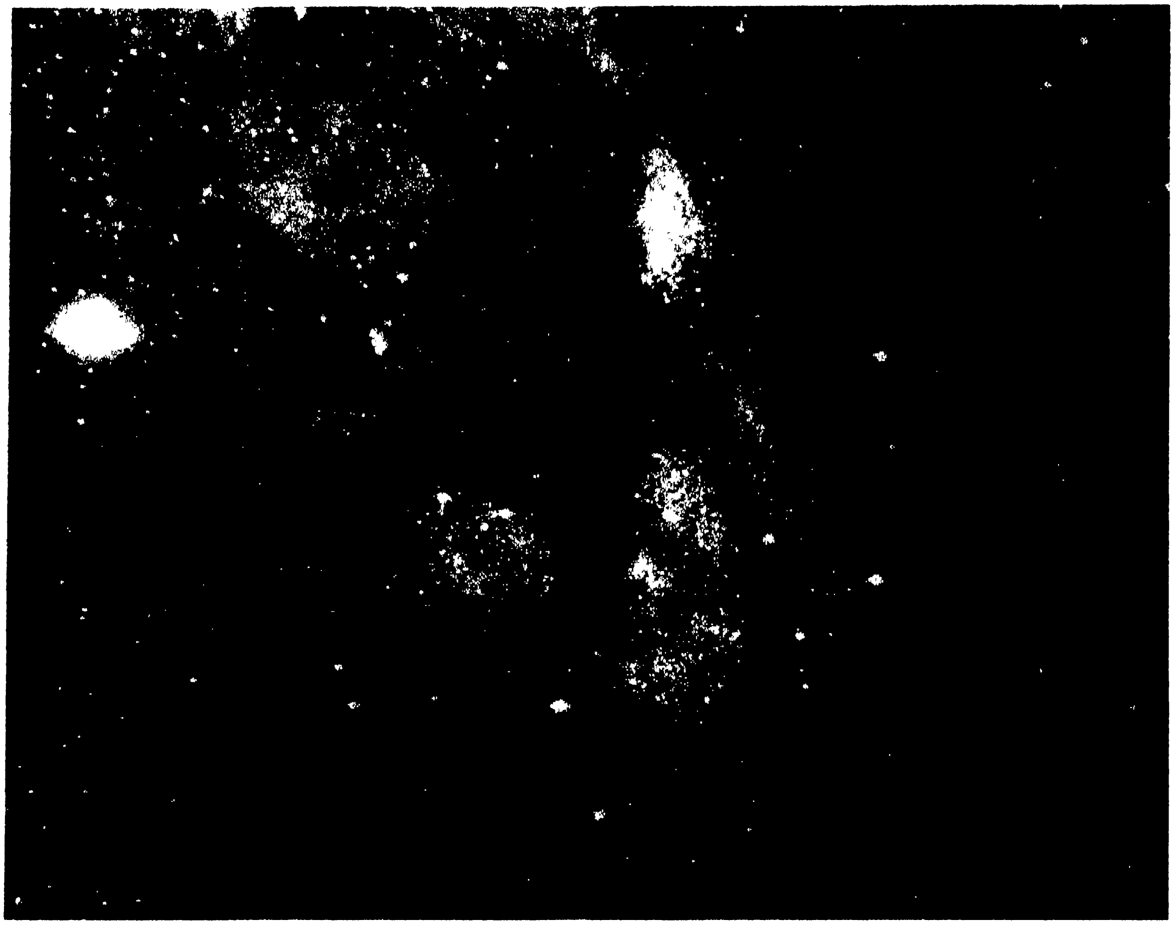
And now let us consider the astronomical picture as of today, as the 200-inch giant moves into the scene. As in the previous description of the astronomer's view in 1916, we shall glance only at the high lights of the scene, now vast beyond all imagining. There are exactly two of these outstanding, all-encompassing features; and there would probably be little argument among the astronomers of the world as to what they are. It is only when we turn to the innumerable details that differences of opinion begin to appear.

Fact No. 1 is that in the approximate sphere of observed space there is uniformity in the large.



THE GREAT NEBULA OF ORION

FEW PICTURES TAKEN ON OUR PLANET CAN RIVAL THIS ONE FOR SHEER MAJESTY. HERE THE DUST AND DEBRIS OF AN EXCITING REGION ARE BATHED IN THE LIGHT OF ENVELOPED SUNS. THOUGH VAST BEYOND ALL IMAGINING (PERHAPS 10 LIGHT-YEARS IN DIAMETER), IT IS AN INSIGNIFICANT DETAIL OF OUR GALAXY.



Mount Wilson Observatory

PART OF THE GREAT NEBULA OF ANDROMEDA

ONE END OF THE FAMOUS NEBULA, WHICH IS A NEAR RIVAL OF OUR OWN GALAXY IN SIZE (THOUGH SMALLER), AS WELL AS A NEAR NEIGHBOR IN DISTANCE. THIS VIEW SHOWS SOME OF THE INDIVIDUAL SUNS WHOSE VARIATIONS IN BRIGHTNESS GAVE AWAY ONE OF THE GREAT SECRETS OF THE AGES. (TWO HOURS' EXPOSURE, 100-INCH TELESCOPE.)

Galaxies are no more and no less numerous near the rim than they are at the approximate center, say, within the small globe containing no points more than fifty million light-years from the sun. But our use of that casual figure instead of, say, five million light-years, is not at all accidental, since the latter choice would have named a sphere containing a local condensation of galaxies. It is as if a giant hand had reached in among the pinwheels of space and pulled them, here and there, into more or less isolated flocks, leaving the larger-scale distribution unchanged. And the uniformity applies not only to the grosser features of mass and structure, but also to chemical composition, as we learn through the magic of the spectroscope. The universe of the 100-inch telescope is one grand unit; and this basic generalization of twentieth-century astronomy should be kept in mind when the new picture begins to unfold.

Fact No. 2 is the mysterious phenomenon variously described as "the red shift" or "the expanding universe." It is concerned with the so-called Doppler effect, which changes the number of vibrations per second that reach us from an approaching or receding body. In the case of sound, this makes the pitch of the whistle blast from an approaching locomotive higher than it is when the train moves away. In the case of light, it causes the lines in the spectra of stars to be shifted toward the red or the violet end according as the source of light is going away from or coming toward the recording spectroscope. And for the visible universe as a whole, the message of this instrument is a most astounding one. Apparently, the farther away a given galaxy is, the faster it is moving outward into space! Or perhaps this is not the case at all. At any rate, the indisputable observed fact is this: the farther away the galaxy, the more the lines in its spectrum are moved toward the red. And if this shift is explained as it is in the case of relatively near stars, for which the interpretation in terms of radial motion is almost certainly correct, it would mean that a fantastic and almost unbelievable situation is revealed by the evidence. It would mean that some two billion years ago—which is to say, practically yesterday as the life spans of suns are reckoned in the best astronomical circles—an explosion took place in the universe that must have surpassed even a Cecil B. de Mille spectacle. The fastest moving clouds, or galaxies, are naturally by now the farthest away. The whole thing is implausible to the *n*th degree, utterly absurd, utterly ridiculous; but there it is.

Here, then, is fact No. 2 about things as a whole; and the fact is the prime mystery of the age. The available explanation in terms of known principles of science leads to the uneasy conclusion that the universe is fading away into endless reaches of nothing at all. I for one am not ready to go along with this thesis. I feel much more friendly to the idea that somewhere in the woodpile of theory dignified by the name of the expanding universe there is a very large and very obnoxious Ethiopian, but of course this is a purely personal reaction. Fortunately, the time is coming when no one will have to rely upon such dubious guides as our own hunches about the fitness of things.

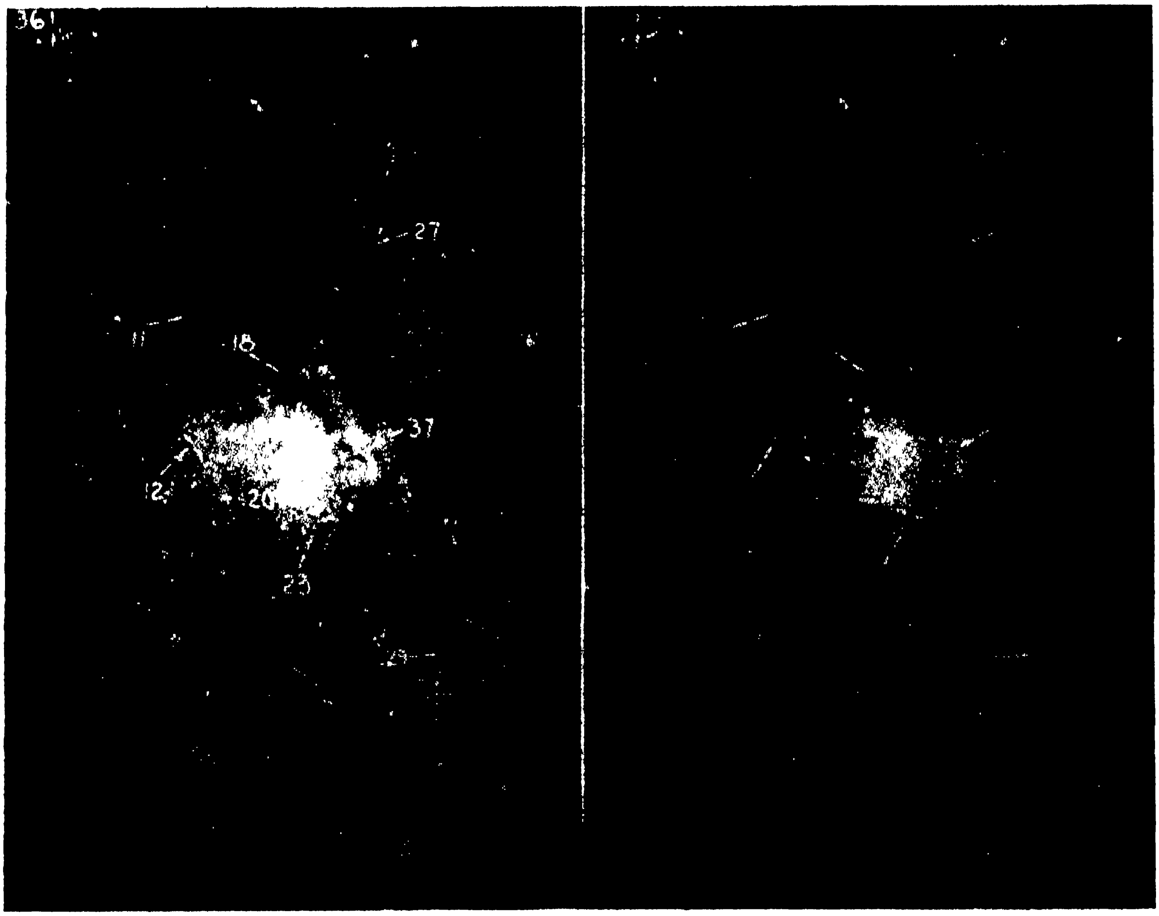
Before leaving the picture of the observed universe as it now presents itself, we should perhaps mention one important but secondary item of information. As of 1916, the grand orbit of the sun, if any, about some more massive body, if such there were, was totally unknown; and I remember that the quest for such information was considered practically a hopeless undertaking. Today, the facts on this point are fairly well established: the orbit (not to be confused with the twelve-miles-per-second local movement of the sun with reference to neighboring stars) is a grand ellipse some seventy thousand light-years across; the massive attracting body is the sum total of suns and matter in the heart of our Milky Way galaxy; our speed in this cosmic Derby is of the order of two hundred miles per second; and the period of revolution is roughly —very roughly—some two hundred million years. Thus has science progressed in some thirty short years! Of course, fact No. 1 is still grand-scale uniformity, and fact No. 2 is apparent outward motion; but this lesser fact has also some large-scale aspects. The spectroscope shows that other galaxies, too, are making giant revolutions. The stars, like our sun, and the planets, like our earth, share this type of motion, as perhaps do the electrons about their protons and neutrons, and some still smaller unknowns about the nuclei of unguessed subatoms. Truly, on any scale, we live in a whirling universe.

WITH this background in mind, it should not be hard to see what are the primary questions that the new telescope will answer, or at least cast new light upon. They have to do chiefly with the nature of the enlarged universe as a whole, since the task for which the big mirror is best fitted is not that of magnifying objects such as near-by planets. Actually, its main job will be that of gathering light from suns and galaxies too far away, or at least too faint, to be studied well with other instruments.

With regard to the first observed fact that the universe is one spread-out unit, the basic question is simply: "Is this a true sample?" If the answer is "No," the telescope may settle the matter; if "Yes," the best it can say is "Maybe," or "So far, it appears to be." In any case, the answer is likely to come slowly, based as it will have to be, upon patient, statistical investigation. The answer it gives, moreover, is tied up with the second main fact concerning present astronomical knowledge, with its inevitable accompanying question: "Is the universe really expanding?"

Here we are on somewhat more optimistic grounds, at least with regard to the likelihood of a positive answer. Indeed, Professor E. P. Hubble, whom many will regard as the leading authority in this field, has reached such heights of confidence that he has, so to speak, put himself out on a scientific limb. He predicts rather flatly that the new telescope will give a definite answer. By that he means "ultimately," of course—not necessarily in 1948. For, whether the cause of the red shift be actually a flying away of the galaxies—and "flying" is an understatement—or a change in the quality of light due to "travel-fatigue," the added reach of the new telescope should supply enough data to provide the answer. In this connection, Dr. F. R. Moulton suggested long ago that according to Planck's equation, $E = hc/\lambda$, in which h is Planck's inviolate constant, c the fixed velocity of light, λ the wave length, and E the radiant energy, if there is even the slightest toll charge on E for an eon-long trip through the rare and minute obstacles of intergalactic space, it must be paid by means of an increase in the wave length λ. If so, all this fuss about an exploding universe may, after all, be quite unnecessary.

As to what will be learned in the investigation, there is still another possibility, commonly described somewhat as follows: Space may be "curved," and the new telescope may reach far enough toward the limits of the bounded universe to make the curvature apparent. In that case, the curvature would be "positive," as suggested by the surface of a sphere; in an alternate situation, it might be "negative," like the face of a saddle. But whether such attempts to "explain the unexplainable" by the use of vaguely suggestive and possibly misleading analogies actually do skirt the fringes of reality and point the way to some deep and obscure truth, we shall leave to those who wish to grapple with such problems.



TWO EXPOSURES OF THE SPIRAL TRIANGULUM

A TYPICAL SPIRAL. THE BARS INDICATE VARIABLE STARS. NOTE, FOR EXAMPLE, THE STAR MARKED "17," WHICH IS MISSING IN THE LEFT PHOTOGRAPH, BUT FLASHES OUT IN THE RIGHT ONE. MOUNT WILSON OBSERVATORY PHOTOGRAPH.

With full knowledge, then, that we are omitting some aspects, both mystic and mathematical, of the idea of "curved space," we shall consider one relatively simple related thought. This thought is that a ray of light bends somewhat, just as does the path of a thrown stone, when it passes a massive object. Einstein predicted this, and the evidence seems to bear him out. If, then, the amount of matter in the universe is finite, a ray of light will not go on forever in a straight line, but will describe a giant curve concave to the bulk of the galaxies. If that be true, then we can never hope to see anything outside the vast but limited pile of stuff that we are pleased to call "the universe," though for all we know there are other piles of stuff, with other self-enclosed light systems, in other remote places. In any case, it would seem more honest, or at least less misleading, to describe this possibly limited realm that man can see as "man's visible domain" rather than "the bounded universe."

And here we get into the semantic anarchy that delights the hearts of some popular expositors of science. A "straight line" is after all fundamentally a mental concept, difficult to define but easy to understand in its ordinary connotation. Since a definite concept of its meaning is shared by intelligent laymen as well as by mathematicians, it seems that a straight line should be entitled to remain straight in the interests of mutual understanding and priority of definition. This means that the two far ends should never meet, whatever the universe may be like. But what happens? Instead of making the simple statement that a ray of light does not move in a straight line—an assertion which, true or false, would be easily understood by those who remember their Euclid—some thinkers in this field prefer to *define* a straight line as the path of a light ray, come what may. Thereby they are led, logically enough, to such subtleties as "curved" and even "non-Euclidean" space. Their straight lines run straight in a bending "space"—and another good word which was formerly accepted as meaning something at least vaguely comprehensible goes into Websterian hysterics. But of course all this is chiefly a quarrel with words rather than ideas, and should not be taken too seriously. Speaking for myself alone, I have always felt that technicians should invent technical words for technical uses, and not confiscate and revamp the old standbys.

Having disposed of the less reliable personal reactions which were predicted in the introduction, we return to the 200-inch telescope and point out that one of its major accomplishments, totally uninfluenced by advance human surmises, may be to verify the truth of the statement that man's *access* to the universe is limited, or, on the other hand, to indicate that unreached space is much like the sample at hand, and that actually the man-sensed universe may stretch outward without end.

Coming down out of the ether of speculation about the facts of cosmogony and the shape of the universe, we can say with certainty that the primary service of the 200-inch telescope will be that of funneling light, in larger quantities than formerly, into suitable apparatus for detailed study. The receiver may be a photographic plate to record magnified features of a galaxy or the distribution of a star cluster, a bolometer or thermocouple to measure the temperature of a light source, or perhaps a photometer to calibrate brightness. Finally, of course, the receiver may be the almost miraculous spectroscope, from which one can learn, among other things, the chemical composition of the source of light, its motion, mass, magnetic condition, cosmic age, or what you will. In fact, in the light of past accomplishments, it seems a bit rash to deny the possibility that the spectroscope will tell us, ultimately, the innermost thoughts of some hypothetical dwellers on unseen planets circling a middling sun in one of the remoter galaxies. At least we shall surely get much more information about many things, both far and near. Among the likelier prospects are the dwarf suns in our own celestial neighborhood—those worn-out dispensers of faint light that are assuming a role of increasing importance in modern astrophysical studies.

Finally, in spite of the fact that the new tool of research is not designed primarily for investigations within the solar system, it does not follow that the telescope will never be used for this secondary purpose. In fact, some plans have already been made to use its great light-gathering power for the purpose of reducing the time of exposure for photographs of Mars. It is believed that in a long series of repeated flash shots there are likely to be some lucky ones, taken in rare moments of atmospheric calm, which will settle once for all some long-debated questions, such as those concerning the straightness and artificial character of the canals on Mars. It is not improbable that some of the more spectacular of the achievements of the new telescope, at least from the standpoint of popular interest, will deal with matters in our own tight little solar family.

In summary, then, the 200-inch giant of Palomar Mountain will give us more data on the question of whether or not the universe around us, with its millions of galaxies lighting the void as far as we can see, is a fair sample of a larger reality. It will probably enable us to decide definitely whether or not this welter of cosmic pinwheels is actually fanning out, as it seems to be, in a vast pyrotechnic explosion. It *may* tell us whether the borders of our enlarged domain are drawing near to the limits of human reach; and it surely *will* throw much more light on the chemistry of all things, far and near. Our knowledge of the solar system will probably be enhanced, and we may get some important information on the problem, or at least the distribution, of life itself.

It is perhaps too much to expect that this new masterpiece of engineering will bring about an intellectual readjustment matching in spectacular quality that which was sparked by the Mount Wilson giant; but even that may happen. After all, it seems wise not to discount too much the potentialities of the atomic age.

NAMING THE GARDENIA

MARGARET DENNY

Assistant professor of English at the University of Rochester, Miss Denny received her A.B. and M.A. from Smith College, where she has also taught. Her article on "The Royal Society and American Scholars" appeared in the November 1947 issue of THE SCIENTIFIC MONTHLY.

NOT content to be merely armchair scientists, many "curious" men of the Enlightenment—lawyers, merchants, country gentlemen, ministers—felt such enthusiasm for the methods and objectives of science that they wished to take an active part in the scientific work being done at that time. Their cooperation was welcomed both by the Royal Society of London and by master scientists who had need of many helpers. Two hundred years ago the word *virtuosi* was used to describe such men. Those amateurs who were able successfully to apply the Linnaean principles of classification were encouraged by the great Swedish botanist to send to Upsala accounts of all flora and fauna believed to be "nondescript." Alexander Garden, a physician of Charleston, South Carolina, was numbered among the so-called "sons of Linnaeus." In 1760 Linnaeus named a certain genus of plants *Gardenia* in his honor, and in later years Garden sent his master so many specimens of various kinds that Linnaeus called him "the happy illustrator of Nature in his own region of Carolina."

In the naming of the gardenia, the behavior of the most renowned of all botanists was both informal and charming. The central character in the story is, of course, Dr. Garden; but another virtuoso, John Ellis, who was a London merchant and the friend of both Linnaeus and Garden, has an equally important role. The story also concerns four others: the daughter of Cadwallader Colden of New York, who shared her father's botanical interests; Mr. Richard Warner, whose garden contained an "elegant plant;" the superintendent at Chelsea Garden, who was the author of a botanical dictionary; and Mr. James Gordon, a famous London gardener. The Duchess of Portland is a mute actress in the drama of the gardenia; one element of the plot is the sum of five hundred pounds.

Such a story is worth telling for its own sake, but it also deserves attention because it explains why the eighteenth-century virtuosi found botany such an absorbing avocation; it reveals the satisfactions to be derived from association with the great Linnaeus and with fellow-amateurs.

Nothing in the story of the gardenia is more interesting than to observe the intimate place science had in the private lives of all the people concerned. Factors in human personality were constantly affecting their scientific work and attitude. To these virtuosi, even to Linnaeus himself, the pleasures of science and of friendship were identical.

Various motives prompted the virtuosi to aid in the progress of science. One important spur was the popular eighteenth-century belief that science strengthened religious faith and induced a growth in religious sentiment. Another was the social concept of science. Like Sir Francis Bacon, these men believed that the end of science was "the relief of man's estate." The actors in the story of the gardenia shared these beliefs, but apparently such assumptions were too familiar to warrant their comment upon them.

Certainly human pride had some part in causing the sons of Linnaeus to search for new plant genera. It was his practice to name such plants in honor of the promoters and benefactors of botany. No little fame was enjoyed by an eighteenth-century man whose "name-sake" was listed in the *Systema Naturae*. The virtuoso who was known to be the correspondent, as well as co-worker, of the master of Upsala was the object of considerable envy.

Linnaeus very wisely permitted the discoverer of a new genus to suggest the name he wished given his plant, and when possible that wish was honored. The amateurs could use this opportunity to compliment a fellow-botanist, cement a friendship, find a new way to pay old debts.

The association of the London disciples of Linnaeus differed somewhat from the conventional club life so popular during that period, but it furnished similar social pleasures because there was an opportunity at public and private meetings to discuss moot points in botany and a chance for an exchange of visits to the gardens of fellow-amateurs. For the botanical virtuoso who lived in America, however, the only substitute for such "instructive conversation" was correspondence.

After the completion of his education in Scotland, Garden returned to Charleston in the early fifties and was dismayed to observe "there is scarce one here that knows a cabbage-stock from a common dock, but, when dressed in his plate, by his palate." When in 1754 Garden became a Linnaean "convert," he felt keenly the need of counsel and encouragement in his new studies. As quickly as possible he established correspondence with kindred spirits in the other colonies and across the seas.

Garden's correspondents must have felt at once the energy and enthusiasm of the man. He exhibited a true flair for applying Linnaean principles, and his attitude toward botanical study was one of high seriousness. He showed himself master of the cooperative method. Obviously, such a man was deserving of compliment by his fellows and of public recognition as a worthy disciple of Linnaeus. It is not surprising, therefore, that this story will be concerned not with one "gardenia" but with many.

Ellis' first gardenia. Garden began his correspondence with John Ellis in 1754. There followed the characteristic give-and-take of a friendship between virtuosi. Garden sent American seeds to Ellis, which were then planted in his garden or distributed among Londoners of his acquaintance. Garden cooperated in Ellis' experiments with improved methods of shipping seeds; he forwarded shipments of plants addressed to Ellis from Florida. Ellis in turn transmitted books and botanical gossip to America, and as a Fellow of the Royal Society he was able to publish in *Philosophical Transactions* descriptions of new plants Garden had discovered. Then, too, after Garden began writing to Linnaeus, Ellis forwarded the letters and specimens from America bound for Sweden.

In 1756 Garden sent Ellis descriptions of six of Carolina's plants. These both Ellis and Garden were anxious to have appear in print because publication usually prevented other men from laying claim to the plant or its name. Ellis, however, wanted to make doubly sure about Garden's plants, and he therefore sought the advice of Linnaeus. "I am very sensible," Ellis wrote in the letter that began his long correspondence with Linnaeus, "your judgment in these matters far exceeds either the english botanists or those of any other nation." Linnaeus gave one of the American plants "the bishop's touch," as it were, and together Ellis and Garden named the genus *Halesia* in honor of the man whose name Garden had used when first addressing himself to Ellis.

Among Garden's six genera was a plant commonly called "Buereria;" but because Ellis knew it carried other names as well, he apparently posed some questions to Linnaeus about this plant. The reply of Linnaeus is not extant, but in May 1757 Ellis wrote:

> You desire my advice in the affair of the Butneria and Buereria. What you say is very true. Call it which you will you will certainly give offense to one or another. Mr. Miller has called it Basteria. But if you will please to follow my advice, I would call it Gardenia, from our worthy friend Dr. Alexander Garden of S. Carolina, who will take it as a compliment from you, and may be a most useful correspondent to you, in sending you many new undescribed plants.

Ellis was not to learn until the appearance of the tenth edition of the *Systema* in 1759 that Linnaeus had not taken his advice.

Miss Colden's gardenia. Garden first met Miss Jane Colden in 1754 when he visited the country estate of Cadwallader Colden, of New York. He sent Ellis a full account of his trip to Coldengham. "Not only the doctor himself is a great botanist," wrote Garden, "but his lovely daughter is greatly master of the Linnaen [sic] method and cultivates it with great assiduity." Her admirers were numbered among the virtuosi overseas also. In 1758 Ellis sent Linnaeus one of Miss Colden's plants and wrote:

> This young lady merits your esteem, and does honour to your System. She has drawn and described 400 plants in your method only: she uses English terms. Her father has a plant called after him Coldenia, suppose you should call this Coldenella or any other name that might distinguish her among your Genera.

Peter Collinson's comment to Linnaeus made in the same year is well known. "As this accomplished young lady is the only one of the fair sex that I have heard of, who is scientifically skilful in the Linnaean system," he said, "you, no doubt, will . . . recommend her example to the ladies of every country."

If Miss Colden provoked such flattering sentiments from those who had never met her, it is easy to understand why she deeply impressed the young naturalist and bachelor from Carolina who was visiting at her home. Garden's letters—all addressed quite properly to Cadwallader Colden—came thick and fast in the months immediately following his departure. In January he reported that he had written from Philadelphia on his way south and had sent another letter a few days after his arrival home. In February he sent Miss Colden some Persian seeds "got from Mr. Mounsey chief

Physician to the Army & Physician to the Prince Royal of Russia." "I shall in my next," he promised, "mention to Miss Colden the method of preserving Butterflies Ecce."

Clouds appeared on the horizon in May, however. Garden wrote to Colden:

> By your second letter I find that I have very innocently offended both you & Miss Colden in some expressions that insensibly dropt from my pen as archetypes of what my heart dictated in was on sincerity. This gives me real concern & give me leave to assure you I shall endeavour as far as in my power to amend any thing in my conduct or manner of writing you are kind enough to point out as wrong. I trust that both you & your daughter will forgive me for once, I shall be more sparing in saying what I think is due to such merit for the future - The expression which you say gave her most offence, gives me now a great deal of uneasiness as I suspect it has deprived me of the pleasure of a letter from her by last opportunity.

The precise nature of Garden's unseemly behavior can only be surmised. Perhaps Garden misinterpreted the temper of Miss Colden when she expressed the desire to give his name to one of her new plants. It is fortunate that Colden granted Garden permission to tell this news to his correspondents, because Garden had immediately written to a friend in Edinburgh about the plant and its proposed name. Early in the following year Garden told Ellis: "By a letter from Dr. Whytt, I learned that they had published the characters of a plant which I had sent me by Miss Colden, which she had called Gardenia."

The episode closes at this point. Linnaeus never adopted one of Miss Colden's genera under this name, and Garden's marriage to a Miss Peronneau of Charleston took place on Christmas eve, 1755.

The gardenia of Alexander Garden. Linnaeus was forced in 1758 to reject a plant sent him from Carolina, but he wrote to Ellis at that time, "If Dr. Garden will send me a new genus, I shall be truly happy to name it after him, Gardenia." Naturally, Garden wished to take advantage of this flattering offer. Within a few months he submitted specimens and description of a "very pretty shrub" he had come upon three years before and he expressed the hope that the plant would find "its proper place in your Systema Naturae." A few weeks later he wrote to Ellis:

> I have sent him a new genus, which he desired I would, that he might name it after me, but that I have left to himself, as I am very sensible I am yet rather too much of a Tyro to have that honour conferred on me.

Linnaeus offered no comment on this plant to either Garden or Ellis, and as late as the end of 1762 neither virtuoso was aware that it had already been described and named.

Ellis' second gardenia. Early in 1758 Ellis became interested in a plant called the "Cape jasmine," which he found growing in the garden of Mr. James Gordon at Mile End. He heard that the plant had been brought from the Cape of Good Hope four years before, but its owner, Mr. Warner, had been unable to cultivate it. Although the other London gardeners had failed in their attempts, Gordon successfully raised the plant from cuttings. It was classified as a jasmine by Mr. Philip Miller, superintendent of Chelsea Garden and author of a botanical dictionary. Ellis, however, believed the plant was new, nor was he impressed by the authority of Miller.

One day Ellis, accompanied by Peter Collinson and Georg Ehret (famous for his drawings in Linnaeus' *Hortus Cliffortianus*), went to "Mr. Warner's, a very curious gentleman, at Woodford near this City, to see his rare plant like a Jasmine, with a large double white flower, very odoriferous." The men induced Warner to let a dried specimen be sent to Linnaeus. Then Ellis, in the presence of Linnaeus' friend Dr. Bierken, dissected the double flower and sent the description to Sweden. "If you find this plant to be no jasmine, but an undescribed genus," Ellis told Linnaeus, "you will oblige me in calling it Warneria after its worthy possessor."

Linnaeus was certain it was no jasmine, but he was loath to offer further judgment as long as he could examine only the double flower. "May I be allowed," he asked Ellis, "to insert the character of this genus, on your authority, in my Systema, as I am unable to make it out myself?" On further consideration, however, Linnaeus decided he would "rather not meddle with this plant at all, till it is better known."

Then came the Christmas holidays, and something happened to Linnaeus that will strike a familiar chord in the heart of many a college professor.

> I had always present in my collection a dried specimen, with a single flower, preserved in some part of my herbarium, which, as far as my memory would serve, I believed to be the same species. Many a time, in the course of the last half year, have I hunted for this specimen, but in vain, my thoughts having been so much taken up with the daily business of the University, my publick and private lectures, and numerous matters besides.
>
> During the Christmas holiday, however, having found leisure to turn over all my dried plants, I luckily met with the specimen in question. The flower is perfectly single, and by immersing it in hot water, I could clearly ascertain every part of its structure, so as to draw up, without any uncertainty, the following characters of your genus Warneria.

To disentangle one snarl, however, was but to encounter another. Suddenly the plant was without a name because the "very curious gentleman" at Woodford had announced that he did not wish the plant to be called "Warneria." Ellis wrote to Linnaeus:

> I believe he is convinced that it differs from the Jasmine; but he has such an esteem for Mr. Miller, that he would not appear to differ from him in so capital a plant by adopting another name.

It is not strange that today we cannot understand why Warner made such a decision; the years that have elapsed since that time have brought about such a radical change in popular scientific thought that even the eighteenth-century meaning of the word *virtuoso* has become obsolete.

Warner was certain to gain in reputation among his fellows if Linnaeus named the genus "Warneria" and published the news in his *Systema,* nor was Warner loath to accept this signal honor that only Linnaeus could confer. But, as the personal friend of Philip Miller, Warner had concern for Miller's reputation among Londoners as a botanical authority. The linking of Warner's name with the new plant genus was certain not only to weaken Miller's prestige, but also to give public announcement of Warner's disagreement with his good friend. Warner was confronted with the hard choice between science and friendship. His regard for the feelings and reputation of his friend triumphed over his respect and recognition of scientific fact. This episode very clearly reveals the important role that science sometimes played in the private lives of the eighteenth-century virtuosi.

Hindered from naming the Cape jasmine after the man who was the proper recipient of the honor, Ellis began searching for a suitable name, one worthy of so handsome a plant. He considered it, he said, "the most rare and beautiful shrub, that has yet been introduced into the European gardens;" some of its double milk-white flowers had a width of four inches; it bid fair to reach a height of six or seven feet. Surely "Augusta" perfectly described such a plant. Ellis wrote to Linnaeus:

> I must therefore desire you would call this plant Augusta, which I think as well deserves that title for its elegance in every respect, as the Methonica does to be called Gloriosa. This will not offend our friend Warner's modesty, nor his particular delicacy to Mr. Miller.

Three times Ellis urged the adoption of this name; then he suggested an alternative:

> Be so kind as to call it Portlandia, after that eminent patroness of botany and natural history the Duchess of Portland, who is a very great admirer of your excellent and learned works, by which you have opened the eyes and understandings of mankind.

Which name Linnaeus preferred Ellis had no way of knowing, for the master made no comment.

In 1759 the second volume of the *Systema* appeared, but it contained no reference to Ellis' plant. The feelings of the virtuoso were deeply injured. He made no attempt to conceal his disappointment from his friends in London, nor did he address any letters to Linnaeus between March 1759 and June of the following year.

Ellis harbored no resentment against Linnaeus, however, for failing to name his jasmine after the Duchess of Portland. The alternative name had reached Linnaeus too late. In the tenth edition, which had just come to hand, Ellis observed that the *Portlandia* was listed among the new genera.

At length news of his sulking disciple reached the master at Upsala, and he wrote:

> [Professor Ferner] informs me that you are displeased at my not having admitted your new genus, by the name of Augusta, in the second volume of my Systema. Allow me to state my reasons.
>
> . . . I have laid down a rule in my Critica and Philosophia, that no adjective should be admitted as a generic name. On this ground I have expunged several names of other authors; but, that I might not carry innovation too far, I admitted Mirabilis and Gloriosa, for which I have often been blamed by my adversaries. Everyone knows that the Harlem florists give this kind of names to their Hyacinths, Tulips, &c. such as superba, augusta, incomparabilis, pulcherrima.

Ellis' recovery from his fit of the vapors was almost instantaneous. "What you say is right," he answered. Indeed, Ellis might almost feel that he owed a debt of gratitude to Linnaeus. Had not the master saved Ellis' "elegant plant" from the stigma of the market place?

Because Linnaeus had urged him to submit a name so that a description of his plant could appear in the appendix to the forthcoming Volume III of the *Systema,* Ellis was forced to act without delay. A glance at the second volume informed him that Linnaeus had ignored the suggestion, made in 1757, to honor Garden with the plant often called "Buereria." Instead, Linnaeus assigned the name *Calycanthus* to this new genus.

Ellis was not slow in making his decision. "I shall write to Dr. Garden this day," he announced to Linnaeus, "that I have desired you to give the name of Gardenia to the Jasmine, which I am persuaded he will esteem as a favour." And observe Garden's reply: "Your compliment of the Gardenia was most acceptable to me . . . I shall gratefully remember it. Has Linnaeus adopted it?"

Linnaeus' gardenia. There was the rub. Linnaeus was showing himself most reluctant to adopt the name suggested by Ellis. He warned his disciple that if this request was granted his carping critics might renew their attack.

> If I may without reserve lay open my mind to you, I would have wished that the supposed Jasmine might have been called Warneria, after the person who has first cultivated it in Europe; Gardenia being applied to some genus first discovered by Dr. Garden. I wish to guard against the ill-natured objections, often made against me, that I name plants after my friends. . . . If therefore I confer this honour on those who have discovered the respective plants, no objections can arise, nor can I be charged with infringing my own rules.

This time, however, Ellis did not reply as he previously had done: "What you say is right . . . and I therefore submit." On the contrary, he maintained a strict silence.

Linnaeus wrote a second letter to London, and from it Ellis could infer that his request seriously interfered with Linnaeus' own plans to honor his disciple in Carolina and at the same time oblige his good friend in London. "I had given the name of Gardenia," wrote Linnaeus, "to an entirely new and very singular genus . . . in order so far to conform to your wishes."

Nevertheless, Linnaeus must have concluded that the bonds of friendship were stronger than the consistencies of scientific method, or even a master scientist's prerogative, for on November 4, 1760, he wrote to Ellis:

> As you still persist in your decision, that the Jasmine so often mentioned between us should be called Gardenia, I will comply, though I cannot but foresee that this measure will be exposed to much censure I find it impossible to deny you anything. All that I beg of you, my dear friend, is, that you would publish the genus and its character in some loose sheet, or some periodical work, or transactions; in which case I promise to adopt the name. . . . Do not therefore indulge any more suspicions of my regard and devotion to you, who esteem you among the chief of my friends.

Ellis forthwith composed his description and obtained Ehret's handsome drawings; both appeared in the *Philosophical Transactions* of that same year.

At long last Alexander Garden had been presented with a "name-sake," and his fame was assured when the *Gardenia* took its proper place in the *Species Plantarum* of 1762. Thus the story of the gardenia explains why only Ellis' Cape jasmine had the honor of inclusion in the twelfth edition of the *Systema* (1767) even though five "gardenias" were entered in competition for that prize.

CERTAINLY if an eighteenth-century man asked, "What's in a name?" he would get no dusty answer from the botanical virtuosi. The story of the gardenia indicates that the giving of names to plants was a far-from-simple task. The popular identification of the plant with the man it honored caused the amateur scientists of the period to attribute a crucial social importance to the naming of plant genera.

One of the six plants that Garden sent to Ellis soon after their correspondence began was the "Carolina Yellow Jessamy." Although Linnaeus had already classified this plant, Garden believed it to be "absolutely a new genus." He wished to make his new friend a gift of the plant by naming it "Ellisiana." Apparently, Ellis advised against challenging the authority of Linnaeus, and so in a subsequent letter Garden wrote:

> I now have a plant which is entirely new, and the most superb lofty plant that ever I met with in America, which I shall beg leave of you to accept as a name-sake . . . but if you should chuse a shrub, I have that beautiful one which I call the hop-like shrub.

Once correspondence with Linnaeus was established, Garden renewed his efforts to compliment the man to whom he once referred as "my dear, my first, my chief Botanical friend." In 1758 Garden submitted the characters of "a very handsome plant," which, he told Linnaeus, "I am very anxious should bear the name of my much valued friend Mr. Ellis." Linnaeus rejected the plant, and he further dashed Garden's hopes by stating that Ellis already had been presented with a plant. In a grateful but saddened mood Ellis wrote to Garden, "Indeed your compliment of the Ellisia (now a Swertia) I can never forget. You see Linnaeus says I must be contented with one place, which Dr. Browne has given me."

In 1762, however, Linnaeus sent Ellis startling news concerning his plant:

> I . . . am obliged to confess that Duranta and Ellisia not only form one genus, but scarcely differ in species, so that the latter cannot be kept distinct. This being the case, I began to look about for a new Ellisia, that you, who deserve so eminently of our science may not be forgotten. Being particularly desirous to fix on some plant known in the gardens of Europe, I have thought of . . . Ipomaea Nyctelea. . . . Write to me as soon as possible, whether this meets your wishes, or whether you would prefer any thing else.

Ellis did indeed prefer something else. He knew Linnaeus' plant well; he had even made a drawing of it for his friend Peter Collinson. What Ellis desired for his "name-sake" was the plant that Garden had sent Linnaeus in 1760. The Charleston physician had anticipated it would be named after him, but its arrival coincided with the moment when Linnaeus had agreed to have Ellis' Cape jasmine called a gardenia. Ellis had suggested at that time that the plant be named "Schlosseria" in honor of the Dutch botanist.

As soon as possible Ellis wrote Linnaeus, reminding him of the plant "with white flowers" that Garden had sent to Sweden:

> If it is not too late, and you find it a distinct genus, I would rather choose to change plants with Dr. Schlosser. If this is not convenient, and you have any new genus of a specious plant, which will grow in England, you will do me much honour; because I may communicate it to the gardens of my friends here, to put them in mind of me.

Ellis had no wish that his friends should identify him with the plant which Linnaeus had selected:

> You will pardon me when I tell you that people here look on a little mean-looking plant as reflecting no honour on the person whose name is given to it, though I am convinced, as it is a distinct genus, the compliment is equally great with the greatest tree.

Ellis might reason as a scientist, but he shared the feelings of the virtuosi.

There is no evidence that Linnaeus was impatient with the unscientific sentiment expressed by Ellis; perhaps he was aware that his amateurs attached social as well as botanical importance to the naming of plant genera. I am inclined to believe that Linnaeus would have given Ellis his "very pretty shrub" if it had not already been named. Ultimately, Ellis was willing to accept with good grace Linnaeus' gift of the "mean-looking plant" with its tiny blossoms and to suffer in silence the inevitable invidious comparisons.

Consider, in contrast, Alexander Garden's pride and gratification in the plant that his London friend and his master at Upsala had bestowed upon him. It had all the virtues: first of all, it was large; its flowers were huge and showy; it was blessed with perpetual verdure; it had a "refreshing aromatic smell." News reached Garden from Ellis that gardenias were in such popular demand in London that James Gordon could charge five guineas for each plant. The gardener at Mile End enjoyed a local fame, and over a period of three years he became the richer by five hundred pounds.

Garden must have been aware that he was a very lucky man. What but luck could explain why Linnaeus did not give Garden's name to any of the other proposed plants? Why had the crucial date of 1760 passed before Linnaeus began adopting the new plants Garden sent to him? There was precedent for giving the so-called jasmine such a name as "Augusta," but fortunately Linnaeus had refused Ellis' request; had Ellis submitted the alternative name only a short time earlier, the plant might have been a compliment to the Duchess of Portland. To all these happy coincidences must be added Garden's extraordinary good luck that circumstances prevented the plant from receiving its proper name of "Warneria."

I doubt if the hapless Mr. Warner was considered either misguided or foolish by the virtuosi of his acquaintance. That was a sentimental age, and Warner was indeed a man of sentiment. Out of consideration for the tender feelings of his friend he forfeited the fame that this large and handsome plant could bring him. As time passed and the popularity of the gardenia increased, his fellows might well have viewed Warner as almost a pathetic hero. The attitude of Philip Miller would also be comprehensible to the virtuosi. Is it likely that Miller's *amour-propre* would have suffered any serious injury if Warner had publicly challenged his friend's faulty judgment about a "little mean-looking plant"?

Meanwhile Garden's personal triumph was real and sweet. His name was well publicized in *Philosophical Transactions*; his plant was assured a life beyond life in the *Systema*. A letter from London in 1761 gave the impression that the gardenia was the talk of the town. Ellis exclaimed: "Every body is in love with it . . . It has given great jealousy to our botanists here, that I have preferred you to them; but I laugh at them and know I am right . . ."

So secure was the fame of the gardenia in 1763 that Garden could afford to jest over the "sudden death" of the plant growing in his garden at Charleston. "I take it to be no good omen," he said, "for the continuance and duration of my botanical name and character." Then he added, "But if I do not outlive it, I shall be pleased."

Even beyond his lifetime Garden continued to be fortune's favorite. The plant that now bore his name was called Cape jasmine by Garden's contemporaries, and it is natural that the public did not discontinue at once the use of a name so attractive as this. In time, however, after the new name had replaced the old, the gardenia and the man whom it honored became forever linked in the minds of men. It is a pity Garden did not have the pleasure of knowing that almost two hundred years later it could still be said of the gardenia, "Everybody is in love with it."

ARE OUR WARS GOOD TIMES?

WILLIAM F. OGBURN and JEAN L. ADAMS

Dr. Ogburn (Ph.D., Columbia, 1912) is Distinguished Service Professor, Department of Sociology, University of Chicago, and was formerly chairman of the Department. He was Director of Research on President Hoover's Committee on Recent Social Trends, which made a comprehensive survey of social life in America. The author of numerous books, his most recent is The Social Effects of Aviation. *Miss Adams is research assistant to Dr. Ogburn on the project "Social Effects of Technology." This article is the first of a series on the social sciences.*

THE effect of war is generally considered to be destruction. Certainly, such it is in battle and in a country that is bombed or invaded. Otherwise, the destruction is limited to materials and lives lost outside the home country. But there are also constructive effects of war, as will be noted in this article.

First, we observe that the consequences of war are much more than loss of life and property. Industry, agriculture, government, and schools are affected, for illustration. In modern wars there is hardly a social institution that escapes its influence, and this influence is not always bad. The purpose of this article, however, is not to be so comprehensive as to survey the influence of war on the various social institutions. We wish rather to compare the conditions of recent wars in the United States with the conditions of the business cycle in peacetime.

For such comparison, we shall be restricted to statistical records, such as those of production, unemployment, divorces, and births. By observing the movement of curves from such annual statistical records in peace and in war years, we shall make some deductions regarding the influence of war. For instance, the annual number of failures of industrial and commercial firms per 10,000 firms in the 1920s and 1930s was 90. In the war year 1944, the number was only 7 per 10,000 firms. We therefore conclude that one influence of the war upon business in the United States was to reduce greatly the number of business failures.

War years not easily discernible in some records. Not all social conditions are so markedly affected by war as are business failures. The hours worked per week in wartime, for instance, are not greatly different from the number worked during peace. In the war years of 1942–44 the average number of hours worked per week in the manufacturing industries in the United States was 44; in the depression years of the 1930s the average was 38 hours. But in the prosperous years of the 1920s, from 1923 to 1929, the weekly working time in factories was 44.6 hours, or slightly more than in World War II. Whatever the influence of the war on hours worked per week, it was not profound.

For some statistical records during peace and war years it is difficult to tell from the curves alone which section of the curve is in the war period. This difficulty is illustrated by Figure 1, which sets forth undated curves of six annual statistical series. Each of these curves is for a twenty-year period, but not for the same twenty years. Each curve, however, does cover the period of either World War I or World War II. Looking at this undated chart, the reader will not find it easy, on the basis of the fluctuations of the curves alone, to locate the war years on each curve of the chart. In the series depicted in Figure 1, what occurred in war years could have happened in peacetime, and vice versa.

For curves of other statistical series, the war period may be recognized approximately. Yet in such curves, undated, it is difficult to determine from them alone exactly when the war began. Six such curves are shown in Figure 2. Each of these curves is for a different seven-year period covering part or all of the second world war. The reader will be interested in trying to find from the curves in this chart the exact year that the United States entered the war. Although the bombing of the fleet at Pearl Harbor came with suddenness, the day is not so dramatically registered on these time-series. Most of the undated curves in Figures 1 and 2 are found dated in Figures 3, 4, or 5.

Figures 1 and 2 indicate that in some statistical records the recent wars of the United States have not produced enough of a change to be easily recognizable, or the change produced has not been very greatly different from what has occurred in peacetime.

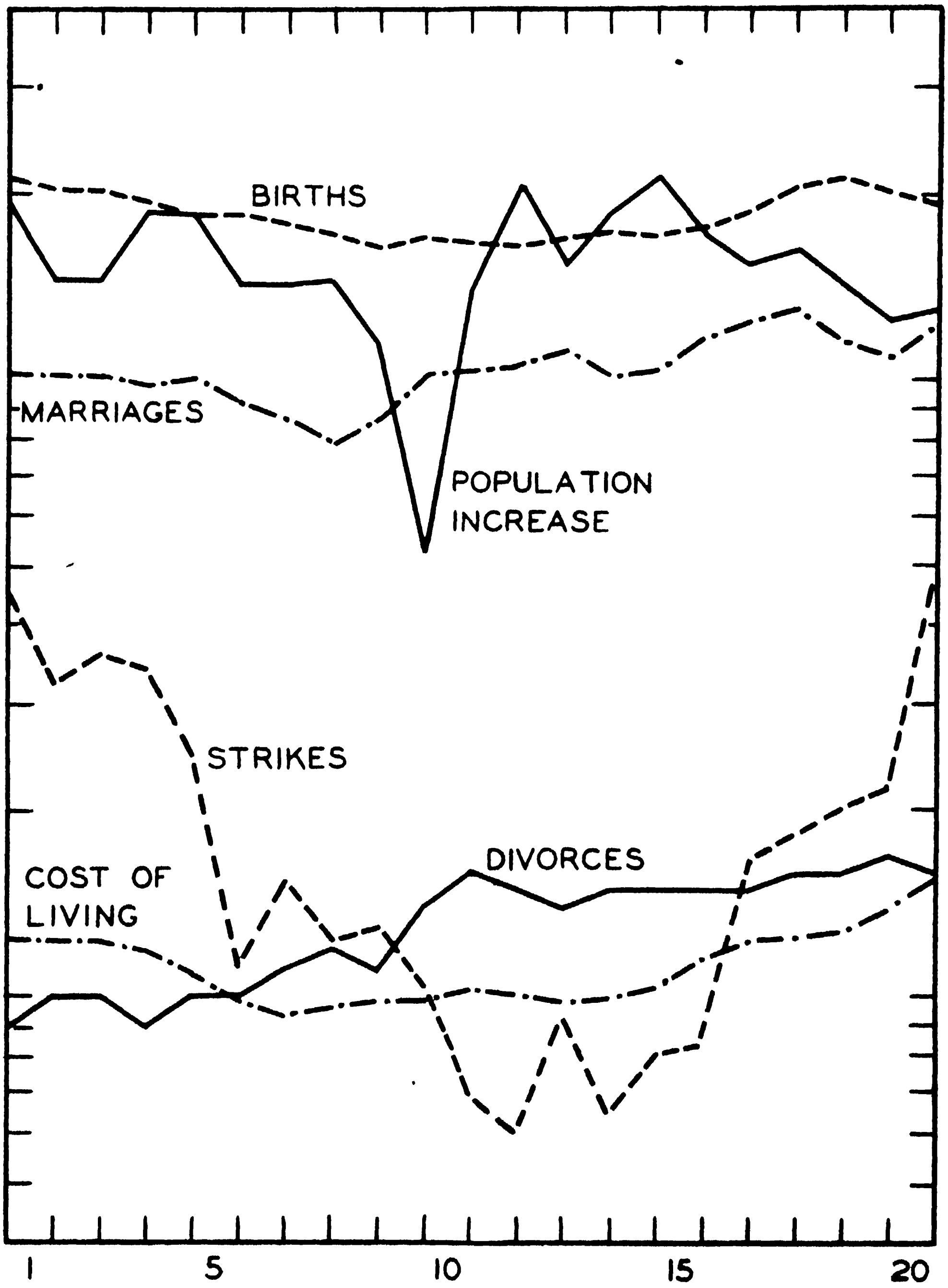


FIG. 1. WAR YEARS AND PEACE YEARS ALMOST INDISTINGUISHABLE

The six records cover 20-year periods, but the periods, dates given in the following paragraphs, are not the same; hence, the figures on the base line of the chart are not dates but numbers of years. Each series includes the years of either World War I or World War II. Without knowing the dates, it is rather difficult to locate the war years on the curves. The variations in the war years, then, are not significantly different from the variations that might be expected in peacetime.

Strikes and Lockouts (1917–37), total number of stoppages in thousands; Marriages (1925–45), rate per 1,000 population; Divorces (1910–30), rate per 1,000 population; Births (1925–45), rate per 1,000 population; Population Increase (1910–30), in millions, calculated from estimates of Bureau of the Census; Cost of Living (1927–47), index number of the Bureau of Labor Statistics; all series taken from *Statistical Abstract of the United States.*

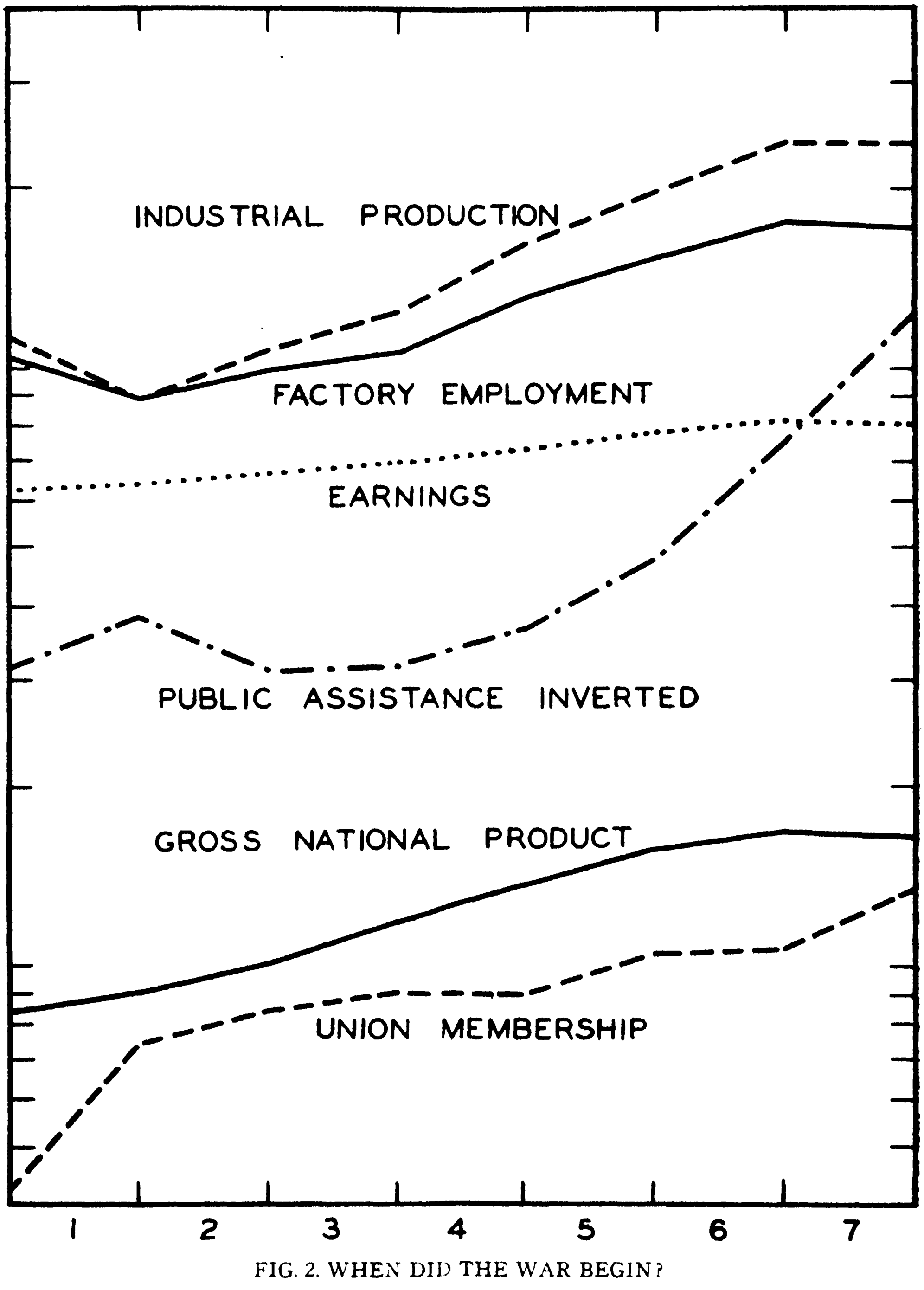


FIG. 2. WHEN DID THE WAR BEGIN?

The six curves include part or all the war years of World War II, but all curves do not cover the same seven-year period. We cannot tell from these undated curves, though, exactly when the war began.

Labor Union Membership (1936–43), in millions of members, from *Labor Information Bulletin*, August 1947, p. 4; Industrial Production (1937–44), index number from *Federal Reserve Bulletins*, 1935–39 = 100; Real Hourly Earnings of Production Workers in Manufacturing (1938–45), cents per hour, deflated by Cost of Living Index, from *Statistical Abstracts*; Factory Employment (1937–44), index number, *Federal Reserve Bulletins*, 1939 = 100; Gross National Product (1938–45), in billions, deflated by Cost of Living Index, from U. S. Department of Commerce, *National Income*, Supplement to Survey of Current Business, July 1947, p. 19; Public Assistance (1936–43), inverted, millions of dollars, deflated by Cost of Living Index, Federal Security Agency, Social Security Administration, *Social Security Yearbooks*.

The indices of prosperity measured our war years. Statistical series, presented like those in Figures 1 and 2, are used by economists to represent the fluctuations of business conditions. These fluctuations alternate between prosperity and depression. Such a course of business is often called a business cycle, although the alternations of business are not as smooth as the cycle of the tides. Five statistical series are commonly used to describe the course of business. These are industrial production; bank clearings outside New York City; wholesale commodity prices; freight car tonnage, representing production, trade, marketing, and transportation; and the percentage of all firms failing.

These five curves are shown in Figure 3. The depression years of the 1930s are clearly indicated, as is the prosperity of 1919–20 and the late 1920s. But the curves that are high in the prosperous business years of peacetime are also high in the years of World Wars I and II. This observation, we think, is important. The curves, used to show the business cycle, have the same behavior during the two world wars as during periods of peacetime prosperity. This behavior suggests that business was prosperous during war periods. This startling statistical inference is in sharp contrast to the idea that war consists only of horror and destruction, which it did in many countries.

The observation that war was a period of prosperity in the United States, as indicated by the time-series in Figure 3, may be explained by those to whom the idea is unwelcome by stating that the curves in Figure 3 (except the one on business failures) show only business activity, and not necessarily prosperity. Prosperity, it might be argued, is shown by profits; and that, although profits normally go with large production in peacetime, such need not be the case in wartime. In other words, industry could be very active in war years, from patriotic motives only, without making any profit at all. Such might be the case, but the curve of profits in Figure 5 shows that profits, like production, were greater than usual in the war year 1917, and again in the years of the second world war, 1942–45. Since the number of plants in operation was only a few more in 1917 than in pre- and postwar years, and was fewer than usual in 1942–45, we infer that the profits per firm were higher than normal.

The curves, then, which are used to measure the business cycle make the war years in the United States look a great deal like the prosperity phase of the business cycle.

Good times accompanied our war years. The years when business is prosperous are called "good times," that is, good times in general, not merely good times for business. There is more money to spend for clothing, for recreation, for travel, and for food. Institutions other than business—for instance, the family and the church—experience good times, too, when business is prosperous. There are more marriages, more births, a better standard of living for farmers, fewer suicides, fewer murders, fewer admissions to prisons, an increase in the membership of labor unions, less public assistance for the needy, and a higher income for the nation.

There are, though, some conditions of life in periods of prosperity that might not be properly called good times, but rather bad times. For example, there are more divorces granted in years of prosperity than in years of depression; and, strangely, there are more deaths in so-called good times than in bad. That we call periods of business prosperity good times rather than bad times indicates that there are more desirable than undesirable conditions accompanying prosperity.

It is interesting to see how the thirteen indices of good times listed in a preceding paragraph fare during wars. This has been done in Figures 4 and 5. These indices indicate both world wars as good times as measured by these thirteen series. In wartime, there was more marrying, fewer murders, more money to spend or save, etc. The associations of the war years were, then, in many activities of life in the United States, pleasant ones.

Distressing conditions found in war years, too. That there were unpleasant associations with wartime is also true. There were fear, sorrow, and separation from loved ones. Unfortunately, there are no index numbers for fear and sorrow; though the 14 million in the armed services is an indication of the extent of separated families and lovers. From the reservoir of time-series in the statistical yearbooks, a few annual series representing regrettable conditions are found (Fig. 6).

Looking at these curves, we may reflect on the degrees of distress they indicate for the war years. The curve for highway construction was low during the war, and discomfort was caused by the bad state of repair of the roads of our states and the streets of our cities. Taxes were higher during the wars, but increments of national income added were larger than the increments of taxes. The vast accumulation of the war debt was frightening, but its burden will be felt more in the years following the war than it was during the war. Rather, during the war the debt added an inflationary buoyancy.

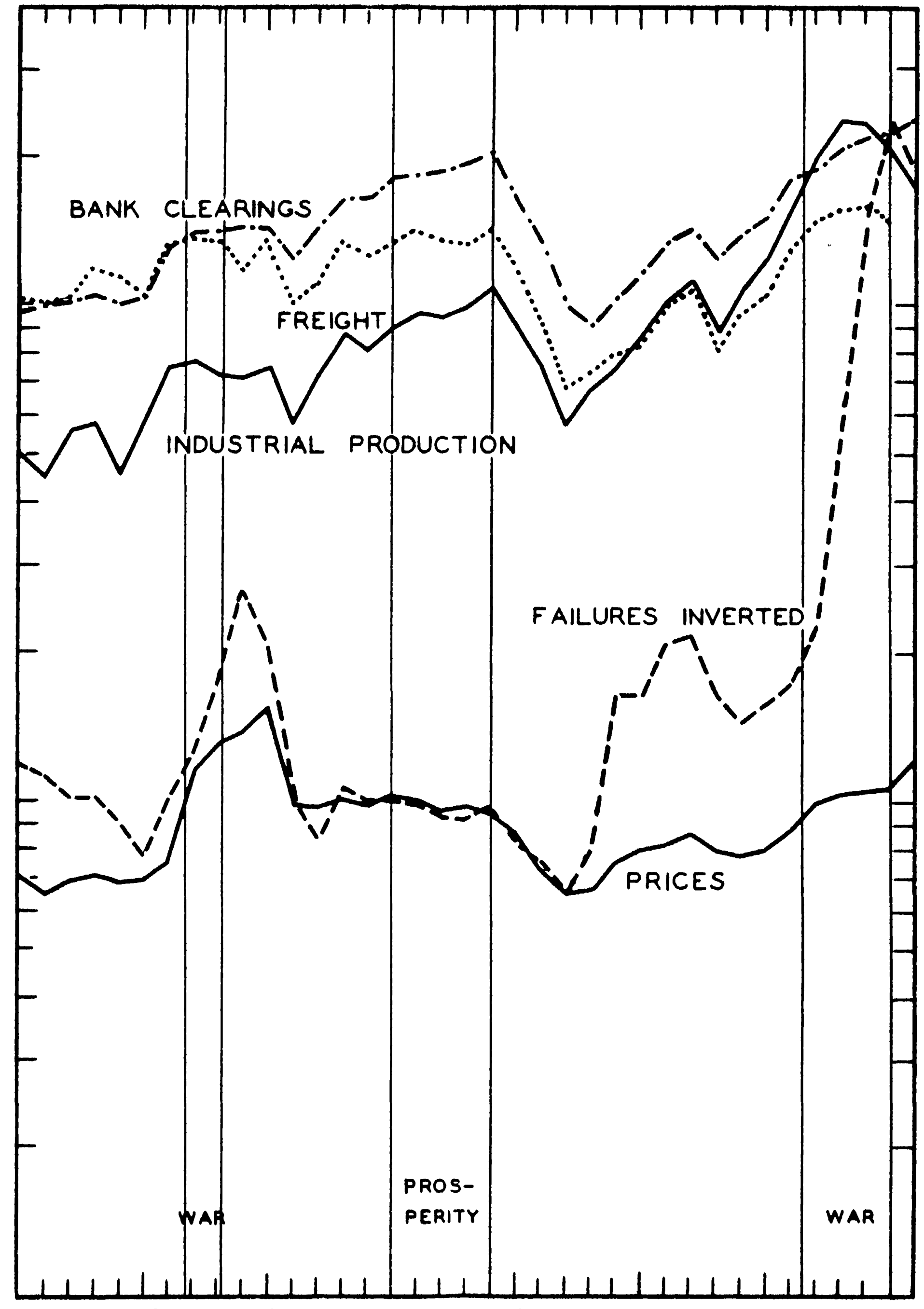


FIG. 3. CURVES USED TO MEASURE BUSINESS CYCLES

The curves ordinarily used to measure business cycles when traced through the period of World War I and World War II show the war years to be like the prosperity phase of the business cycle, as seen in the years of the late 1920s. If the war years were unknown, the curves alone would seem to indicate these war years merely as periods of business prosperity, which are higher on the chart than the preceding and following depression phases.

Freight, billions of tons of revenue freight originated, from *Statistical Abstracts*; Commodity Prices, Wholesale, index number from *Fed. Res. Bull.*, 1926 = 100, unadjusted; Industrial Production, index number from *Fed. Res. Bull.*, 1935–39 = 100; Bank Clearings Outside New York, in billions of dollars, deflated by Cost of Living Index, from *Com. and Fin. Chron.;* Fairures, industrial and commercial, inverted, number per 10,000 enterprises, from *Statistical Abstr.*

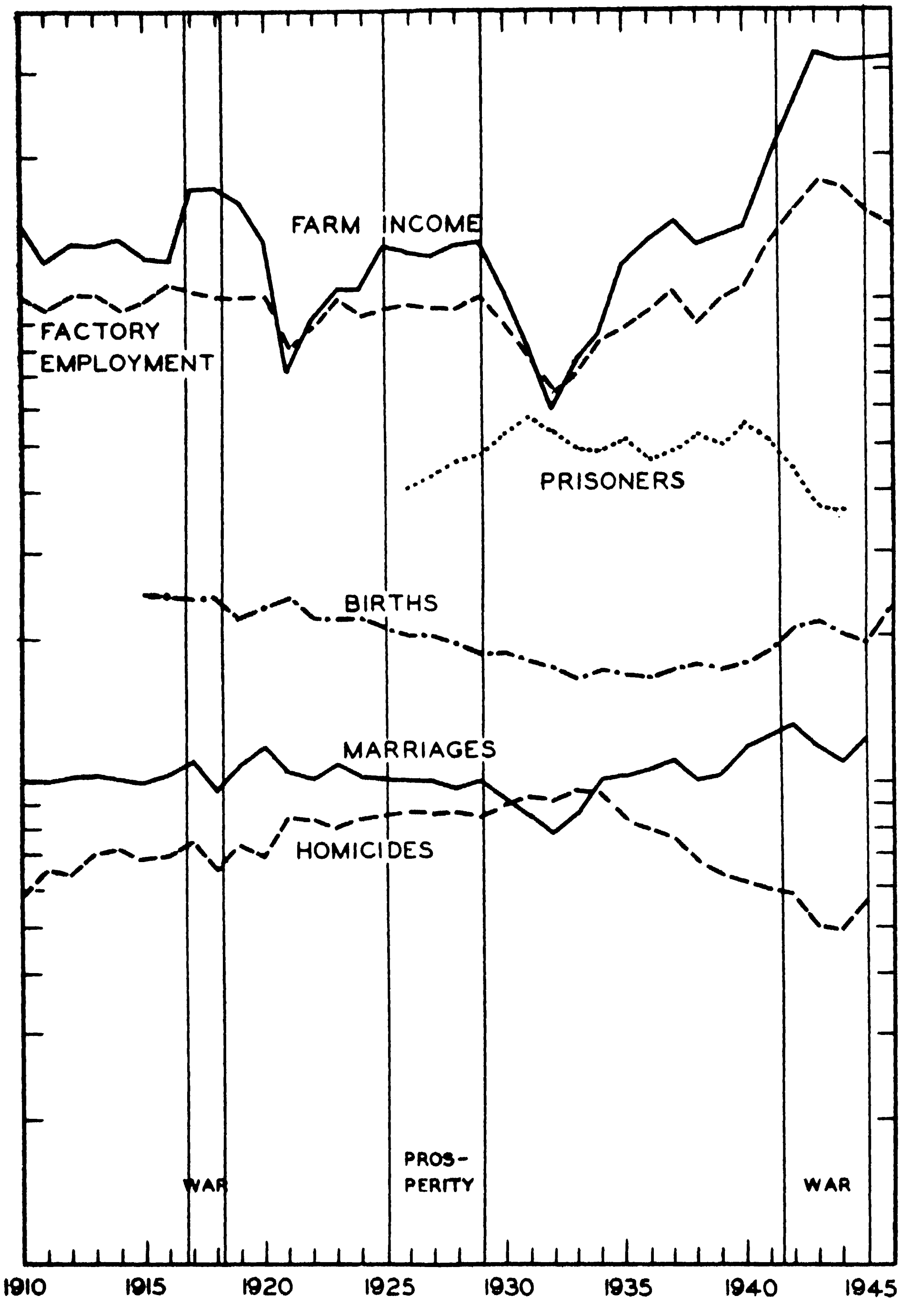


FIG. 4. GOOD TIMES IN WAR YEARS

With business prosperity are associated good times in other areas of life's activities. In Figures 4 and 5 curves are drawn that have been found correlated with the business cycle and are used as evidence of good times. These indices of good times generally show the same characteristics in war years as in periods of prosperity in peacetime; that is, the curves are higher (or lower) in both war years and years of prosperity than they are in periods of business depression.

Marriages, rate per 1,000 population; Births, rate per 1,000 population, both from *Statistical Abstracts*; Factory Employment, index number, *Federal Reserve Bulletins*, 1939 = 100; Farm Income, net income from farming to all persons on farms, per capita in hundreds of dollars, deflated by prices paid by farmers, including interest and taxes, 1910–14 = 100; Homicide Deaths, rate per 100,000 population, from *Statistical Abstracts*; Prisoners Received from Courts during the year, Federal and state prisons and reformatories, rate per 100,000 population, from *Statistical Abstract of the United States*.

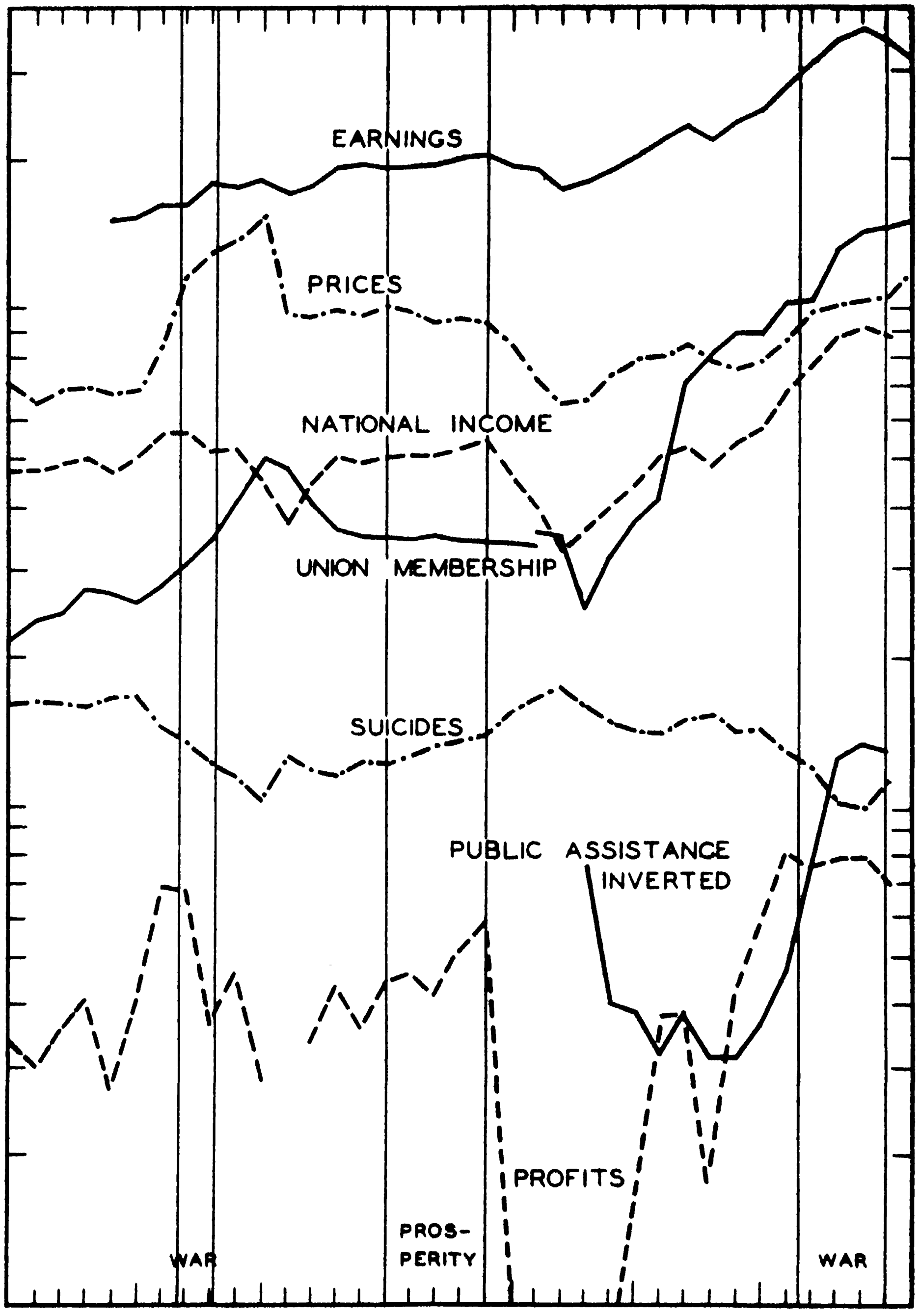


FIG. 5. GOOD TIMES IN WAR YEARS (*CONTINUED*)

Weekly Earnings of Production Workers in Manufacturing, deflated by Cost of Living Index, in dollars, from *Statistical Abstracts*; Commodity Prices, Wholesale, index number from *Federal Reserve Bulletins*, 1926 = 100, unadjusted; Suicides, rate per 100,000 population, from *Statistical Abstracts*; Labor Union Membership, in millions of members, 1910–1931 from Leo Wolman, *Ebb and Flow in Trade Unionism*, p. 16, average annual membership, 1931–1946, from *Labor Information Bulletin*, August 1947, estimated from graph; Public Assistance, inverted, millions of dollars, deflated by Cost of Living Index, from Federal Security Agency, Social Security Administration, *Social Security Yearbooks*; Net Corporate Profits, in billions of dollars, deflated by Cost of Living Index, below the bottom line of the graph is a loss except for 1934, which had a profit of .5 billion dollars, but is not shown—no figure is available for 1921—from *Economic Almanac*, 1946–1947, p. 44; National Income, in hundreds of dollars per capita, deflated by Cost of Living Index, from *Economic Almanac*, 1946–1947, National Industrial Conference Board figures.

It has been claimed that a smaller percentage of the eligible population voted in local elections during war years and that there was less interest in local affairs, but there are no published statistics available.

Divorces and deaths are tragic, but their wartime increase actually touches only a small proportion of the families. Divorces numbered 1,400,000 during the second world war, but without war there would have been about 940,000 divorces, assuming the same divorce rate as from 1937 to 1939. Thus, the added contribution to the number of divorces during the second world war was 460,000, whereas the number of married couples was 32 million.

The war deaths were about 350,000; in three and a half years of peace at that time there would have been around 5 million. Thus, the war added 7 percent to the number of deaths. Of course, no one of the families that furnished 14 million men to the armed forces knew that death would not strike. So there was fear, the memory of which remains as a terrible association of wartime, for which there are no statistics. To these distressing associations with war should be added the hard and fearful conditions of life for men at the front. One third of the medical discharges during World War II were for neuropsychiatric reasons.

We do not think it possible to strike a net balance between the good and the bad associations of war years in the United States. To many, the utter horror of war overshadows all else. Nevertheless, the data of this article do show that the indices we use to measure good times characterize the periods of our activity in the past two world wars. These observations are made on the United States and not on France, Japan, or Russia, where the sufferings of war were much more evident.

War and preparation for war. Our analysis has concerned the movement of certain indices of social conditions during war years, but in the United States the years of war were not all spent in fighting. During the first parts of World War I and World War II we did little or no actual fighting, for we were not prepared to fight. We entered both world wars suddenly, with our soldiers inadequately trained and equipped. So the war periods were divided into two parts. The first part was characterized by preparation, which might have been done in peace years; in the second part we were engaged in actual fighting. We were able to be at war without much fighting because our allies held back the enemy.

The question arises, then, as to whether the prosperity and good times that characterized the war years of the United States were due to the preparation for war or to the conditions of a fighting war.

It is easy to see how preparation for war would bring great business activity and profits; and, of course, the good times that accompany industrial prosperity. But how about the fighting years of war after the initial period of preparation was over? In order to compare these two parts of our war years we have prepared Figure 7, in which are drawn some statistical indices of business conditions by months during the period between the declaration of war and the collapse of the enemy. The first year of this period and most of the second year were months of preparation, with little fighting. During the last year and a half of the war period, the participation of armed forces in battles and bombings increased.

The curves in Figure 7 were rising in the first part of the war, generally, until around the close of 1943 or the beginning of 1944, as they do in the business cycle with the recovery of industry from a depression. Such was expected. The question is, How did these curves behave after the period of preparation was over? The chart shows that they generally ceased rising, and flattened out. In no case was there a significant recession during the fighting years of World War II, from the peak or plateau of business prosperity. In other words, the short period of the actual fighting war was characterized by the same industrial boom times that signalized the period of preparation for fighting. We do not know what the course of the curves in Figure 7 would have been if the war had lasted three or four years longer, nor what it would have been if we had been invaded or bombed extensively.

Conclusion and discussion. The conclusion of this study as shown by the data is that the experiences at home in the United States of our social and economic institutions and activities during the periods of our participation in the last two world wars were much like those of the prosperity phase of the business cycle in peace years, commonly called good times; though there were some incalculable conditions of fear and distress.

The United States was more fortunate than were the combat countries of Europe and Asia that were bombed or invaded and that were in the war longer. Our industries were more prosperous than in other warring countries. The positive association of good times in the United States during the war probably has a bearing upon our willingness to engage in another war. Rewards and punishments, we know from psychology and from experience,

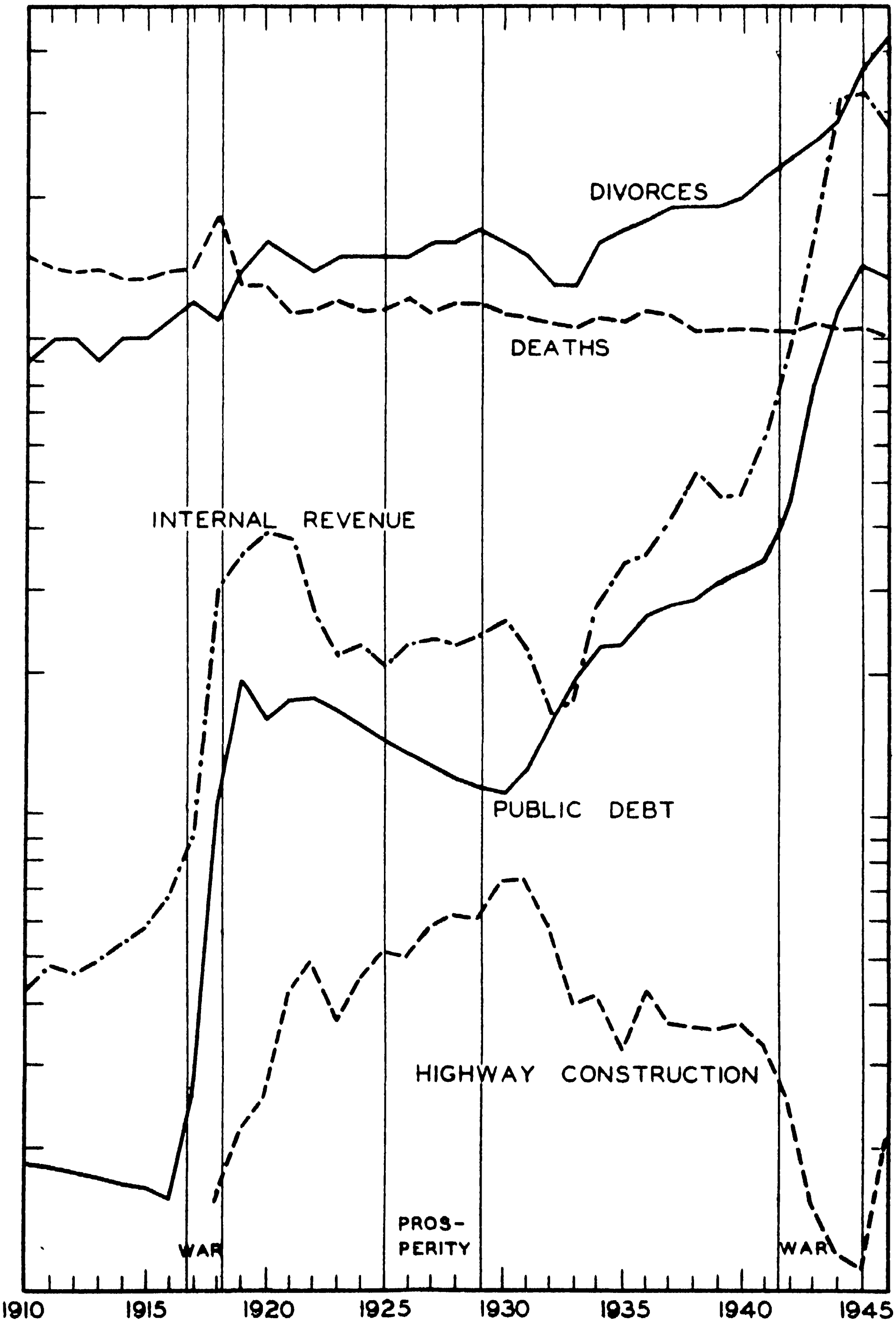


FIG. 6. SOME DISTRESSFUL CONDITIONS OF WAR YEARS

Not all the activities of war years can be called prosperity and good times. Statistical records reveal some unfortunate conditions, such as those shown by the five curves of time-series found in the statistical yearbooks. There are other distressing conditions in war years, such as fear, for which there are no statistical records.

Deaths, rate per 1,000 population; Divorces, rate per 1,000 population; Public Debt, dollars per capita, deflated by Cost of Living Index; Internal Revenue, income and profits taxes plus other revenue, in billions of dollars, deflated by Cost of Living Index, all from *Statistical Abstracts*; Highway Construction, in millions of dollars, 1918–1929, from *Economic Almanac*, 1946–1947, p. 235, 1929–1946 from *Statistical Abstr.*, 1947, p. 968, deflated by *Eng. News Record* construction cost index, which measures the movement of construction costs in general, from *Statistical Abstr.*, 1947, 778.

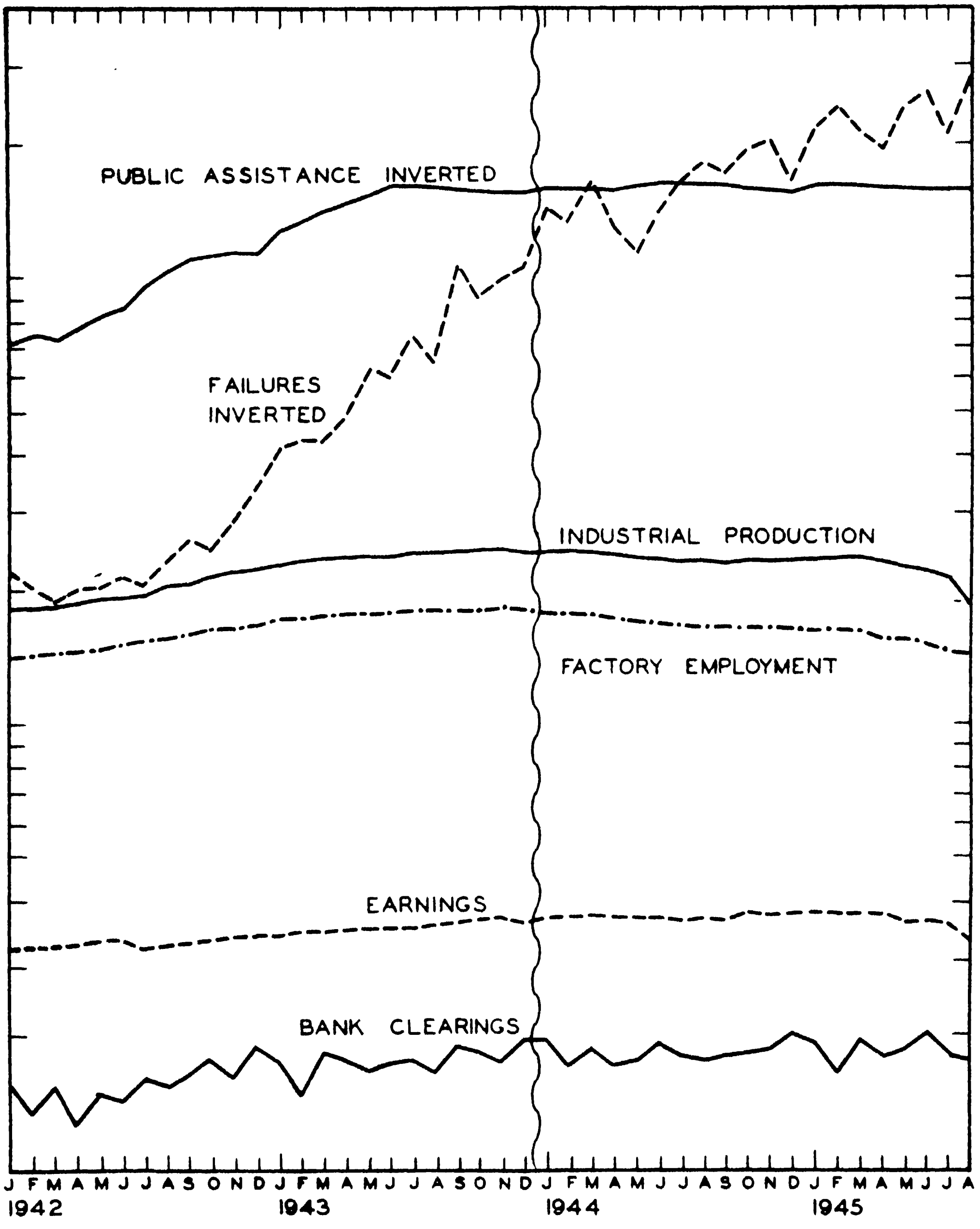


FIG. 7. PREPARATION FOR WAR, AND COMBAT WAR

In the first part of the years in which the United States was officially at war in 1941–1945, activities were largely preparatory. In the latter part of the period, combat fighting was much more extensive. The wavy line indicates, approximately, the division between these two periods, but more accurately for some of the curves than others. These curves, which have been used to indicate good times and prosperity in peace years, are high both in the preparatory and in the fighting phases of the war years.

Industrial Production, index number from *Federal Reserve Bulletins,* 1935–39 = 100; Bank Clearings Outside New York, in billions of dollars, deflated by Cost of Living Index, from *The Commercial and Financial Chronicles*; Weekly Earnings of Production Workers in Manufacturing, deflated by Cost of Living Index, in dollars, from *Statistical Abstracts*; Public Assistance, inverted, millions of dollars, deflated by Cost of Living Index, from Federal Security Agency, Social Security Administration, *Social Security Yearbooks*; Failures, industrial and commercial, inverted, number per 10,000 enterprises, from *Statistical Abstracts*; Factory Employment, 1919–1946, index number, *Federal Reserve Bulletins,* 1939 = 100, 1910–1918 from Paul F. Brissenden, *Earnings of Factory Workers, 1899–1927,* p. 61, decreased by average percentage of difference in 1919–1920.

are effective determinants of behavior. Animal trainers find that their subjects repeat more readily when rewarded and that they desist more quickly when punished. Our religions have a heaven and a hell. Rewards and punishments, however, are not the only factors bearing upon behavior and shaping our attitudes toward war.

In view of this possible influence of our past fortunate war experiences upon our attitudes toward engaging in future wars, it is well to remember that we may not be so fortunate in another war. We may be in another world war from its beginning and not after the enemy is partly worn down by fighting with our allies. Nor are our allies likely to hold the enemy while we take a year or two to prepare. It is almost certain, because of the existence of long-range bombers of great speed, that we shall be bombed in the next war. The bombing will be very destructive if our enemy has the atom bomb. Nor is it clear that such a war will be brief.

◆

REMNANTS

These are still left: a budding, wayside elf
That droops dust-covered; the moth a blinded troll;
Birds are shy sprites hurtling from hole to hole
In the shattered veil of Earth; the Earth itself,
Fear host to little things that dare to thrill
And glow only in midnight mystery;
The dwindling mammals slip away and flee
From all erect, who crush what is not still.
Man ever burgeons! He is the imp that mars
All that is beauty, all the ethereal
Magic, Earth makes of gleam and waterfall;
Nothing is safe but cloud and brooding stars.
God save these remnants whatever Man may do,
When ribs of cities totter against the blue.

Robert Thomas Moore

BURTON E. LIVINGSTON, 1875-1948

WARREN B. MACK
Department of Horticulture, The Pennsylvania State College.

MY FIRST meeting with Professor Burton E. Livingston took place in his laboratory at Johns Hopkins during the summer of 1927, in an interview he gave me in response to my inquiry as to whether he would accept me as a graduate student. I arrived at his laboratory a little after 11:00 A.M., the hour he had named, and I was concerned about infringing upon his lunch hour. But he had left word with his secretary to call him at his home when I arrived, and she assured me that he would come in at once and would not worry about lunch.

The interview lasted until well into the evening, and my protests about delaying his supper were smilingly disregarded. Several conversations of similar duration took place before plans for my studies were ready to go into effect in the fall of 1928; these interviews, I discovered, constituted a good part of my preliminary examinations, informally conducted. It was impressed upon me, however, that it was necessary to clear the formal requirements of the University—"hurdles," he called them—as decisively as possible.

Lectures and laboratory exercises in plant physiology I found to be equally informal. Professor Livingston announced several days after the official opening of the University that he would lecture from 11:00 to 12:00 on Mondays, Wednesdays, and Fridays; actually, the lectures might begin at any time up to 11:45 and continue to 1:30 or later. Students brought their lunches to the laboratory and worked there until Professor Livingston started from his office to the classroom, when we all filed in with him. After the lecture, the students returned to their desks in the laboratory and ate their lunches; Professor Livingston accompanied one or more students, discussing their work or any other topic that might come up. Usually, the conversation narrowed down until it centered upon one student, and it would continue for an indefinite period, sometimes until 9:00 in the evening or even later.

As little attention was paid to the calendar as to the clock. The Friday after Thanksgiving Day was part of the holiday, but the students came as usual, and in due course the Professor appeared too. The same thing might have happened during the Christmas recess also, but the annual meetings of the American Association for the Advancement of Science then claimed Professor Livingston's time and energies.

His lectures, though informal in presentation, were well organized, and they were characterized by many references to the persons who had contributed to knowledge under each topic, many of whom had worked in his laboratory. Methods of measurement and their invention and improvement were allotted a great deal of time, and always the ones who invented and improved were named and paid full tribute.

As to laboratory exercises, typewritten outlines of the classical experiments were issued to each student, but after he had completed a few general experiments, the student was urged to examine critically one or a limited number of the experiments in which he had a special interest. Some of the students, for example, spent several months on Askenasy's demonstration of the tensile strength of water enclosed in capillary tubes; their work discovered the precautions required to ensure a reasonable degree of reliability to this demonstration, which formerly was a hit-or-miss affair.

Aimless activity or lack of sustained interest was discouraged. The student who characteristically wrote verses about raindrops coursing down the laboratory windowpanes and forgot to make readings on his instruments was encouraged to pursue studies elsewhere, even though the verses, according to Professor Livingston, were fairly good.

The objective point of view was maintained at all times; the student was required to observe quantitatively and discover relations among events and conditions, but he met little enthusiasm for speculation or conjecture. He had to account for all his observations and conform with rigid logical procedure. Of the student who thought that his technique must be faulty because a decapitated plant did not develop positive pressure in his experiment on root pressure, Professor Livingston asked: "How do you know? All plants do not develop the same pressure. Maybe this one did not develop any."

The Laboratory of Plant Physiology was small, but was well planned and well equipped; shops with wood- and metal-working and glass-blowing equipment provided ample facilities for the mechanically adept student, who met every encouragement to devise apparatus for more accurate measurement of processes, or for more precise control of environmental conditions. Apparatus such as porous cup atmometers, to measure evaporation, and unglazed porcelain cones, to measure the water-supplying power of the soil, which were developed in these shops, were deliberately designed to be simple and inexpensive, to bring them within the reach of as many laboratories and students as possible. For any purpose, however, the apparatus had to be adequate; sometimes it became very elaborate, as, for example, that used to measure the oxygen-supplying power of the soil.

Professor and Mrs. Livingston always maintained friendly social relations with students and graduates in their home or at their summer camp site on the Magothy River. Conversations were lively, but they usually came back to plants, which were present in the house and were grown under various conditions, including water culture, in the home gardens. In these general conversations, politics, religion, philosophy, literature, and the arts came up, but they were of secondary interest to plant science; all were approved, however, as human activities having value both in the maintenance of the social order and in the development and recreation of personality.

Though the periods of sustained philosophical discussion were few, both in lectures and in conversations with students severally or in groups, brief allusions to Dr. Livingston's scientific point of view were quite frequent. He reacted against the "teleology or even out-and-out mysticism" that characterized most writers in biological science up to the turn of the century; as he stated, "My *Weltanschauung* seems always to have been of the aetiological rather than of the teleological sort." This point of view evidently was not studied nor consciously adopted; it was a thoroughly characteristic part of his personality, a result of his own reactions to all the influences and personalities he had met during his lifetime.

His life was almost completely *expressed* during his thirty-one years (1909–40) in the Laboratory of Plant Physiology of the Johns Hopkins University; it seems to have been *shaped* almost as completely by his experiences before he accepted the professorship there.

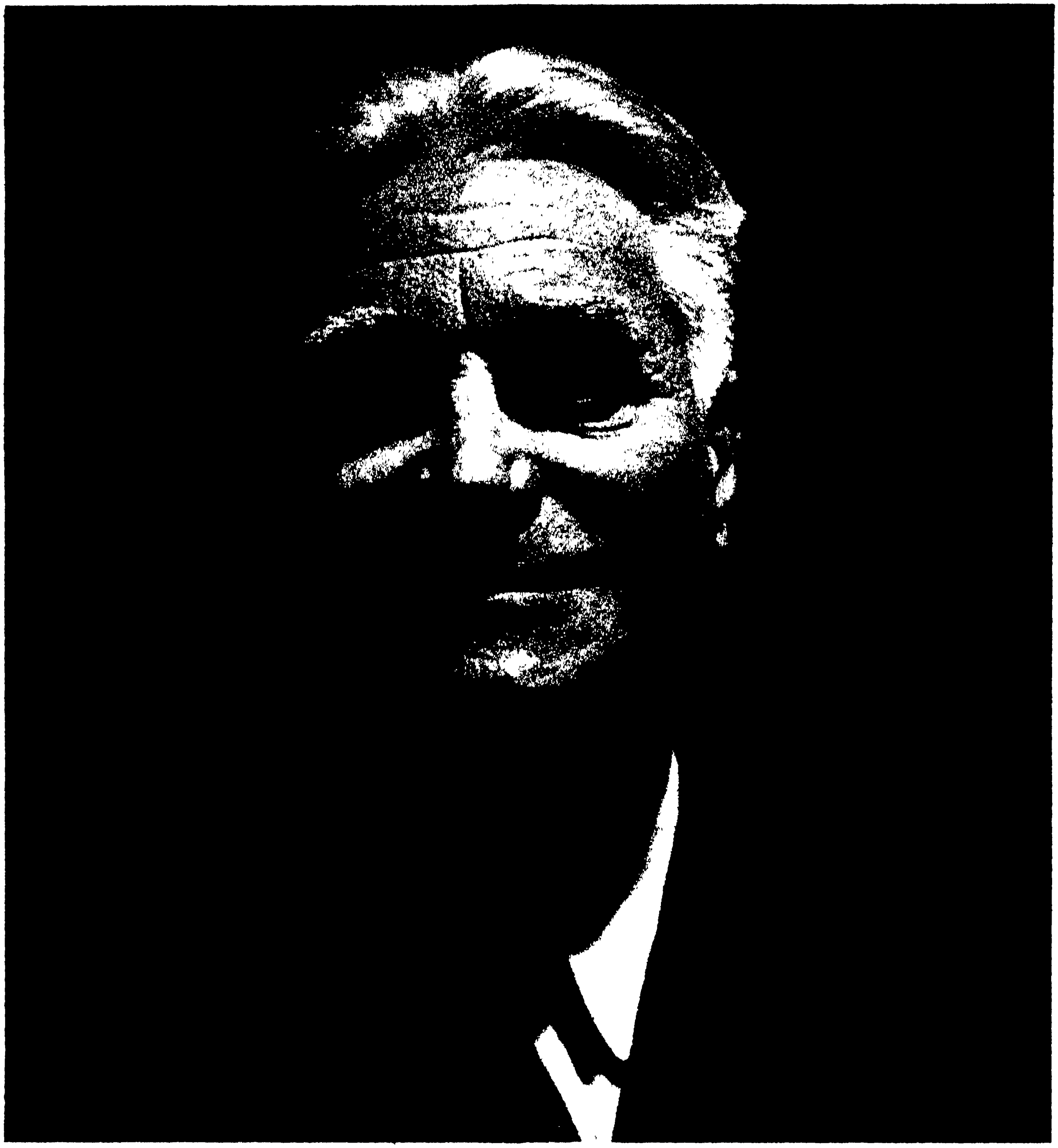
Dr. Livingston's birthplace and boyhood home was on the outskirts of Grand Rapids, Michigan; his parents were farm-reared, but in the rapidly growing city his father was a street grading contractor. The home lot was large enough, however, to express the farm-fostered interests of his parents. There were vegetable and flower gardens, collections of wild flowers, and ornamental plants outdoors and many potted plants indoors, including an orange and a lemon tree grown from seeds that Burton planted when he was less than four years old.

Parents and older brothers and sisters were especially interested in plants; and this interest, which probably came naturally to him, was heightened in Burton by many excursions with older brothers to near-by bogs, streams, and woodlands in which the native vegetation grew undisturbed, as well as to upland meadows and fields with their cultivated crops. They called many plants by their Latin names, and Dr. Livingston could not recall the time when he did not recognize the two *Hepatica* species native to that region as *triloba* and *acutiloba*.

Another feature of his home environment, which exerted a strong influence, was the fact that during the winter months his father's tools and equipment were at home, where he could repair them for use during the following summer; the boys assisted him, acquiring skill in simple mechanical operations and wood-working. With tools at hand, the boys naturally employed them to construct devices for their own use, including, on Burton's part, canoes, sleds, glass-covered trays for a butterfly collection, and presses and cases for plants. Books of a great variety and a small microscope were at hand for leisure reading and study.

His high-school training, begun when he was fifteen, was characteristic of the time and included a considerable range of natural science. He accomplished the equivalent of three years of Latin and one of Greek as extras, studying them by himself during the summer; this home study indicated his avid interest in languages, which was maintained throughout his life, as well as a penchant for acquiring knowledge independently.

Books and magazines were the foremost recreation of the Livingston family. "Everyone in the family circle read and read," according to Dr. Livingston. The children played the card game "Authors," became acquainted with the portraits and titles of noted authors, and read many of their works as they grew older. Burton read "dozens and dozens of boys' books," Longfellow and Bryant, parts of the great religious books of the world, and Thoreau, Emerson, and Carlyle before he finished high school. He bought a morocco-bound set of Shakespeare's works with money earned by mowing neighbors' lawns.



BURTON E. LIVINGSTON, 1875–1948

After finishing high school, he spent a week at the first Chicago World's Fair, "a good deal of it in the horticultural building," then worked a year in Pitcher and Manda's nurseries at Short Hills, New Jersey, where his older brother Luther, who had spent a year collecting cattleyas in Venezuela and Colombia for the firm, was a cataloger and bibliographer. His labors acquainted him with a great variety of cultivated plants, and he extended his acquaintance to the wild plants of the Short Hills region on Saturday afternoons, Sundays, and holidays, as well as during the midsummer of 1894, after he had left the employ of the nursery, and until he entered the University of Michigan in the fall. At the same time he studied Spanish with his brother, who had made a beginning at it while he was in South America.

His herbarium, begun in high school, and added to as a hobby until the fall of 1894, earned him ten hours of advance credit at the University and, with his high-school course, constituted all his schooling in taxonomic botany. At the University, he worked at a plan to catalog the plants in the region of Ann Arbor according to habitat, with the sympathetic interest of V. M. Spaulding. During his first college summer vacation, he read works on plant geography in the botany building at Columbia University while visiting his brother Luther in New York, and at that time became acquainted with N. L. Britton and John K. Small. On his return to Ann Arbor, he studied plant physiology under F. C. Newcombe, became his assistant in 1895, and continued in that capacity until graduation in 1898.

He taught the natural-science courses in the Freeport, Illinois, high school during the first year after his college graduation, and then became a Fellow and assistant to Professor C. R. Barnes at the University of Chicago, under whom he had charge of the laboratory exercises in plant physiology until 1905. He became deeply interested in ecology, which was being developed at Chicago largely through the work of H. C. Cowles. He continued his work begun while a senior at the University of Michigan on the influence of the osmotic value of solutions on a fresh-water alga, and published *The Role of Osmosis and Diffusion in Plants* in 1903. At Chicago he attended the lectures of J. M. Coulter in plant morphology, Jacques Loeb in general physiology, and F. Lengfeld and J. O. Stieglitz in chemistry. The Ph.D. degree was conferred upon him by the University of Chicago in 1902.

While he was a Fellow at the University of Chicago, he spent one summer recess teaching botany at the Eastern Illinois State Normal School, and another at the New York Botanical Garden, where he became acquainted with its director, D. T. MacDougal, and with F. E. Lloyd, H. M. Richards, and C. C. Curtis, fellow-workers with whom he maintained a lifelong friendship.

The summer of 1905 was spent at the Desert Laboratory of the Carnegie Institution of Washington, at Tucson, Arizona, and the early winter of 1905–06 in the U. S. Bureau of Soils, where he was engaged in soil-fertility investigations. He was a staff member of the Carnegie Institution of Washington from 1906 to 1909, part of which period he spent at the Desert Laboratory, working under MacDougal's direction on problems of physiological ecology, mainly on the relations of plants to soil moisture and to evaporation; he also spent some time at the Missouri Botanical Garden with William Trelease, H. von Schrenk, and J. A. Harris; and the year 1908 in Europe, mainly at Munich with von Goebel, Hegi, and Renner, but also at Leipzig with Pfeffer, and at Heidelberg with Klebs and Gluck; he made, besides, trips through Germany and Switzerland and a brief visit to the Rothamsted Station in England.

On his return to America he attended the AAAS meetings of 1908 in Baltimore, and there saw Professor Johnson and his newly established botanical garden on the Homewood tract, soon to become the campus of the Johns Hopkins University. This meeting led to an offer of an appointment as professor of plant physiology at the Johns Hopkins University, sent to him by telegraph while he was on a tour of southern California with MacDougal and several visitors to the Desert Laboratory, in the early summer of 1909.

Professor Livingston once told me that he found Lon A. Hawkins waiting on the steps of his laboratory on his first day in the new position; Hawkins announced that he would be his first student. Other students during that first year were F. H. Blodgett, W. D. Hoyt, W. R. Jones, L. W. Sharp, H. H. York, and W. H. Brown. With members of this group, Professor Livingston designed and constructed new equipment for the laboratory, and, when the new building which was to serve during the remainder of his professional life was erected in the winter of 1911–12, together they designed the interior details and installed some of the equipment. He once said that he had "gained as much from those students as they gained from [him]."

The summers of the first several years after he joined the faculty of the Johns Hopkins University were spent at the Desert Laboratory, carrying forward studies on soil moisture and evaporation supported by grants from the Carnegie Institution, and with the help of different assistants. He frequently acknowledged his indebtedness to these men: W. H. Brown, J. S. Caldwell, E. M. Harvey, H. E. Pulling, J. W. Shive, A. L. Bakke, H. C. Sampson, E. S. Johnston, J. D. Wilson, G. S. Fraps, and L. J. Pessin.

In 1921, he spent the summer with Mrs. Livingston at the Desert Laboratory, completing studies begun and carried on with Forrest Shreve during ten years. That year he published *The Distribution of Vegetation in the United States, as Related to Climatic Conditions*. Two later summers, those of 1922 and 1926, also were spent at the Desert Laboratory.

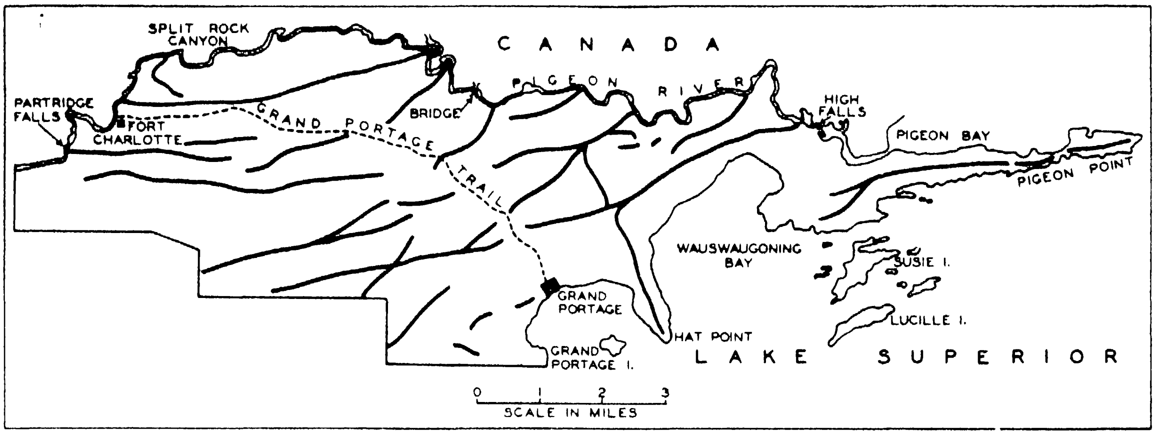
The studies carried on by Professor Livingston and his students in the Laboratory of Plant Physiology at the Johns Hopkins University dealt with the influences of all phases of the environment—temperature, radiation, atmospheric and soil moisture and oxygen composition of the atmosphere, air movement and salt content, other dissolved or colloidal substances, and osmotic value of the soil or nutrient solution—on the major plant processes, transpiration, respiration, absorption, photosynthesis, germination, growth, configuration, vigor, pathology, development, and distribution of plants. Much attention was paid to the dynamic aspects of the environment, the rates at which substances or energy are made available to plants and at which plants absorb, transform, and utilize them. Physical and chemical properties of environmental phases were studied apart from their relations to plants in many instances, and practical applications to agriculture, horticulture, and forestry received considerable attention. A list of publications from the laboratory, from 1909 to his retirement to the status of emeritus professor in 1940, by Professor Livingston and his students and associates, includes 285 titles, six of which were those of books containing 360–979 pages. Those who worked or studied in his laboratory numbered 137, and of these, 28 received the Ph.D. degree from the Johns Hopkins University with him as their major professor.

Professor Livingston's energies were extended to the dissemination of knowledge by means other than teaching, lecturing, editing, and writing. He was active in the affairs of the American Association for the Advancement of Science throughout most of his professional life; he was its permanent secretary (1920–31), general secretary (1931–34), member of the Executive Committee (1920–46), and chairman of the Executive Committee (1941–45). During his service as permanent secretary, he began several activities to increase the value of the Association to its members, among which were the publication of the *Preliminary Announcement, Reports* of the Annual Meetings, and periodic *Summarized Proceedings*. He is given credit by his associates for placing the financial affairs of the Association upon a secure basis, and for introducing or developing general sessions and popular lectures, as well as the Exhibitions, press services, Association Prize, secretaries' conference, Academy conference, and *General Program* as features of the Annual Meeting of the Association.

He was active in the American Society of Plant Physiologists from the drafting of its constitution and the organization of its financial program, in which he played a conspicuous part, to the end of his life. He was its president in 1934, was elected its first Barnes Life Member, and was awarded its Stephen Hales Prize in 1946. Other societies of which he was a member and to which he contributed energies and resources were the National Research Council; the American Philosophical Society; the Botanical Society of America; the Ecological Society of America; the American Society of Naturalists, of which he was president in 1933; the American Academy of Arts and Sciences, of which he was a Fellow; and the honor societies of Sigma Xi and Phi Beta Kappa, the latter of which he served as a member of its editorial board.

He died on February 8, 1948, one day before the seventy-third anniversary of his birth. He is survived by his second wife, the former Marguerite A. Brennan Macphilips, to whom he was married in 1921, and with whom he collaborated throughout their married life, in the production, standardization, and distribution throughout the world of porous porcelain atmometers, cones for measuring the moisture-supplying power of the soil, and autoirrigators, as well as in other phases of his work, studies, and travels. His life is a distinguished example of what the traditional American family, home, and educational environment are capable of producing.

◆



A FAMOUS WILDERNESS HIGHWAY

KARL VER STEEG*

Professor Ver Steeg (Ph.D., Columbia, 1930) has been teaching at the College of Wooster since 1923 and is now head of the Department of Geology there. He became interested in the Grand Portage while working on a problem in geology in northeastern Minnesota, and carried out the field work in the summer of 1946 under a grant from the William H. Wilson Research Fund.

NEARLY one hundred and fifty years ago the Grand Portage, one of the oldest and most important highways in North America, was abandoned. Today, deer, bear, and moose wander across the route of this ancient forest highway, which was once used by thousands of Indians, fur traders, and *voyageurs.* It is now overgrown with vegetation, but one can still trace its course, in places, by the depressions worn by the trampling of thousands of feet in the moss and forest duff. Majestic pines, spruce, and white birches line the trail over which passed the traffic of half a continent. In this wilderness the names are centuries old.

The geological and geographical factors responsible for the Grand Portage are interesting examples of the effect of surface features on human activities. A study of the map reveals the Pigeon River flowing in an easterly direction from the site of old Fort Charlotte. This crooked stream flows nearly 20 miles to reach a point only 12 miles distant as the crow flies. Below Partridge Falls, the Pigeon River has deep, steep-walled, rocky gorges; one of these, Split Rock Canyon, is spectacular. The lower course of 20 miles to Lake Superior is interrupted by many falls and rapids; the largest is High Falls (90 feet).

Bordering Pigeon River on both sides is rugged country; on the Canadian side the land is too mountainous and the distances are too great for portaging to be practicable. It is almost impossible to navigate the lower Pigeon River by canoe or boat and equally difficult to find a good portage above it. Grand Portage Bay extends so far back into the southwest-trending shoreline of Lake Superior that the distance to the Pigeon River above the falls and rapids is considerably shortened. Along Pigeon River the distance is 20 miles and difficult to traverse, whereas the more direct route by way of the Grand Portage is less than 9 miles.

The region back of the village of Grand Portage and in the immediate vicinity is also rugged, the ridges being high and steep-sided, often with bluffs. These hills, which stand several hundred feet above the level of Lake Superior and the adjacent valleys, look almost like mountains and form a nearly continuous, impassable barrier in an east-west direction. There is a series of deep, broad gaps through the ridges directly in line with the village. These gaps afford a gradual and rather low gradient from Grand Portage Bay to Pigeon River below Partridge Falls near Fort Charlotte.

---

* The author is indebted to Drs. G. M. Schwartz and F. F. Grout, Geology Department, University of Minnesota, for the loan of manuscripts. Other source material was The Story of the Grand Portage (Solon J. Buck: *Minn. History Bull.* 5, 14–27, 1923) and *The Voyageurs' Highway* (Grace Lee Nute: Minn. Historical Soc.).

I found the walk along the trail not difficult as compared with portages having steep ascents over rocky outcrops. With the exception of local undulations—usually of gentle gradient—the trail is a gradual ascent from Grand Portage Bay through the gaps in the ridges. In the first 1.6 miles from Lake Superior, the rise is about 211 feet per mile; the remainder of the distance to Pigeon River has an average gradient of about 40.5 feet per mile. Thus, most of the rise takes place in the first few miles from Grand Portage Bay. Occasionally, muddy and swampy conditions after heavy rains make the portage more difficult.

The characteristic topography of the area north of the village of Grand Portage extends well beyond Pigeon River into Canada and westward for 55 miles to Gunflint Lake. It consists of a belt of ridges stretching for the most part in a nearly east-west direction, having a northeast-southwest trend in the area north and northwest of Grand Portage Bay (black lines, Map, p. 39).

The area through which the Grand Portage trail passes is underlain by two main types of rock, widely different in their resistance to erosion. The weaker rock is the Rove slate, of Upper Cambrian (Animikie) age. This formation was intruded by sills and dikes of diabase of Keweenawan age. Where the intrusions cut across the slate beds vertical walls, or dikes, were formed. The formations have been folded, and the slate beds and diabase sheets now tilt at a gentle angle (5°–15°) toward Lake Superior. The topography developed on rocks of unequal hardness, having a dip in one direction (monoclinal) toward the south and striking about N. 70° E., or nearly east and west, is that of a series of parallel unsymmetrical ridges with the steep side facing the north and long gentle back slopes to the south. This topography resembles the Appalachian Ridge and Valley surface features, the only difference being that it is on a smaller scale, and the prevailing dip of the hard rock beds which constitute the ridges is mainly in one direction and averages lower.

The valleys have been modified by glaciation, which scoured the ridges to some extent and deposited debris in the valleys, thus changing the preglacial drainage from streams to chains of lakes running parallel to the ridges. Following the recession of the ice sheet, the streams for the most part followed new courses and postglacial gorges were cut. In places, water gaps cut by transverse streams through the ridges in preglacial time were abandoned, the streams taking new courses as a result of glaciation.

Two broad, open notches in the diabase ridges are present within 1.5 miles of Grand Portage and

HIGH FALLS, ON PIGEON RIVER

another in the ridge immediately to the north of the village, 2.5 miles from Lake Superior. A fairly large stream must have occupied these gaps in preglacial time. The present stream flowing through the gaps, Grand Portage Creek, is a misfit, a small postglacial stream occupying a large valley, the former site of a much larger river.

Beyond the last important gap, about 2.5 miles from Grand Portage Bay, the trail turns to the west and in a broad curve follows an almost level route on the back slope of a gently dipping diabase ridge, avoiding the great swamp that extends almost directly eastward from Fort Charlotte. It appears that Grand Portage trail follows a natural route connecting Lake Superior with the great chain of interior lakes to the northwest and that it was a preglacial river course cut through the gaps in the ridges.

The present inhabitants of the village of Grand Portage number about 300 and are mainly descendants of the Chippewa (Ojibway) Indians, survivors of the once-powerful, warlike tribe that occupied the site from time immemorial. These Indians have been immortalized in the famous poem "Hiawatha." Longfellow went to the wilderness of the north shore of Lake Superior for his hero and to the *Kalevala*, an old Finnish epic, for the form of his poem.

CHIPPEWA MEDICINE MAN

ALEX POSEY, SOMETIMES CALLED MA-MUS-QUASH.

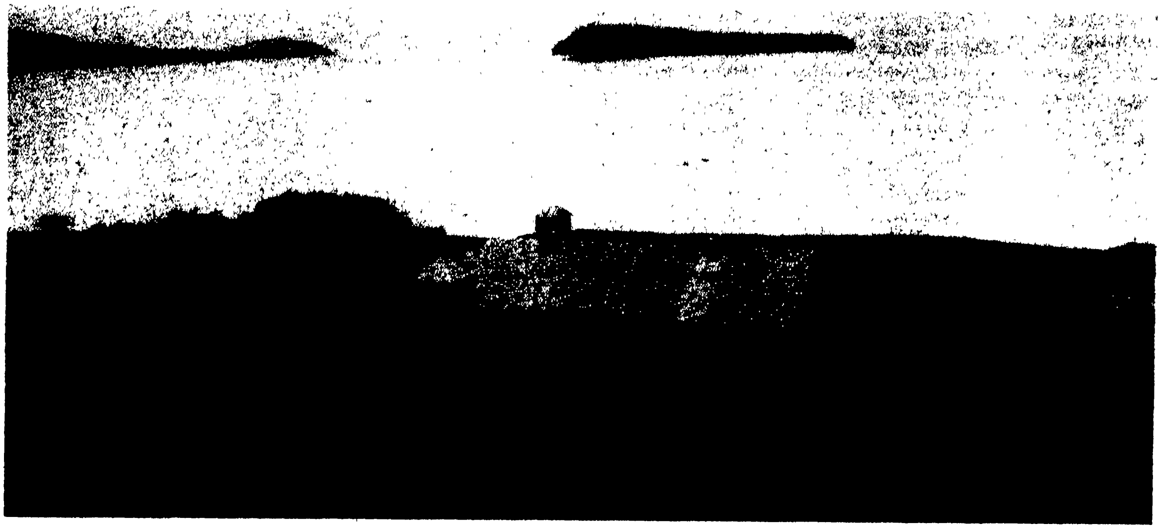
MIKE FLATT

A HEREDITARY CHIEF OF THE CHIPPEWAS. HIS LONELY CABIN IS JUST ABOVE THE PRESENT VILLAGE OF GRAND PORTAGE.

Not far from Grand Portage, at a spot on Hat Point overlooking Wauswaugoning Bay, stands the celebrated Witches' Tree, on a rock pedestal rising from Lake Superior (*Gitchi Gummi*). Indian legend relates that this old gnarled cedar is inhabited by an evil spirit having the form of a dark-brown, eaglelike bird. The Indians avoided it as a symbol of bad luck. Today, this noble old tree, which probably antedates the earliest explorations in the region (it is believed to be four hundred years old), stands with its roots firmly embedded in cracks in the rock, like a sentinel breasting the strong winds of Lake Superior.

History tells us that the fierce Sioux Indians first occupied the forests of Minnesota and that they were driven out by the Chippewas during long generations of warfare. The Sioux then became Plains Indians, and the Chippewas continued to be forest dwellers. The Grand Portage and the border lakes area to the west were the sites of many battles between the Sioux and the Chippewas.

One of the remarkable landmarks of this region is Picture Rock, overlooking Crooked Lake on the Voyageurs' Highway. This rock was described by Alexander Mackenzie, the celebrated explorer and trader of the North West Company, who passed along the Grand Portage route about one hundred and sixty years ago. Its smooth face was split and cracked in different parts, and into one of its horizontal chasms, he remarks, "A great number of arrows have been shot, which is said to have been done by a war-party of Sioux who had done mischief in the country and left their weapons as a warning to the Chebois [Chippewa]."



RECONSTRUCTED STOCKADE AND HEADQUARTERS OF THE NORTH WEST COMPANY

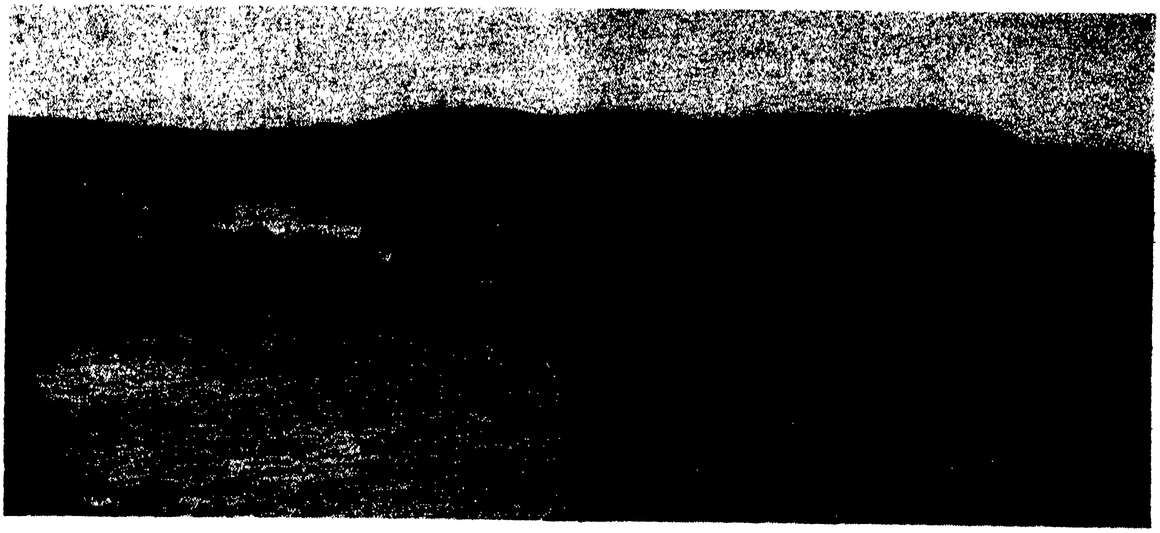
GRAND PORTAGE ISLAND AND HAT POINT IN THE DISTANCE.

The Grand Portage was used by the Indians for generations before the advent of the white man. It was the shortest distance between Lake Superior and the Great Lakes region and the far Northwest. The Indians called the trail *Kitchi Onigum*, meaning "Great Portage." The French, the first white men to pass over it, called it the "Grand Portage" or "Long Carrying Place," because of its length (8–9 miles). The trail begins at the water's edge on Lake Superior, where the fort and stockade of the North West Company were located, and winds up a long slope back of the village of Grand Portage to the cabin of Mike Flatt, the Chief of the Chippewas. This interesting old Indian possesses medals, uniforms, and a British flag presented to his ancestors.

From Mike's lonely cabin the trail leads through the forest to Fort Charlotte on Pigeon River. Ruins are all that remain of this famous rendezvous used by Indians, fur traders, and *voyageurs* during the French and British regimes. From Fort Charlotte the route, appropriately called the "Voyageurs' Highway," leads westward over the chain of lakes that includes Gunflint Lake, Lake Saganaga,

VILLAGE OF GRAND PORTAGE TODAY

THE GRAND PORTAGE TRAIL PASSES THROUGH THE FORESTS IN THE BACKGROUND.



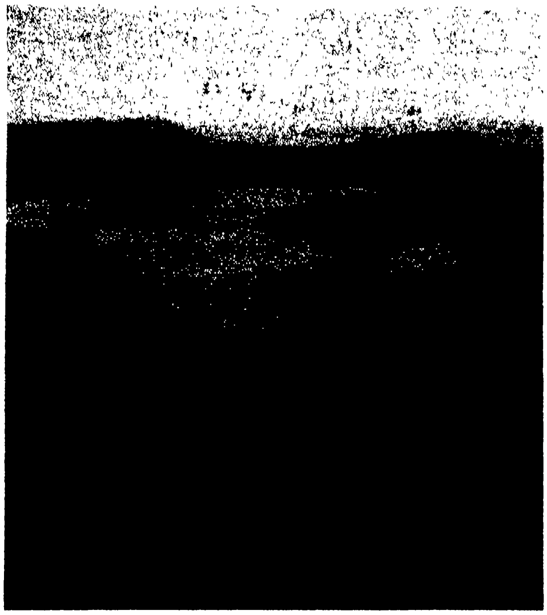
Lac-la-Croix, Rainy Lake, and Lake of the Woods to Lake Winnipeg, from which the *voyageurs* scattered far and wide to their respective trading posts—to Hudson Bay, Assiniboine, Saskatchewan, Peace River, Lake Athabaska, and even to Great Slave Lake, Oregon, and the Pacific Coast.

Grand Portage was the first town and trading post in Minnesota, its activities preceding the earliest white settlement in that state by two generations. As early as 1767 it was an important rendezvous and trading post, and at the time the Declaration of Independence was signed it was a busy emporium, the center of the Northwest fur trade.

Over the Grand Portage trail passed many celebrated characters of the early period of exploration. First came the Chippewas, Cree, and Sioux, then the French, English, Scotch, Yankees, and *voyageurs*. From the far-flung reaches of the great Northwest wilderness, as far away as the Pacific Ocean, Mackenzie River, Hudson Bay, and the Canadian Rockies, came the furs to the great North West Company. The Northwesters were, in fact, the men who explored the northern two thirds of the continent. They traced routes to the Arctic and the Pacific. They discovered the passes through the Canadian Rockies. Storms, hostile Indians, conspiracy, violence, murder, and illness took their toll of these adventurous explorers and fur traders.

Radisson and Groseillers, in 1665, were probably the first white men to know of the Grand Portage. Other great explorers who passed over it were Sieur de la Verendrye (1791), the French explorer, and Alexander Mackenzie (1789), the fur trader of the North West Company who discovered the Mackenzie River and reached the Pacific Ocean in his search for a northwest passage. He was the first white man to cross the continent in northern latitudes. David Thompson, astronomer, explorer, and trader, said by competent authorities to have been the greatest practical land geographer the world has ever known, arrived at Grand Portage in 1797. The North West Company employed him to follow the 49th parallel and determine the location of the company's trading posts. This intelligent man mapped a large area in the northwest wilderness under difficult conditions. In 1822 he returned to Grand Portage to serve as the British member of the International Boundary Commission.

Dr. John Bigsby, a member of the David Thompson party, served as an artist and physician. He gave us our first authentic pictures of the border lakes, and his sketches of the Indians along the canoe route are a valuable contribution to historical records. He was secretary of the International Boundary Commission.



McFARLAND LAKE

SHOWING THE LOW RIDGES WITH STEEP SCARPS TO THE NORTH AND GENTLE BACK SLOPES TOWARD LAKE SUPERIOR.

The celebrated North West Fur Company was organized in Montreal in 1784. It dominated the shores of Lake Superior with relentless severity, expelling all other traders; its men were "Lords of the Lake." The story of the North West Company is one marked by despotism and immorality, as well as by adventure and courage. The traders debauched the natives and became immensely wealthy from the profits derived from the rich cargoes of furs. They monopolized the fabulous fur trade of half a continent.

The X Y Company was organized by wealthy men who had been excluded from the North West Company. The jealousy, violence, and murder that followed were finally ended when the two companies merged. The North West Company also warred with the Hudson's Bay Company, and the same violence and bloodshed marked their commercial competition. They destroyed one another's forts and trading posts and shot one another's agents. This bitter feud was carried to the courts and to Parliament and was finally ended by a compromise and consolidation in 1821.

The end of the period of French domination in the Grand Portage and border lake region came with the loss of Canada in 1760 to the British. Up to the time of the French and Indian wars, traders pushed to great distances into the apparently boundless Northwest. In 1761 and 1762 British traders arrived at Grand Portage, only to find the Indians very hostile, since they did not welcome the British as substitutes for the French. The next year Pontiac's Conspiracy and the massacre of the British garrison at Mackinac caused the cessation of British trade in the Northwest until 1765. From then on, however, the English dominated the Grand Portage and the boundary lakes region until 1796, when the post at Mackinac was occupied by American troops. It was rumored that at Grand Portage revenue agents would collect duty on British goods entering the United States there. The North West Company found another route to the interior by way of Kaministiquia River and erected new headquarters at Fort William at its mouth. The X Y Company remained at Grand Portage until it merged in 1804 with the North West Company. The Grand Portage trail was then abandoned.

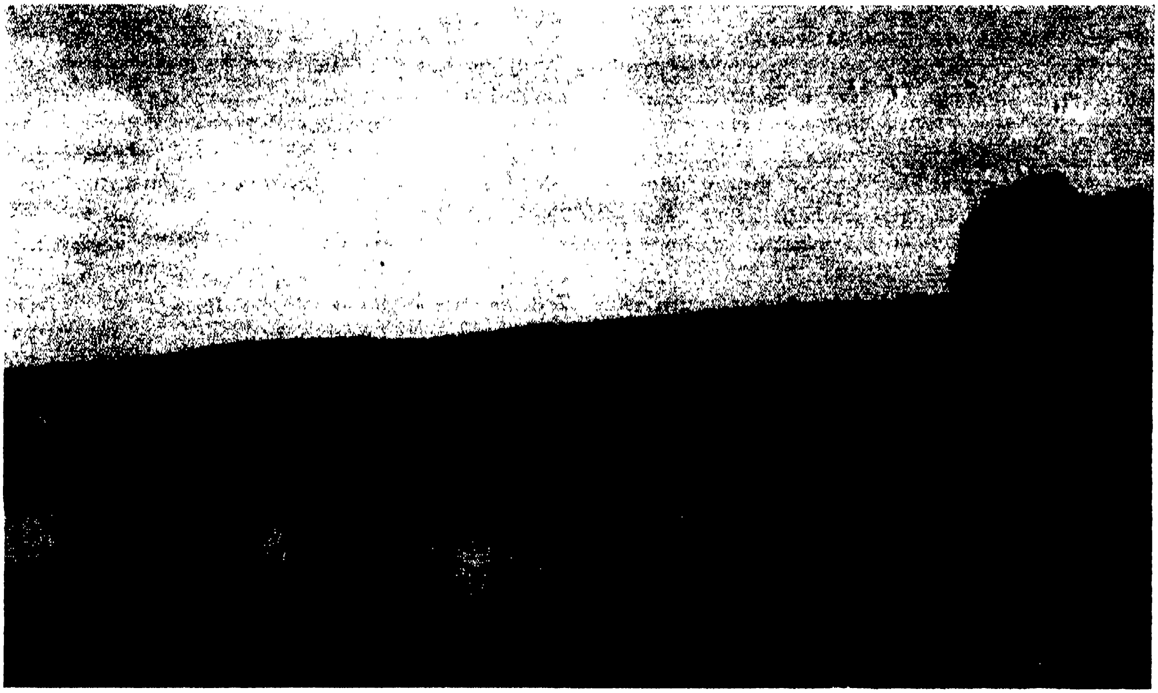
THE fur trade introduced the white man's luxuries to the Indians. Brandy, rum, and wine constituted the wet goods; guns and ammunition came next in importance, then dry goods, hardware, and trinkets. Beaver skins were the most important of the furs; in fact, the fur trade was sometimes called the "beaver trade." The barons who controlled the North West Company lived in splendor at Montreal and Sault Ste Marie. At Grand Portage, their western headquarters, the furs were gathered, baled, and shipped to Montreal, at that time the great fur market of the continent. The workers in the fur trade at Grand Portage were nearly all Canadian, French, or French half-breeds. Those of French extraction were known as *coureurs de bois* ("rangers of the woods") or *voyageurs* ("travelers" or "canoe men"). They worked for a body of Scotch superintendents and owners, businessmen who organized the fur trade, sorted furs, and purchased goods. These courageous and industrious men constituted the North West Company.

Throughout the months of July and August, Grand Portage was a scene of great activity. Goods for trade came in fleets of canoes from Montreal, accompanied by officials of the North West Company, who met other traders coming from the northwest wilderness. The annual meeting was held, and arrangements and agreements made for the following winter. Over hundreds of lakes and portages, through wild rapids and cataracts, came the birchbark canoes loaded with furs. In the canoe yards of the North West Company as many as 75 of these canoes were constructed annually for the fur trade, since they were the only practical mode of transportation for that purpose.

As many as 3,000 men—*voyageurs,* Indians, officials, and traders—congregated in and around the fort and stockade at Grand Portage. Furs and supplies were interchanged, pelts were sorted and baled, and accounts were settled. Then there was a grand frolic: gallons of rum were issued, and the strains of the violin and the skirl of the bagpipe were heard from morning till night. In the great banquet hall, 60 feet long, tables groaned with the weight of game and fish. The dancing lasted till morning. At least 1,500 people of both sexes assembled at the celebrations, and 100 large and 200 small canoes were moored in the navy yard. At the banquets, financial tycoons from Montreal ate and drank side by side with the half-breeds, *voyageurs,* and the untamed Crees and Chippewas from the far Northwest. Such were the scenes at Grand Portage at the time of the signing of the Declaration of Independence.

Paulie Swan

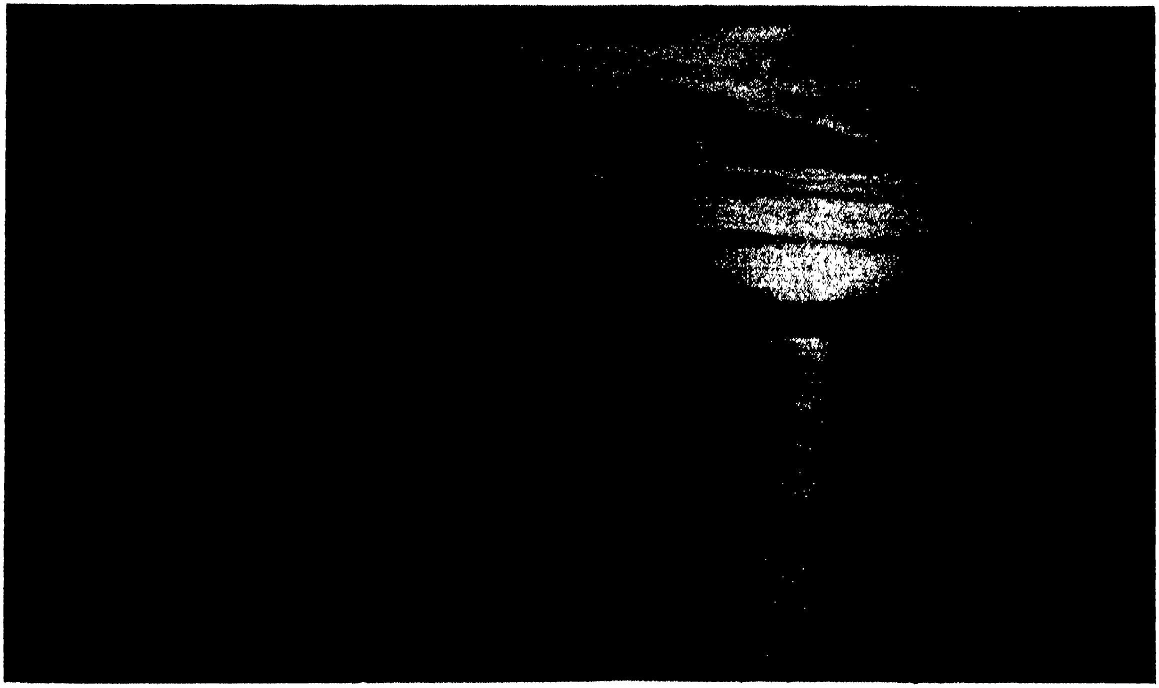
ARTIST'S SKETCH OF A *VOYAGEUR*



HUNGRY JACK LAKE

ON THE VOYAGEURS' HIGHWAY TO THE GREAT NORTHWEST.

During July and August the North West Company employed as many as 500 men at Grand Portage. Supplies and furs had to be transported between Fort Charlotte and the eastern end of the trail. The round trip of almost 17 miles was accomplished in about six hours. The size of the burdens



LOON LAKE

ALSO ON THE VOYAGEURS' HIGHWAY.

carried across the portage by the *voyageurs* is almost unbelievable. Two, and sometimes three, bales of furs were carried on each trip, and an equal weight in supplies on the return journey. During wet weather there were places where the men waded knee-deep in the slippery mud. In September, the traders shouldered their canoes and, by way of the Grand Portage and the great chain of interior lakes to the west, scattered throughout the Northwest for another winter.

The most interesting characters of the fur-trading days were the *coureurs de bois*. These eighteenth-century bush rangers forsook their homes and families and took to the woods to engage in the beaver trade. The movement became so general that whole communities were virtually deprived of their male population, and much destitution resulted. Adventurous youths, and even soldiers of the regular garrison, took to the forest and lived the free life of the Indians, trading with them and often marrying among them. Always loyal to the French, their alliances with the Indians gave them great ascendancy over the savages and kept the natives friendly to the French.

The *voyageurs* were the pioneers of commerce on Lake Superior. Mostly French or *Brûlés* ("half-breeds"), these swarthy men were truly the heroes of the canoe. A better canoe man than the Indian, the *voyageur* handled a canoe in the rapids and fast water with phenomenal skill. His ability to live in the wilderness, make canoes, erect forts, manage sled dogs, and procure furs made him the mainstay of the fur trade.

For generations, the canoe songs, which were folk songs inherited by the French Canadians from the French of the Loire Valley, were heard along the shores of Lake Superior. It was a great sight in those days to see a fleet of canoes with the oars moving in cadence with one of the rollicking folk songs. The *voyageurs* were polite, jocular fellows who worked hard and took orders well, but who nevertheless assumed little responsibility and, on the whole, took life easily. Fickle, reckless, and immoral though they were, they were liked by the Indians, especially the Indian women.

Pemmican was the main item of food for the fur trader. This concentrated food was made of dried buffalo meat, into which were pounded dried berries and marrow fat; the whole was packed in a rawhide bag. It was put up without salt so it could be eaten without producing thirst. Pemmican, made at prairie forts in the buffalo country of western Canada and shipped by canoe, made Grand Portage and the fur trade possible.

In his mode of dress the *voyageur* was picturesque; he loved vivid colors. His cap was of bright-red woolen material; his shirt was short, made either of buckskin or woolen cloth. He wore a pair of deerskin leggings, which reached from his ankles to a little above the knee and were held up by thongs attached to a belt about his waist. He wore a breechcloth like that of the Indians, and on his feet he wore moccasins without stockings. His thighs were bare. His coat, with hood attached, was of some gaudy color—often blue. To complete the costume, he tied a brightly colored sash about his waist. A beaded or otherwise ornately decorated bag hung from the belt in which he carried his pipe and tobacco and other objects.

These men performed the almost incredible feat of crossing and recrossing the continent in birch-bark canoes in a single season. They would start in a canoe from the Columbia River on the Pacific Coast in April and, by threading their way through rivers and lakes over hundreds of portages, shooting the rapids, portaging over mountains without halt in fair weather or foul, sleeping only four hours in twenty-four, they would reach Grand Portage on Lake Superior by the first of July with the regularity of a steamboat. Returning across the continent with equal precision, they would arrive at Fort George at the mouth of the Columbia by the twentieth of October.

Today, only the lapping of the waves on the shores of the beautiful lakes and the whisper of the pines and birches break the silence of the wilderness where once rang the shouts and gay songs of the fur traders. All that remains to remind us of the exploits of these sturdy men of the Northwest are the traces of their trails and portages and the ruins of their forts and stockades along the Voyageurs' Highway.

SCIENTIFIC AND TECHNICAL ENLISTED MEN IN THE ARMY

MARSH W. WHITE

Professor White (Ph.D., Penn State, 1926) has been teaching at his alma mater since 1927 and has been professor of physics there since 1942. During 1944–45 he served as Expert Consultant in the War Department and, in 1945–46, to the Secretary of War. He is now Consultant on Scientific Manpower, General Staff, U. S. Army. Author of several books, his latest is College Technical Physics, *in which he collaborated with R. L. Weber and K. V. Manning (McGraw-Hill, 1947).*

THIS article is a brief report of the activities of the Technical Detachment, an organization established during the latter part of the recent war by the War Department for the preferential selection, assignment, and utilization of enlisted men who were professionally qualified for scientific and technical duties. Data are presented showing the ages, marital status, fields of specialization, academic degrees, work prior to induction, and the branches of the service to which assignment was made.

During the spring and summer of 1944 the needs of the armed services for additional personnel resulted in the induction of an increasingly large number of professionally qualified scientific and technical personnel, many of whom had previously been given a deferred status by Selective Service. This situation became a matter of great concern to both civilian and military groups charged with the responsibility for research and development of military devices and techniques. After consideration of a number of possible solutions to the problems presented by this situation, the War Department on August 30, 1944, directed the establishment of a Technical Detachment. Responsibility for the procedures for the organization and administration of the Detachment was placed in the New Developments Division, War Department Special Staff, Brigadier General William S. Borden, Director. On May 1, 1946, the Research and Development Division, War Department General Staff, with Major General H. S. Aurand as Director, succeeded the New Developments Division. At present, this unit is designated the Research and Development Group, Logistics Division, General Staff, U. S. Army.

The major objective of the Technical Detachment was to ensure the directed assignment of professionally qualified scientific and technical personnel upon their induction into the Army or to reassign men already in the service when it was learned that they were not being utilized in their technical capacities as fully as they might be. By locating places in the research and development installations having a need for technically qualified enlisted men, the Technical Detachment acted to conserve the skills of such personnel and made possible their exploitation in the development, testing, and application of the newest devices under development by the Army. The Technical Detachment was admittedly an experimental approach to the problems involved in the individually directed assignments of scientists and technical personnel with rare skills inducted into the Army. It was hoped through the functioning of the Technical Detachment to ensure their effective utilization in the Zone of Interior laboratory installations and on teams for the introduction of newly developed devices into the theaters of operations.

In the first few months following the establishment of the Technical Detachment, assignments were limited to men who, as civilians, had participated in the development of special devices for military use. Later this limitation was eased to provide for the inclusion of those who, by education and experience, were peculiarly qualified for such work, even though they had never had actual research experience in these fields. Standards of minimum qualifications for assignment to the Technical Detachment were established. These varied considerably, depending upon the needs of the services and the relative scarcity of various professional groups. For example, physicists were among the scarcest and the most desired, whereas certain kinds of chemists were relatively numerous and at the same time not in great demand. Some types of engineers qualified for research and development, particularly in electronics, were especially needed. The Technical Detachment did not concern itself with the directed assignment of conventional production engineers or nonprofessional technicians.

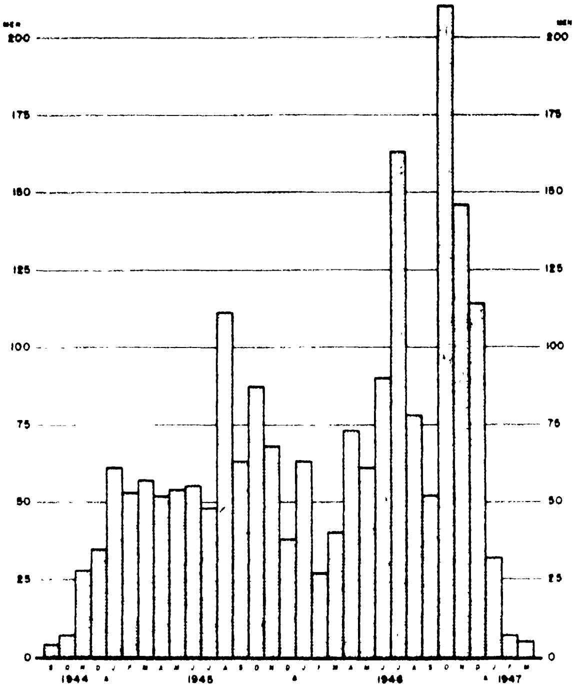
During the early period of the Technical Detachment, candidates for consideration were obtained from information received from the National Roster of Scientific and Specialized Personnel, the Office of Scientific Research and Development, the Office of Scientific Personnel of the National Research Council, and from individuals, research organizations, industries, and collegiate institutions. Arrangements later were made whereby Army-wide screening of all inductees was conducted at reception centers in order to identify men of the desired technical qualifications. Enlisted men thus located were earmarked as Technical Detachment potentials while their qualifications were carefully examined during the period of their basic military training. For each such man the data obtained from Army personnel records, the National Roster files, and other sources were made into a complete personnel file. Then, in view of the current needs of Army research and development installations, a directed assignment was ordered for all men found to be properly qualified. These assignments were reviewed and approved by a high-level Committee on the Assignment of Scientific and Technical Personnel in the Army. This Committee included representatives from the interested staff divisions of the War Department and from the personnel sections of the Army Service Forces, Army Air Forces, and Army Ground Forces. The Committee had complete authority to assign and transfer any qualified enlisted man in the Army.

It was recognized that the problem of proper utilization after assignment was a difficult and continuing one. To minimize improper placement originally, no man was assigned unless a specific request for his type of skill had been received. To assist in ensuring that the men would not be diverted to nontechnical duties after their original assignments, the orders transferring all men assigned by the Technical Detachment contained the following directive:

> Subject enlisted man has been selected because of his education and qualifications for scientific and technical work in the Army. He has been classified as a critically needed specialist and will be assigned only to duties which will make full utilization of these qualifications.

In spite of efforts to ensure satisfactory utilization of men assigned through the Technical Detachment procedures, it is probable that after the completion of their initial tasks many of the duties given to some of the men were not such as to require highly technical competence. Some spot checks were made to determine the degree of effectiveness with which the scientific skills of these men were being exploited. The men themselves frequently notified the New Developments Division concerning their work, particularly when their duties did not seem to be of a sufficiently technical nature. Many of the Technical Detachment men were assigned to an installation on a 60- to 90-day temporary-duty basis. At the end of these periods, the commanding officers had to justify their requests for an extension of the assignments, thus securing a review of duties to which the men had been assigned. In general, it is believed that most of the men were satisfactorily utilized in their technical specialties, but some undoubtedly were eventually diverted from research and development work to less scientific duties.

TECHNICAL DETACHMENT ASSIGNMENTS
SEPTEMBER 1944 THROUGH MARCH 1947.



The Technical Detachment began operations in September 1944 and was discontinued in March 1946, when the induction of scientists under Selective Service ceased. During this period, the qualifications of some 2,500 men were critically examined and directed assignments were made for about 1,800 of this group. Those assigned by the Technical Detachment procedures included 1,056 single men (63 percent) and 614 married men (37 percent). In age, the men ranged from twenty to thirty. Of 1,790 men, 19 percent were between twenty and twenty-three; 17 percent were twenty-four; 17 percent, twenty-five; and 47 percent were between twenty-six and thirty.

The professional occupations of the Technical

Detachment assignees prior to induction are shown in Table 1. In civilian life, 2 percent had been engaged in teaching; 9 percent in research of various kinds; 59 percent in industry (in both production and research); and 7 percent had been students.

TABLE 1

Field of Specialization	No. of Men	Percentage
Chemical engineering	326	18
Biological chemistry	41	2
Organic chemistry	203	11
Physical chemistry	33	2
Inorganic chemistry	92	5
Electrical engineering	171	10
Aeronautical engineering	166	9
Civil engineering	31	2
Mechanical engineering	516	29
Metallurgy	28	2
Physics	89	5
Mathematics	35	2
Zoological sciences	17	1
Miscellaneous	36	2
Total	1,784	100

Out of 1,607 men, 3 percent held the Ph.D. degree; 8 percent, the M.S.; 10 percent, the B.A.; and 79 percent, the B.S. Table 2 shows the numbers of men assigned by the Technical Detachment to the various arms and services in the Army.

The lessons learned from the operation of the Technical Detachment during World War II should provide a partial basis for planning for the mobilization of scientists in the event of another national emergency. The Research and Development Group of the General Staff, United States Army, is cooperating with the Navy, Air Force, and a number of civilian organizations in preliminary work which looks forward to the development of an over-all plan for the effective allocation and utilization of all scientists before and during periods of war. Such a comprehensive plan contemplates the establishment of legislative authority and administrative mechanisms that will be essential in the implementation of the plan. The plan will probably provide that the bulk of scientists qualified for research and development activities shall remain in civilian status for work in OSRD-type organizations and in the laboratories of universities, industries, and the armed services. For the relatively small group that will be inducted into the Army an organization molded along the lines of the Technical Detachment may be included in the plans for mobilization on M-Day and later.

TABLE 2

Assignment	No. of Men	Percentage
Signal Corps	106	6
Ordnance	185	10
Chemical Corps	280	16
Engineers	130	7
Quartermaster	101	6
Medical	90	5
Army Air Forces	728	40
Army Ground Forces	100	6
Others (Manhattan District, etc.)	73	4
Total	1,793	100

IMPOTENCE PRINCIPLES IN MODERN PHYSICS

R. B. LINDSAY

Professor Lindsay (Ph.D., M.I.T., 1924) was an American-Scandinavian Fellow in Denmark in 1922–23. He was on the Yale faculty from 1923 to 1930, when he became associate professor of theoretical physics at Brown University. For the past fourteen years he has been chairman of the Department of Physics at Brown.

THE interest of the distinguished British applied mathematician Sir Edmund Whittaker in problems of physical methodology is too well known to require much comment. His essays in *Nature* and in the *Proceedings of the Royal Society of Edinburgh* are always well thought out, historically well documented, and very stimulating. Would that more mathematically minded people were so enlightening!

In a presidential address to the Royal Society of Edinburgh in October 1941, Whittaker discussed "Some Disputed Questions in the Philosophy of the Physical Sciences" and in the course of his address paid particular attention to those postulates or principles in physics which state categorically our inability to carry out certain processes that are not intuitively impossible. For such statements he introduced the name "postulates of impotence." They might just as well be called "postulates of renunciation." We are all familiar with examples.

Thus, there is the second law of thermodynamics, which tells us in one of its several modes of formulation that it is impossible for any self-acting machine continuously to convey heat from one body to another of higher temperature. Or, again, it is impossible for energy transfer between material systems to take place at a velocity greater than that of light in free space, a postulate closely connected with the theory of relativity. In quantum mechanics we learn from the so-called principle of indeterminacy that it is impossible to make simultaneous measurements of the position and momentum of a particle with the same degree of precision. And so we might go on with the list, even including one or two ancient renunciations, such as that it is impossible to raise water more than 34 feet by means of a lift pump, or that it is impossible to raise oneself by one's own bootstraps!

Such expressions of human impotence both in the realm of the practical and that of the abstract have a peculiar fascination, and the theoretical structures in which they have a place have always seemed to possess a mystery not shared by others in which the postulates have a more affirmative ring. Inevitably, a renunciatory statement in a science like physics, a statement that "you cannot do thus and so," provides a challenge which naturally provokes effort to show that after all it *can* be done. We all know the history of this in the case of the second law of thermodynamics and how many persons have tried to disprove the principle by direct experiment, i.e., by the construction of successful perpetual-motion machines of the second kind. From a practical standpoint, it might be felt that this has been a sheer waste of time and that this has been an unfortunate effect of the statement of the second law in the form of an impotence principle. On second thought, we might, however, conclude that all this essentially futile experimentation has really been a good thing, since there is always the chance that some ingenious mechanical device will come out of it. This has certainly happened in the case of perpetual motion of the first kind in the production of so-called self-winding clocks, etc. The trouble is that, swayed by their conviction of the validity of the impotence postulate, most professional scientists are disinclined to look into the mechanical aspects of so-called perpetual-motion devices, and dismiss them out of hand as idle violations of the postulate.

More important for our present consideration is the *theoretical* standing of renunciatory principles. Whittaker is sufficiently impressed with the theoretical importance of impotence postulates to visualize a future day when any field of physics can be developed along the same methodological lines as Euclid's geometry with impotence postulates as the *a priori* principles and with all the relevant physical laws obtained therefrom by syllogistic reasoning. I am not a prophet and Whittaker may be right, but I should like to examine somewhat critically his point of view from the standpoint of the history and methodology of physics.

We may as well begin with thermodynamics. It must at the outset be admitted that scientists have not been satisfied with the statement of the principles of thermodynamics in impotence form. This is well illustrated indeed by the history of the first law. This, of course, can be expressed in renunciatory form as the impossibility of producing perpetual motion of the first kind, i.e., the impossibility of constructing a machine that will do work continuously without receiving energy from an external source. Actually, it has been found preferable to state this in *positive* form, namely, that in all the interactions of physical systems the total energy in the universe remains constant. When we reflect that the definition of energy in theoretical physics is based on the existence of a particular invariant of the motion of a dynamical system resulting from a first integration of the equations of motion, and that strictly the energy does not exist when the integration cannot be carried out—i.e., when there is no potential function—we are faced here with a rather paradoxical situation. In no actual terrestrial motion does it prove possible in all strictness to find a potential function and assign an energy; hence, one might suppose that this fact would serve as an excellent principle of impotence. But no such attitude has been adopted; rather, the idea of energy has seemed of such utility and importance that its definition has been extended so as to make its conservation one of the keystones of the theory of thermodynamics. In other words, the difficulty just mentioned has not led us to renounce the use of the energy principle; it has only encouraged us to expand it meaning and extend its significance.

The history of the second law also indicates a more recent desire to get away from the renunciatory element contained in its early formulation. On the purely formal level, the introduction of the entropy concept by Clausius was an attempt to give the principle an affirmative ring and enable quantitative conclusions to be drawn from it. Even the elementary student must have observed that the only way in which one can draw positive conclusions from an impotence statement like the second law in its historical form is by means of the principle of contradiction. This has long been a favorite method of proof of theorems in pure mathematics, but it has never found such favor among physicists. Even some mathematicians now regard it with a jaundiced eye. We may not be impressed with the possible utility of multiple-valued logics in the development of physical theories, but I believe most of us would prefer to avoid the chains of the excluded middle class as far as possible.

As a matter of history, the introduction of entropy in an attempt to replace the impotence implication of the second law by what might be called a *competence* implication has not been deemed sufficient. No physicist nowadays feels satisfied with the principle of increasing entropy except in terms of the statistical point of view, with its emphasis on the probable character of calculated results. On this view, we no longer renounce forever the possibility of finding the uncompensated increase in free energy of an isolated system, but merely look upon this occurrence as extremely unlikely in systems of any degree of complexity. Why do we find this point of view more satisfying? There are doubtless many reasons, but one of them certainly is the removal of the drastic prohibition even to think of the possibility of a violation of the second law in our universe. It makes it possible for us to account for the obvious violations presented by the Brownian motion and many other fluctuation phenomena without subterfuges based on the contrast of microscopic and macroscopic. At the same time it provides a healthy deterrent to any philosopher or theologian who might be inclined to erect a far-reaching metaphysical demonstration (e.g., the existence of God) on the basis of the definitive renunciatory character of the second law. This is, of course, wholly aside from the value of the statistical method in providing a fundamental basis for the whole of thermodynamics, in which thermodynamic principles become deductions rather than independent postulates. Moreover, this also leaves out of consideration the power of the statistical method to answer questions about the specific behavior of matter (e.g., equations of state and problems of energy transport), which thermodynamics is quite powerless to tackle.

THE view that I am propounding here may then be set forth briefly as follows: principles of impotence have a certain value in one stage of the development of physical thought; they fascinate and they provoke thought as to their fundamental meaning. But we should never be contented with them as the final expression of the content of any physical theory. Too firm an adherence to them and too close a preoccupation with them may actually retard the progress of physics.

In any consideration of the reasonableness of this point of view, we are inevitably faced with the necessity of examining what is probably the most famous of all principles of renunciation, namely, the indeterminacy principle of Heisenberg and the associated but more general complementarity principle of Bohr. Both have achieved prominence in recent years, not merely among physicists but among philosophers and the educated laity.

The history of the indeterminacy principle in quantum theory is an interesting one. It will be recalled that, when in 1925 Heisenberg founded his brand of quantum mechanics, the prevailing mood in the Copenhagen school of Bohr was one of pessimism about the possibility of any further success in applying the Bohr quantum postulates to mechanical atomic models. Though the broad outlines of the theory of polyelectronic atoms seemed well founded, the inability to attain precision in calculating the ionization potential of even such a simple atom as helium was disheartening. And, of course, there were other serious difficulties connected with optical dispersion and collision phenomena. It was then that Heisenberg decided to forego the further use of mechanical or space-time picture models in which electrons, conceived as actual charged particles, were assumed to move in definite orbits. His plan was to build an abstract mathematical formalism in which the only physical quantities entering are directly observable. Relevant examples are the frequencies and intensities of spectral lines. The resulting so-called *matrix mechanics* was remarkably successful in its predictions, even though many physicists called it formal, abstract, and unpicturesque and therefore not understandable by experimentalists accustomed to thinking in terms of pictures. To what extent Heisenberg was affected by these complaints I do not know, but it is a fact that in a famous paper in the *Zs. f. Physik* in 1927 he proceeded to inject some *Anschaulichkeit* into quantum mechanics: the indeterminacy principle was the result.

Why have we reviewed this well-known historical background? Simply to emphasize that in the straightforward use of quantum mechanics there is no need for the indeterminacy principle. Its status as a principle of renunciation has thus a different basis from that of the second law of thermodynamics. It was not discovered as a fundamental restriction on physical theorizing in the atomic domain from which the theorems of quantum mechanics flow as do those of thermodynamics from the second law. It is not only unnecessary for the application of quantum mechanics to the problems for which the theory was designed, but may actually be said to have arisen out of a violation of the very principles on which the quantum mechanics was based, namely, the restriction to observable quantities. This is not to say that the principle has been useless, for it has certainly served to attract attention to problems in the methodology of quantum mechanics. At the same time it has misled many.

Thus the renunciatory element in the indeterminacy principle has been much exaggerated in popular accounts. Possibly this is due to the fact that its usual presentation stresses the disturbance produced in an atomic system by the attempt to measure any quantity associated with it. This in turn is closely related to the disturbing interaction of any physical measuring device with the system under measurement. But this is misleading. Classical physics has never questioned the necessity of accounting for the disturbance produced by a measuring instrument on the system being measured. Thus, the introduction of a voltmeter across a portion of a circuit changes the circuit, and its reading must be "corrected" if the drop in potential existing before the insertion of the meter is to be obtained. From another point of view, the operation of inserting the meter, reading it, and making the corrections, taken together, constitute what we mean by the concept *potential difference*, though we later generalize this by the introduction of more general abstract concepts. The same situation is met throughout physics. It is our hope that the assignment of numbers to these concepts can be carried out consistently enough to enable us to test unambiguously in the laboratory the deductions of theory. The well-known fact that laboratory measurements are always given in terms of sets of numbers to which some sort of statistical analysis must be applied in order to obtain a unique answer could lead logically to an impotence postulate stating that precise physical measurements are impossible. It has not been thought worth while to introduce such an utter renunciation.

Actually, the indeterminacy principle considered from the standpoint of disturbance has been applied largely to mental experiments and measurements and not to those performed in the laboratory. This is not to deny that it has not been useful in some cases. Thus it can associate the mean breadth of spectral lines with the mean lifetime of an atomic stationary state. But it is well to point out that this result has nothing essentially to do with the renunciation of precision of measurement due to the disturbance produced by the measuring instrument. The indeterminacy principle in its useful form is simply a relation between the standard deviations of quantum mechanical canonically conjugate quantities as derived in straightforward fashion from the postulates of quantum mechanics. In this there is certainly no more expression of impotence than there is in the well-known statement that in a finite wave train the shorter the train the greater is the spread in frequency required for its adequate representation by a Fourier integral. As a matter of fact, it was this type of representation which DeBroglie used in his foundation of wave mechanics. It is a definitely affirmative approach with no element of renunciation involved in it—unless we wish to take the stand that all physical theorizing is renunciatory.

And so it seems to me that, though the indeterminacy principle (and here I include also the wider generalization, the complementarity principle of Bohr) has a certain theoretical fascination, its contribution to the future of physics as a principle of impotence is not sufficiently profitable to justify the attention that has been paid to it. It is naturally the kind of physical statement that appeals to many philosophers who in general are not in a position to study minutely its precise meaning in physics and hence are tempted to erect on it philosophical generalizations bearing little justifiable relation to its physical meaning. Many such philosophical misinterpretations have been pointed out and discussed by Philipp Frank and need not be further emphasized here. We may merely mention the numerous attempts to use the principle to establish the freedom of the will as a philosophical principle. The attempted application of complementarity to biological problems and the nature of life must be regarded, in spite of Bohr's ingenuity, as of highly questionable scientific value.

It is only natural that Whittaker should have numbered among impotence postulates the Einstein principle of special relativity in the form that "it is impossible to detect uniform translatory motion with respect to the primary inertial system by any means whatever." Unlike the indeterminacy principle, the principle of special relativity in its impotence form would appear to have an unimpeachable historical basis in the famous Michelson-Morley experiment, with its attempt to measure motion of the earth relative to the luminiferous ether and its so-called null result. Most popular presentations of the theory stress this aspect. But when we examine the situation more closely, the issue is not so clear-cut.

In the first place, the experiment even as originally performed never gave a completely null result, and I presume most physicists are familiar with the long-continued, often-repeated, and admittedly careful experiments of the late D. C. Miller. These also led to a positive result, though by no means as great as that expected on the original Maxwell prediction. No one questions the historical importance of the failure of the experiment to give the full predicted result in suggesting to Einstein the postulate of special relativity. But it must also be recognized that the great contributions of the special theory to physics in the mass-energy relation, etc., have long since rendered it impregnable against attack based on an eventual positive result of experiments like those of Michelson, Morley, and Miller. By the same token the impotence or renunciatory aspect of relativity has become of relatively minor significance. The really important contributions of relativity have come as positive deductions from the Lorentz-Einstein transformation equations, and the test of the theory is in the experimental verification of these deductions. And it seems to me that it is ultimately less misleading to phrase the fundamental postulate of special relativity in the positive form—"the laws of physics have the same form in all inertial systems"—rather than in the impotence form to which this indeed logically leads. On this point of view, an experimental violation of the impotence postulate would not be interpreted as a complete overthrow of the whole theoretical structure of relativity, but merely as an indication that we were not dealing with inertial systems. Methodologically, I cannot help thinking that this is of considerable importance.

To summarize briefly the point of view maintained here, though principles of impotence or renunciation are interesting milestones in the development of physical thought, they are of too negative a character to serve as true signposts for future theories. It seems necessary to reiterate that physics will not flourish under the shibboleth *Nequimus* any more successfully than under the old discarded battle cry of the du Bois-Reymonds: *Ignorabimus.*

THE PARADOXICAL RIVERS OF THE GREAT PLAINS*

OREN F. EVANS

Professor of geology at the University of Oklahoma. Dr. Evans (Ph.D., Michigan, 1940) has been at Oklahoma since 1920. His summers he devotes to physiographic research and in other spare time writes both technical and semipopular scientific articles. In October 1945 we published his "Scientific Beachcombing."

IN THE early eighties a waggish New Yorker, recounting his experiences on a recent trip to the then frontier lands of Kansas and Oklahoma, described the region as a "land of more rivers and less water, more cows and less milk, and where [he] could see farther and see less than anywhere else in the world."

Today, should he repeat that journey across the Great Plains, he would find a landscape well dotted with towns and cities, and he would have no trouble in obtaining cream for his morning coffee—at a price. But he would find the rivers the same. They are now spanned by great bridges of steel and concrete, but they are still the same treacherous watercourses, full of quicksands and subject to sudden floods, that they were in the days when they took their toll of the wagon trains on the Santa Fe Trail. In the dry season, the channels of drifting sand lie flat and dreary under the brazen sky, their tediousness only accented by the occasional trickles of lukewarm water meandering between sluggish pools. But, if and when the rains come, these same sand-choked streams are changed in a few hours to raging torrents, and their flood waters fill the broad valleys from side to side, sometimes even turning the adjoining uplands into miry bogs.

To a casual observer, the Great Plains rivers seem to defy all known laws of normal stream behavior. Instead of cutting their valleys deeper, they are, throughout much of their courses, engaged in filling them by depositing more material than they take out. The beds of their tributaries, instead of being at a higher level than the main stream, often flow, over a great part of their courses, at a lower elevation. As a result, instead of the stream branches working away from the main stream into the neighboring drainage system, they are likely to turn back and commit piracy on the parent.

The main stream, instead of remaining at a nearly constant gradient between high and low stages, increases its slope rapidly as the floods rise. Again, a tributary often has a broad, flat bottom with the stream meandering through it in a deep, narrow trench. To the student of stream work, this suggests rejuvenation, but in the Great Plains streams it means nothing of the kind. In this environment, it is the perfectly normal development of a degrading watercourse. The explanation of such seeming anomalies lies in the climate, the rock structure, and the nature of the sediments that cover the Great Plains.

From the Missouri on the north to the Rio Grande on the south, the more important rivers of the Great Plains head in the Rocky Mountains or their foothills, and with a steep gradient and an abundant supply of clear water they move rapidly eastward, occasionally breaking into rippling rapids or foaming cascades. But their nature quickly changes when they leave the mountain fringes and begin their long journey across the plains.

In passing from the steep, well-watered slopes to the relatively dry, flat, sandy plains, their volume becomes less, they flow more slowly, their waters turn turbid with suspended sediment, and they no longer cut downward in their beds. For two hundred fifty miles they struggle eastward, losing water to the thirsty sands as fast as their tributaries can supply it and, in dry seasons, barely able to maintain their flow.

The annual rainfall of these High Plains averages between ten and twenty inches, is irregularly distributed, and varies greatly from year to year. Much of it comes in local torrential downpours that hurry great loads of sand down first one tributary and then another into the already overloaded channels of the main streams.

Farther east, toward the Mississippi River, the precipitation gradually increases, but not fast enough for the streams to take care of the increased loads of sediment. So, even here, they are often filling their valleys faster than the material can be carried away. When filling occurs in normal streams in humid regions, it is usually the result of a decreased gradient brought about by a general lowering of the land level. In these Great Plains streams, on the contrary, filling results from a load of sediment greater than the stream can carry.

---

* Drawings by R. L. Harris, Oklahoma Geological Survey.

In fact, for streams of their size, the rivers of the Great Plains have exceptionally steep slopes, varying from two to ten feet per mile.

These peculiar conditions are the result of an evolution that began in Cretaceous times. In middle Cretaceous an interior sea spread from the Arctic to the Gulf and reached east almost to Lake Superior. At that time there was no Mississippi Valley as we know it, and the drainage of the continental interior was very different from what it is today. Many of the streams flowed west into the Cretaceous sea, and the Mississippi was only a small stream emptying somewhere near the present city of Cairo, Illinois, into an embayment reaching north from the Gulf. It was then a very humble stream and gave little promise of developing into the huge drainage monopoly it has since become.

When the sediments that had been depositing for millions of years in the Cordilleran trough began to be squeezed up, however, the situation quickly changed. As the bottom of the trough came above the level of the sea, the whole area became a great swamp that remained for many thousands of years, during which it was covered with a succession of dense forests. These furnished the material for the great beds of peat that have since been transformed into the coal fields of the Rocky Mountains. In the beginning, the rise of the land was slow, but it became more rapid toward the end of the Cretaceous as it developed into the early Rocky Mountain uplift. This movement set up an entirely new drainage system for the interior of the continent. As the eastward slope increased, the swamp streams took on new life and their movements became more purposeful and definite as they flowed east and south toward the youthful Mississippi River and the Gulf.

At first, these streams were relatively short and their valleys shallow. But as time went on and the region to the west continued to get higher and higher, they began reaching headward toward the rising mountain axis. At the outset, the area was well watered and the streams developed normally, slowly lengthening as they cut their valleys deeper. As the surface by degrees took on this eastern tilt, the low gradient and the great volume of water, along with the level-lying rocks, was conducive to the formation of broad and relatively shallow stream valleys.

But there came a time when the rising mountains to the west began shutting off the moisture from the Pacific. The rainfall became less and less, the forests died out, and the region of the headwaters of the streams gradually took on the appearance of a desert.

By this time the streams, in their upper reaches, had cut down enough to begin to expose the Dakota sandstone that covers broad areas all along the east side of the Rockies. As more and more of it was uncovered, the tributaries rushed great quantities of sandy material into the streams and they began filling their valleys. The mountains to the west continued to rise, the climate became more arid, the rainfall more erratic, and the streams seemed to be flowing in valleys too large for them. This aspect has been accentuated with time and persists to the present day.

Another puzzling feature of the plains streams is in the peculiar relations existing between the large streams and their branches. In a normal degrading stream of the humid regions, a tributary, being younger and smaller than the main stream, runs at a higher elevation. But for a number of the older and longer tributaries of the plains streams, this relationship does not hold. At equal distances above the junction, the beds are often at a lower level than that of the main stream.

These seeming anomalies result from the way the drainage has developed. In its early history, while the rivers were still cutting their beds lower, they threw out numerous branches. Then, when the parent streams had cut down into the sandy rock beds of their upper reaches and began taking in a greater load than the water could carry away, they started to aggrade. But the tributaries, being shorter than the main streams, failed to reach into the sandy area and so had no opportunity to pick up the extra load. Instead of aggrading, they continued cutting their beds lower, as they had been doing from the first, with the result that many were soon flowing lower than the main stream. The topographic maps of the region show many such relationships.

Not far from the town of Norman, Oklahoma, Little River, a tributary of the South Canadian, is some fifty feet lower than the main stream. This is at a point some fifty miles above the junction of the two. Here one branch of Little River is cutting straight toward the South Canadian and is already within three miles of it (Fig. 1). It seems certain that within a few hundred years, the two will join. Then, in time of flood, the water from the larger stream will leave its present bed and go roaring down the Little River Valley. Thus Little River will have committed piracy on its parent, and the bed of the South Canadian will be dry except for the small amount of water that drains into it locally.

A similar condition exists a few miles to the north, near Oklahoma City. Here Deep Fork, only a few miles from the North Canadian, is flowing some forty or fifty feet below it and is in a favorable position, if left to itself, to commit piracy on its parent. Yet the two streams flow ninety miles east before they come together. Again, near Tulsa, Oklahoma, there is a similar relationship between the Verdigris and the Arkansas, though in this instance the two streams are somewhat farther apart and piracy is not quite so imminent.

This diversion of the waters of a stream by one of its branches is believed to have happened fairly frequently in the past history of the region. From Byers, Oklahoma, to near Haileyville lies the Gerty sand, which has the general shape and appearance of being an old stream bed. It is about ninety miles long, up to fifty feet thick and, in places, as much as four miles wide. It is thought to mark a former course of the South Canadian River, which was diverted from its bed sometime in the Pleistocene through piracies committed on it by some if its tributaries. It is interesting that Little River, which at one time was considerably longer than now, was involved in the diversion.

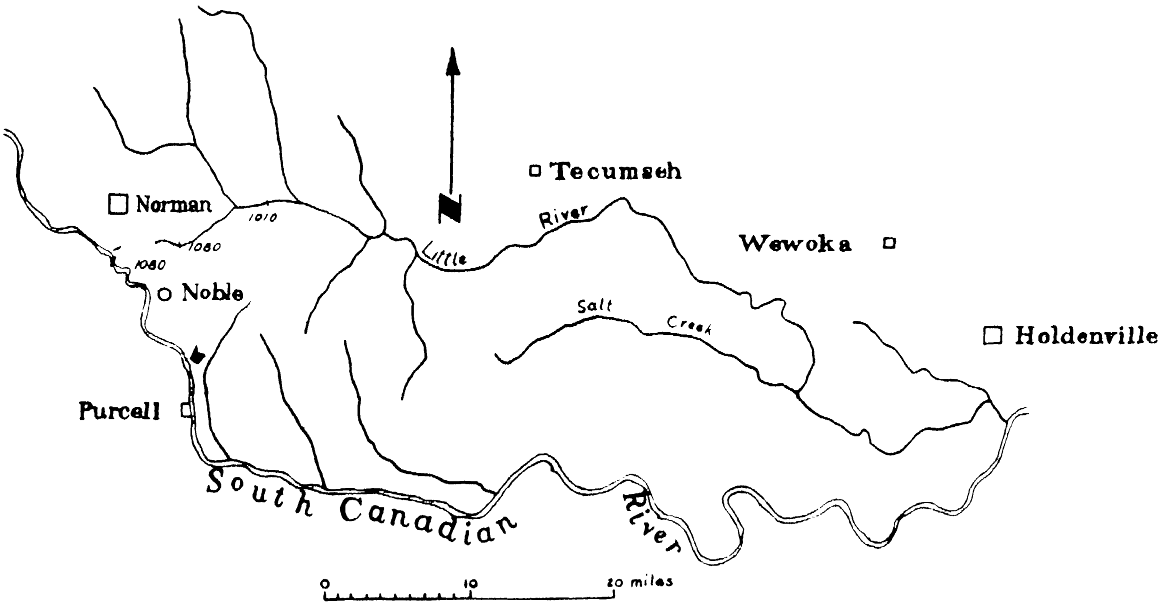


FIG. 1. THE SOUTH CANADIAN AND LITTLE RIVERS

SHOWING A CASE OF PIRACY IN THE MAKING JUST SOUTH OF NORMAN, OKLAHOMA.

Though Little River probably began its existence later than did the Canadian and other large streams of the plains, its valley has the shape, even well up among the small branches near its head, that is usually associated with an old rejuvenated stream. To a student of streams, the first sight of it is as startling as seeing a girl in her teens with gray hair. But the broad, flat terrace, with the deep channel winding through it, does not indicate old age. Here it is the normal appearance for a degrading stream. It results from the combined effects of the horizontal rock structure and the sudden intense floods succeeded by long periods of drought.

The horizontal rock layers, some soft and some hard, are conducive to the cutting of a wide valley. During heavy floods, water fills the valley from side to side, depositing over it a thick layer of alluvium and forming a broad, flat flood plain within which, during the long periods of low water, the stream flows in a fixed, narrow channel. Thus, though a steadily degrading stream, Little River has a cross section like that usually associated with old age, or with old age followed by rejuvenation.

During low water, the main streams of the plains, with their aggraded valleys, instead of flowing in deep, narrow channels, wander widely from side to side within their broad, shallow valleys. Because of the loose character of the material, they change their courses easily and often, with the curves working rapidly downstream. Such streams are very difficult to control.

In streams confined to rocky channels, the downstream travel of the bends is slow, but in plains streams, where the water wanders here and there between loose sandy banks, their courses change rapidly. Thus, a river flowing on one side of its valley today may, within a year, be found

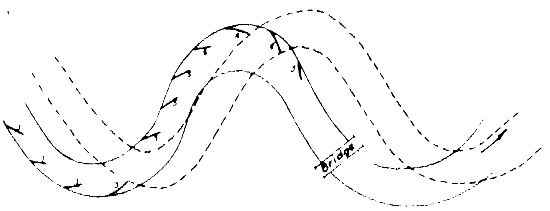


FIG. 2. A LOST BRIDGE

HOW THE CURVES OF A RIVER, BY MIGRATING DOWNSTREAM, MAY CAUSE THE ABANDONMENT OF A BRIDGE. *J* SHOWS HOW BY THE USE OF A NUMBER OF STRATEGICALLY LOCATED JETTIES SUCH A WANDERING STREAM COULD BE CONTROLLED.

a mile away on the other side. This situation is complicated by the fact that in time of flood the entire bed and valley may be full of water.

Such behavior makes bridging a difficult problem, especially so at a place where the river happens to be flowing down the middle of its valley. The bridge may no sooner be finished than there comes a flood, the curves in the stream shift downstream, and the river that was under the bridge is now flowing out around the end (Fig. 2). Something like this happened near Norman, Oklahoma, some twenty-five years ago. At that time there was an iron bridge about four miles southwest of town under which the water had flowed for eight or nine years. Then came two or three years of high water, during which the river curves moved rapidly downstream. This threw the channel from under the bridge near the north bank over close to the south side of the river, where it cut rapidly around the end of the bridge, and, before it was through, it was half a mile away to the south, having nearly doubled the width of its bed. For several years it continued to flow close against the south bank. But now, after some twenty years, the next phase of the river curve has reached the bridge, and the channel at low water is again back about where it was in 1920.

The difficulty of the bridging problem is much reduced where one side of the stream is against a high bank of rock. If this happens to be on the outside of a broad curve where the river is thrown hard up against the rocky shore, the bridge can be built in full confidence that it will be useful for many years. At such a location, a few miles above the Norman bridge, the Newcastle bridge has functioned continuously for forty years. But even in the absence of such a favorable location, it may be possible to keep the stream under the bridge by exercising constant vigilance.

It is a matter of persuasion rather than force. The control should be started some distance up the stream. With a good map or aerial photograph, a skilled physiographer can tell pretty closely the position the curves will assume as they migrate downstream. By the judicious use of wing dams and jetties on the first two or three curves above the bridge, the stream can be gently turned from its wandering ways and, in most cases, kept within its channel beneath the bridge.

The only other method of control is the building of heavy stone or concrete revetments and wing dams in the immediate vicinity of the bridge; but where there is no solid rock in which to anchor the outer ends of the structures, the method is very troublesome and expensive. It has been successfully done at some railroad river crossings, but for highway bridges it can seldom be justified.

For the geomorphologist, the streams of the Great Plains present many enticing problems. They obey the same laws of stream flow as do rivers in the humid regions. Yet, because of differences in environment, the results sometimes appear to be exactly opposite.

◆

SHEFFIELD SCIENTIFIC SCHOOL—THE FIRST HUNDRED YEARS

CHARLES H. WARREN

Dean Emeritus Warren (Ph.D., Yale, 1899) was instructor of mineralogy at Sheffield during 1900–01. For the next twenty-one years he taught at M.I.T. In 1922 he was made Sterling professor of geology at Yale and dean of Sheffield. This article is from an address delivered at the Centennial Convocation in Woolsey Hall on October 17, 1947.

IT IS the aim of this article to give a summary history of the founding of the Sheffield Scientific School, of its development during its first century of life, and to point out the part that it played in the evolution of higher education in the American college or university.

The firmly established pattern of education in the American college up to the middle of the nineteenth century was of the classical academic type, inherited in large part from the ancient universities of England. Its broad objective was to educate young men for service in "Church and Civil State," and this objective was to be achieved by storing the student's mind with a thorough knowledge of the Greek and Latin languages and of the civilizations they recorded, to provide him with what was regarded as a sound and orthodox theology, a correct religious and moral philosophy, and to train his mind in logical thinking. As a part of the process he was exposed to a modest amount of mathematics and natural philosophy, which latter included some physics, astronomy, and natural history. There was no opportunity to study the sciences in any modern sense.

During the first half of the nineteenth century a great expansion was taking place in agriculture, commerce, and industry, and with this arose a demand for men trained in scientific and technical knowledge who could cope with the problems arising on every hand. Young men with practical interests were seeking an education that would equip them to meet this demand. The colleges offered them little or nothing of that sort. This fact was recognized by some of those interested in education, and as early as the 1820s here and there "Technical Institutes" were organized. These were patterned after similar schools abroad and, although they were of post-secondary school level, they were not of collegiate grade.* During the period roughly from 1850 to 1870, a considerable number of colleges took more or less definite steps to meet the call for scientific and engineering instruction, and there were also founded a few independent technical schools on the collegiate level.

It was a Yale president, Timothy Dwight the Elder, to whom I think should be given the credit for starting, indirectly at least, the scientific educational movement in the colleges on its way. As early as the turn of the last century this wise and learned man had reached the conclusion that science and its applications were soon to assume an importance that made it advisable for the college to give them a fuller recognition in its educational program. He had in some way persuaded the Yale Corporation to authorize the appointment of a professor of chemistry and natural history, and in 1802 Benjamin Silliman was appointed to this chair. This act, as subsequent events were to prove, was Dwight's contribution to the cause of scientific education.

Benjamin Silliman (B.A., Yale, 1796) was only twenty-three years old when appointed. He had been a tutor in the college and was but newly admitted to the bar; he had only the barest acquaintance with chemistry or any other science, but after two years' study of science, chiefly at the Medical School in Philadelphia, he assumed his duties at Yale in 1804. Dr. Dwight's ideas of the qualifications for a professor of chemistry seem a bit strange in these days, but it must be admitted that he was a shrewd and competent judge of men. He picked a great man.

During the next forty-odd years Silliman did more than any other man to arouse an interest in science in America. Endowed with a striking appearance and personality, with extraordinary gifts of mind and character, unexcelled as a lecturer, he preached his gospel of science far and wide throughout the country, always insisting that scientific studies were nothing less than an inquiry into the manifestations of the works of God in the natural world and that a wise use of the knowledge so gained would result in great benefit to mankind. He prepared the way for the acceptance of the study of science as a desirable and essential part of a liberal education.

---

* These are not to be confused with the trade school or with the junior college. They occupy a place between the secondary and trade schools and the engineering or technical college. Only a few of them have survived in their original form and few if any have been organized in recent times. I think this is unfortunate, for they might be made to serve an important function in our educational program, relieving our four-year, degree-granting institutions of truly collegiate grade of many students whose capacities and interests could be more effectively served by "Technical Institutes" of this type.

Although Silliman's lectures in the College in chemistry, and later in geology and mineralogy, were notable and very popular with students, they were open only as electives, with little or no academic credit. However, he attracted a number of private students, most of whom later became leaders in scientific work and education. As their numbers increased, he felt the importance of extending the scope of this work, of adding new teachers, and of providing facilities for laboratory and experimental work, then practically nonexistent.

Silliman was responsible for the appointment, in 1846, of two professors, John Pitkin Norton, in agricultural chemistry, and Benjamin Silliman, Jr., in practical chemistry. In the same year a committee nominally headed by President Day, but actually by Benjamin Silliman, was formed to consider the advisability of establishing a distinct department of the College within which courses of study should be offered which were, or ought to be, given for others than members of the undergraduate classes and which were not in the Departments of Theology, Law, or Medicine. A favorable report was made by this committee and on August 19, 1847, the Corporation established a Department of Philosophy and the Arts to perform the functions designated by the committee. Under this department instruction was to be offered in the sciences and other subjects to graduates of the College and to other students of good moral character and adequate preparation. The creation of this department was the initial step in establishing postgraduate education in the American college, and presently, in 1859, on the recommendation of the faculty of the Scientific School, the Corporation established the degree of Civil Engineer and in 1860 that of Doctor of Philosophy. This latter degree was first conferred in 1861 on three candidates, to be followed two years later by a conferral of the degree on a man of the highest renown, Josiah Willard Gibbs.

The creation of this department also paved the way for the development of a new type of undergraduate education exemplified in the courses of instruction soon to be offered by the Sheffield Scientific School. It was the beginning, not only of the future Scientific School, but also of postgraduate education in America. It was an important step in the evolution of our university education.

The appointment of John P. Norton and Benjamin Silliman, Jr., both chemists, with assignment to this new department, led naturally to the offering of courses in that field as the first scientific courses in this department. This work was very soon being referred to as a "School of Applied Chemistry." In 1850 the Corporation, upon the recommendation of Silliman, John P. Norton, and James Dwight Dana, instituted the degree of Bachelor of Philosophy, and it was first conferred in 1852 on eight students who had completed the then two years' program of study.

In the same year, acting upon a recommendation of the men just mentioned, William A. Norton, an engineer, educated at West Point and then teaching at Brown University, was appointed as the first professor of civil engineering at Yale. He immediately organized a course of study in engineering. This was referred to as "The Yale Engineering School." In 1853 these schools of chemistry and engineering were together unofficially designated "The Yale Scientific School."

It was at this time that John P. Norton, broken down by overwork, died at the age of thirty. He it was who during the five years of his brief service carried the main burden and responsibility of teaching and formulated many of those policies of scientific study and research that were in large part to form the basic pattern of education in the Scientific School. Norton was the founder of agricultural science in America, and as a result of the training and inspiration received from him several of his students, John Addison Porter, his successor, William H. Brewer, and Samuel W. Johnson, all members of the School's first faculty, carried on his work. Indeed, this group was largely responsible for the designation of the School by the state of Connecticut to serve as its College of Agriculture and the Mechanical Arts from 1863 to 1895 under the Land-Grant College Act. One of this group, Johnson, in addition to his important contributions to the science of crops and soils, has the distinction of having established the Connecticut Agricultural Experiment Station, the first in the country and still functioning as one of the foremost institutions of its kind.

The small but distinguished group of men who made up the early faculty of the School found themselves faced with the problems of providing adequate instruction for a steadily increasing number of students and of broadening the scope of the School's work. The funds available, derived from the meager tuition fees and from contributions by members of the staff itself, were all they had. Outside support must be sought—how familiar that sounds!

On August 19, 1856, James Dwight Dana, Silliman's successor as professor of geology and mineralogy and America's foremost exponent of these sciences, gave the Yale Commencement address and made it the occasion of an eloquent appeal to the alumni and friends of Yale to support a plan for a better and more extensive organization of scientific work and instruction in the form of a distinct Scientific School. He gave a comprehensive outline of this plan, which presented a remarkable vision of the place of science in a university. This address was published, as were two other pamphlets written on the subject under his inspiration. These were widely circulated and excited much interest. They are well worth reading today.

The response to these appeals was immediate. A number of individuals gave substantial sums, but the principal donor was Joseph Earl Sheffield, of New Haven, railroad builder and financier, who was deeply and intelligently interested in promoting scientific and technical education. He not only gave the School substantial sums of money but a building, which he caused to be remodeled, enlarged, and well equipped for its use. This building (known as Sheffield Hall), now gone, stood at the head of College Street and was, I believe, the first college building to be devoted exclusively to scientific work. In recognition of Mr. Sheffield's generous support, the Corporation in 1861 named the new school "The Sheffield Scientific School." He continued his benefactions during his lifetime and was responsible for the formation of a Corporation, known as The Board of Trustees of the Sheffield Scientific School, to hold funds and property for the use of the School. In his will he made the School through this Board one of his heirs and his residuary legatee.

The success and prestige of the School were due principally to the extraordinary group of men who presided over its affairs during its earlier decades. They were eminent as scholars and teachers, able administrators, and pioneers in establishing a new type of education. They gave a self-sacrificing devotion to this cause and in the end established a pattern of liberal scientific education which influenced strongly the whole trend of higher education in the American college. It may be added that the condescending disapproval and suspicion with which the academic faculty chose to regard the alleged vocational training offered by the Scientific School undoubtedly acted as an additional spur to its faculty to justify and promote its program of education, which it firmly believed was a sound and liberal one. Indeed, there is evidence that there was often a good deal of—shall we say?—vigorous argument between the two groups as to the relative virtues of their divergent views on education.

I have already mentioned some of the early faculty members. It is not possible to give here an account of those who were active during the early decades nor of their many distinguished successors in the fields of science, engineering, and the humanities in later years. A full account of them may be found in Director Chittenden's admirable and comprehensive *History of the Sheffield Scientific School* (New Haven: Yale Univ. Press, 1938). I shall confine myself to mentioning only a few names, in addition to those earlier referred to, of those who seem to me to have made some of the more significant contributions to the School's development and whose names also illustrate the breadth of the School's program. I must mention George Jarvis Brush, mineralogist, the first director of the School and its treasurer; Russell H. Chittenden, the founder of physiological chemistry in this country, the originator of the first organized program of study in subjects preparatory to the study of medicine and the medical sciences, and the successor in 1898 to Dr. Brush as director of the School. To these two men, who guided its destinies for well over fifty years, the School owes an inextinguishable debt of gratitude. I will mention also William P. Trowbridge, the first professor of dynamical or, as we call it, mechanical engineering, and the organizer of the first course of study in that field; General Francis Amasa Walker, the eminent economist, who later became president of the Massachusetts Institute of Technology; William D. Whitney, the most distinguished linguistic scholar of his generation; Thomas R. Lounsbury, noted English scholar and writer and brilliant teacher, who effected a revolutionary change in the teaching of English literature; Chester S. Lyman, professor of astronomy and physics; Othniel C. Marsh, eminent vertebrate paleontologist; Addison E. Verrill, zoologist; Daniel Cady Eaton, botanist; and, last, Daniel Coit Gilman, professor of physical and political geography. I mention him particularly because I do not think that the importance of his connection with the School both to it and to Gilman himself has been adequately appreciated. His remarkable and versatile abilities, as well as his extensive knowledge of educational practices here and abroad, were fully recognized by members of the Governing Board. He became its official spokesman. His articles and addresses regarding the School became, as has been said of them, "The School's Articles of Faith" and have seldom been equalled and never excelled as statements of sound educational principles. It was during the nine years of his connection with the School that much of the educational philosophy was crystallized that later found a brilliant and epoch-making expression in his accomplishments as president of the Johns Hopkins University.

Years later, in an address Gilman gave at the fiftieth anniversary of the School, he said, in part:

> In quick succession, colleges, departments of science and independent institutions have appeared in every state. Of these not a few have adopted the methods here followed or have called to their support those who have been trained here. For one such institution, now celebrating its majority, permit me to acknowledge with filial gratitude the impulses, lessons, warnings, and encouragements derived from the Sheffield Scientific School, and publicly admit that much of the health and strength of the Johns Hopkins University is due to early and repeated draughts upon the life-giving springs of New Haven.

I TURN now to a brief review of the School's actual program of instruction. It was at first conducted on the postgraduate level, to which was soon added a three-year undergraduate program. Both expanded rapidly in numbers and scope and were administered together until 1918–19, when all postgraduate instruction was transferred to a separate Graduate School. I need not dwell on the character of the early postgraduate program since it differed little in principle from prevailing university practice.

In the undergraduate program, with one exception, the courses offered were confined to the major fields of science and engineering. The objective of these courses, clearly stated from the beginning and adhered to consistently, was that of providing a foundation in the basic principles, concepts and methods of scientific work, the mastery of which must precede further advanced study or successful practical applications. These studies were to be accompanied by a substantial amount of attention to English, the modern languages, and other cultural and humanistic subjects. In other words, a liberal education with the emphasis on science. As scientific and technical knowledge increased, the basic courses were broadened in scope and new subjects and new major courses were added.

There was one course, however, of a radically different character, the celebrated "Select" course, now but a memory. There was on the part of influential members of the faculty a feeling that the School in breaking away from the traditional classical curriculum ought to champion the idea that a liberal education could be secured through pursuing a course made up of the modern humanities, English, modern languages, the basic sciences, and mathematics, and as early as 1860–61 a course in "General Studies" was offered. In 1864 this course was reorganized, under the influence of Gilman, and an excellent selection of required subjects was offered under the title "Select Course in Scientific and Literary Studies." It was a unique course and undoubtedly contributed to the development of modern college curricula in the liberal arts and sciences. It had a strong appeal to many students and grew rapidly in numbers until it became by the end of the first decade of this century the largest course in the School; it even overshadowed to some extent the scientific and engineering courses. Whatever its shortcomings may have been, it was a good course in its day and graduated many men prominent in afterlife. It came, in time, to be out of place in a university that also maintained another liberal arts course in the academic college.

Yale College, up to about 1880, had adhered closely to its traditional curriculum. It was opposed to any substantial change and felt no responsibility for education in the sciences. It was content to leave that to the Scientific School. There was, however, a faculty group, led by William Graham Sumner, who were strongly in favor of liberalizing and broadening the curriculum to meet a growing public demand for such a change and to keep abreast of similar movements in other colleges. In 1884 Sumner set forth his views on higher education in a notable article published in the *Princeton Review*, entitled "Our Colleges before the Country." In this and in other essays referring to educational matters, Sumner's statements are remarkable for their breadth of view, force, and clarity. His views were in close agreement with, and supplemented, those expressed so well in an earlier day by Dana and Gilman, and all are well worth reading and pondering in these days when the educational world is all astir with plans for reforming our educational program. I have yet to hear or read anything that, in so far as fundamental and enduring principles of education are concerned, was not fully covered by these gentlemen half a century or more ago.

The progress of Sumner and his sympathizers was slow. But they did succeed in imposing a more liberal policy on the College, and, although this did not go as far as Sumner desired, it opened the way for future constructive changes. Gradually, departments and laboratories of chemistry, physics, geology, and biology were established in Yale College, and programs of study including these sciences were offered. As a result, a large amount of duplication developed between the two schools in the sciences and even in the humanities. This situation as a whole led in 1918–19 to a reorganization of the University, which effected many important changes. It established University departments of study, each to serve the needs of the several schools. It placed all postgraduate study under a Graduate School. The three-year course of the Scientific School was made a four-year course. It abolished the "Select Course," as well as a graduate program in Business Administration that had been started in the Scientific School with a substantial endowment. It established a separate faculty to administer a program of freshman studies preparatory to the courses of both Yale College and the Scientific School. The Scientific School then became a purely undergraduate school for the study, it was stated, of professional science and engineering; Yale College was to be the school for the study of the liberal arts and sciences, with emphasis on the humanities. The engineering courses could be regarded as professional in character, but the attempted distinction was not valid for the science majors offered by the Scientific School. The science programs in the latter were more rigorous and definite in character than those of the College, but there was nothing to prevent a student in the College, majoring in the sciences when working for his B.A. or Ph.B. degree, from following substantially the same programs as those offered in the Scientific School leading to the B.S. degree. He could study the same subjects and under the same teachers, and many were permitted to do so. The choice of school by the student who wished to major in a science became largely a matter of which degree he preferred and which school with its distinctive social system appealed to him most.

In 1932–33 the residential college plan, made possible by the generous gifts of Mr. Edward S. Harkness, was inaugurated. This brought upperclass undergraduates together as residents of the colleges, with admirable opportunities for intellectual and social intercourse, not only with each other but with the faculty Fellows of each college. It tended to obliterate that unique Yale practice of dividing its undergraduates into the "Ac" and "Sheff" species according to their school affiliation, a division that once had a real significance but had become largely a survival.

The engineering departments, which had originated and grown up as an important part of the Scientific School, but which had been constituted University departments with their instructional duties divided between the Graduate and the Scientific Schools, were united in 1932–33 to form a distinct School of Engineering, thus conforming to the general practice of American universities.

During the war years just past a committee, representative of the many fields of study, was appointed by Dean William C. DeVane, of Yale College, to make a thorough study of undergraduate curricula and to draw up a new program better adapted to the objective of educating our young men for service in the postwar world. When this new and admirable program was completed, it became clear that it incorporated, among other desirable features, the objectives, set forth in principle so well by Gilman, to which the Scientific School had always adhered. This new program was such that the science courses of the Scientific School could with only minor changes be incorporated into it, and it seemed eminently wise on all counts that all undergraduate instruction should henceforth be administered by a single united faculty representative of the arts and sciences. This was accomplished in 1945, and Yale now offers to all its undergraduates, except those in the Engineering School, under the faculty of Yale College, a well-balanced, fully integrated, yet flexible, program of study designed to provide its students with a knowledge of the important fields of human thought and accomplishment, together with an opportunity to concentrate and master the fundamentals of some field of study chosen according to their individual aptitudes and interests, and, most important of all, to help them to secure a mental discipline and outlook that shall help them to use their education to the best advantage.

You may now ask what is to be the function of the Sheffield Scientific School as it begins its second century. The old "Sheff," as men of my college generation knew it, has disappeared. It has now resumed its original function, that of postgraduate education, and has added certain new and appropriate functions that will enable it to preserve its integrity as an institution, its honorable name, carry out the purpose for which it was founded, and move forward to new achievements as a vital part of the distinguished University which gave it birth and which it has served so long and so well.

The faculty of the School will henceforth be concerned with the instruction and guidance of students registered in the School, candidates for the degrees of Master of Science and Doctor of Philosophy, and with scientific research. The Board of Permanent Officers of the School has been constituted the University Division of the Natural and Physical Sciences and Mathematics and as such will, under the President and Corporation, exercise general supervision over educational policies for all study and research in these fields, including particularly the correlation and integration of teaching and research in the departments within the Division. Most important of all, the Director and his Permanent Officers, acting through standing or special committees, will examine and pass judgment on all recommendations for promotion and new appointments to the faculties of the departments within the Division and report the decisions to the departments and schools to which they may be assigned, and to the President and Corporation for their information and for final action. Thus, all scientific work in the University will be under the general supervision of a single competent group of scientists. This arrangement restores to the School functions that it exercised so successfully during the earlier decades of its history.

The Sheffield Scientific School will go forward into its second century of service with all its energies and resources devoted to the pursuit of its time-honored objective: "The Promotion of the Study of the Natural, Physical and Mathematical Sciences"—to discover and interpret the laws and phenomena of the natural world to the end that these may be wisely and humanely applied for the welfare of mankind.

⁂

PARADOX

Not truth, nor certainty. These I forswore
In my novitiate, as young men called
To holy orders must abjure the world.
'If . . . , then . . . ,' *this only I assert;*
And my successes are but pretty chains
Linking twin doubts, for it is vain to ask
If what I postulate be justified,
Or what I prove possess the stamp of fact.

Yet bridges stand, and men no longer crawl
In two dimensions. And such triumphs stem
In no small measure from the power this game,
Played with the thrice-attenuated shades
Of things, has over their originals.
How frail the wand, but how profound the spell!

CLARENCE R. WYLIE JR.

SCIENCE ON THE MARCH

TEAMWORK IN THE SOCIAL SCIENCES*

IN THIS article I shall seek, first of all, to test the hypothesis that a greater part of the conflict among contemporary scientists is methodological in nature and that these methodological controversies are largely the result of the reactions of individual scientists to scientific traditions. Second, I shall offer certain suggestions for resolving scientific controversies and for promoting cooperative endeavors in the social sciences.

If we could all agree upon some definition of science, it would have to be a simple one, such as: Science is tested knowledge. Or, science is all endeavor to ascertain facts and their interrelations. According to these definitions, each science may determine what knowledge it should test or what facts and their interrelations it should ascertain. Furthermore, such definitions place no limitations upon any science as to the methods and techniques it should employ in research. This spirit of freedom is one of the most important of our scientific traditions. Of course, it is broken occasionally by political coercion, as was the case in Hitler's Germany; it is also limited by the subject matter that may be accepted as belonging to each discipline, limited somewhat by the leaders in the various scientific fields, and by the pressures from society and culture, which are controlling forces in human activity or achievement.

Although this spirit of freedom is regarded as essential for scientific progress, it may retard such progress where individual scientists spend their time and talents carrying on heated controversies as to the accuracy and validity of the work of others. Too often, perhaps, one scientist judges the work of another in terms of what he thinks the other person *ought* to do, *should* have done, *should not* do, or what he *ought not* to have done. In many instances, this "ought attitude" is carried over into the future tense to express a sort of "scientific idealism." Hence, the tradition of scientific freedom tends to motivate personal controversies. The personal patterns of such controversies are intensified when certain theories are criticized, amended, refuted, or exposed as erroneous.

A second tradition handed down to modern scientists is that some of the sciences are "pure," some are "applied," and others are neither, but really belong in the field of technology or art. Much of the controversy over this question has been suggestive and stimulating both from the standpoint of the classification of the various disciplines and from that of modifying and standardizing scientific method, but it has often led to dogmatic conclusions that have set up barriers to scientific research, on the one hand, and, on the other hand, has produced a pattern of unhealthy autonomy among some of the sciences.

This sort of scientific dogmatism has sometimes resulted in an uncritical approach in scientific research, based upon the assumption that in this or that specific field of research the scientist is not simply courting objectivity, but is wedded to it.

It is my contention that such a tradition is misleading, and unwarranted in view of the facts; for no science is pure in the sense that it is wholly separated from applied science, technology, and art; every science is to some extent a pure and an applied science and at the same time something of a technology and an art.

Pure science asks the question "What is it?" and seeks to answer it by some system of research using a skill that involves technique and art. Applied science asks: "What can you do about it? What predictions and controls are suggested?" and, whether a particular scientist admits it or not, he is interested in predictions and controls, and in some respects is influenced in his research by these applied aspects of his science. Technology asks the question "How can you do it?" which involves the choice of equipment, techniques, and methods of research to be used. Art asks the question "How can we acquire skill in doing it?" which involves the development and application of certain skills for carrying on specific experiments and research.

Another scientific tradition almost universally accepted among scientists is the research process necessary for acquiring and testing knowledge. We say, first, that one must begin with the formulation of some problem; second, we give a tentative answer to the problem in the form of a hypothesis; then we collect data to test the hypothesis or to formulate new hypotheses; next we classify and compare the data; then we interpret the findings; and in the final step before publication we verify the conclusions by repeating the process as many times as the researcher considers necessary to substantiate his conclusions.

---

* From an address on "Some Personal and Traditional Aspects of the Methodological Conflicts Among Contemporary Scientists" before the twelfth annual meeting of the Florida Academy of Science, Tallahassee, April 21, 1948.

It is doubtful, at the present stage of the development of science, whether this research process, as a general policy, can be improved upon. Yet there may be some question as to the ways individual scientists utilize methods and techniques in carrying on their research. Here is another source of conflict that is basically methodological.

The personal aspects of methodological conflicts in science are generally linked with convictions regarding scientific dogmas. I have selected five commonly accepted dogmas which tend to promote conflict:

1. The scientist separates his subjective preferences, emotions, and desires from the logical, universal elements in his thought and works completely objectively.
2. Scientific research is divorced from all types of biases, value judgments, and preconceptions.
3. Science employs universally valid and understandable symbols.
4. Quantitative symbols and language are scientific, whereas qualitative language and descriptions are vague and pseudoscientific, if not unscientific.
5. The subjectivity of qualitative descriptions can be objectified by transferring qualitative language into statistical and quantitative formulas and symbols.

Other personal aspects of methodological conflicts grow out of research methods and techniques employed by individual students. This is particularly true of observation techniques, the goals of research, the methods of logic employed, and the adaptation of quantitative techniques to the classification and comparison of data. Individual choices must be made constantly in each of these areas of research; hence, research tends to become personalized with each choice. But there is always the possibility that another person, with a different point of view or a different background, might make different choices. This personalized aspect of research not only leads to conclusions that often conflict where two or more individuals are studying the same problem, but it also makes for methodological conflicts as well.

Scientists are fairly well stratified into groups in terms of their acceptance or rejection of certain traditions, dogmas, and fads of science. Such stratification is often stimulating, but it is likely to produce numerous methodological conflicts. For example, there are those scientists who stand by scientific traditions as opposed to those who feel that problems of values and ends should and do come within the scope of scientific methods. Likewise, there are those who feel that there is essentially no difference between the methods of the physical and social sciences, as contrasted with those who forcefully seek to draw lines of methodological distinction between the two.

In every case, the conflicting positions are essentially methodological, and they grow out of the personal reactions of individual scientists to scientific tradition, on the one hand, and to the attitudes and goals that the respective scientists hold for themselves or for fellow-scientists, on the other. Our most important task at present is to develop a cooperative spirit while coordinating the efforts of all individual scientists and all groups of scientists.

The physical sciences have demonstrated their capacity in such a manner that they are revolutionizing surgical, medical, and related practices. They have gained the respect of a greater part of the civilized world. For the most part, the social sciences have not gained this type of respect. Principles and laws of human relations, as developed by the social sciences, have not been accepted with the authority many of us feel they deserve.

We have reached a point in the development of scientific thought and research, and in the development of civilization as well, when both social and physical scientists must reconsider their roles in the life of man. The scientific age in which we live would seem to preclude drastic competition and conflict between the sciences as well as the complete autonomy of any science, or any group of sciences. But since the competition and conflict now existing among scientists rest so largely upon the problems of methodology, the immediate task before us is that of developing a uniform methodological outlook that is at once critical and constructive. This would tend to weaken the forces that are causing some lags in our scientific knowledge, while reducing the conflicts within the scientific realm itself. When this has been accomplished then the physical and social sciences can move toward reducing those external forces such as establish intolerance, institutionalized suppression, and selfish interests of all types, which too often impede scientific progress by producing confusion, strife, and conflicts among individuals, groups, and nations.

I do not mean by this that science can save the world or solve all its problems, but it will be able to contribute more in this regard through cooperative scientific effort than it is now able to do with scientists often working at cross-purposes or in actual conflict with one another. But not all our methodological conflicts need be resolved before cooperation begins.

One cooperative effort urgently needed is that of organizing the research of social scientists in such a manner that we can bring together, in a well-planned, systematic, and synthetic summary, the contributions social scientists have admittedly made in the fields of community planning, juvenile delinquency, housing, marriage and family life, international relations, education, sanitation, health, and the like. Such a venture would not only help the teachers of social science by placing in their hands valuable teaching aids, but it would also stimulate the application of certain fundamental social principles and laws, which need social sanction in the same sense in which certain discoveries in the physical sciences have been applied in solving individual and social problems.

Teamwork is especially needed in the development of original research in the social sciences. There are at least five ways in which cooperative research may be promoted at present:

Coordination in planning research. This involves pooling research interests and making them known through publication of research projects in process. This is already being done by various professional societies. But the planning should go beyond this and designate those students who are available for collaboration in given fields.

Cooperative conferences in promoting research. This would involve bringing together students from a particular locality or those with common research interests to discuss and plan research programs and methods and to allocate the work agreed upon by those participating in the project.

Cooperative utilization of resources and skills. Short institutes might be held for specific types of research training after a project is begun. These institutes would resemble the so-called workshop training courses, where all individuals exchange information and discuss various types of skills and samplings of research already in progress They might also lead to cooperative use of research equipment and trained technicians.

Teamwork in the execution of research. More comprehensive studies might be made by following the symposium technique of farming out different aspects of a question to specialists, or by parallel studies of the same problem by different students in different localities using the same hypotheses and the same methods and techniques of research, or simply by joint participation in a limited field or in a specific locality.

Cooperative dissemination of facts and information. Although this type of cooperation does not fall strictly within the area of scientific research, it is of utmost importance. It involves every sort of educational skill, from the most technical and academic type of report and lecture to the most popular type of writing and speaking. The dissemination of facts should be directed to *all* groups and classes of people.

The social sciences have a long way to go if the fruit of those who labor in the field is to influence public opinion in ways that will benefit all members of society. There are numerous difficulties to overcome before the work of social scientists is accepted on the same footing as the findings of many of our physical scientists are already accepted. The gap between the two fields of science must be made narrower, and it appears that nothing short of cooperative research will give the social sciences the status they need to deal constructively with today's problems. Long before this goal can be realized, a united front among scientists must be developed. Teamwork, and teamwork alone, will eliminate many of the methodological conflicts between social scientists and lessen the controversies that now prevail between physical and social scientists.

Science has never been faced with a greater challenge than it has today. But science cannot meet this challenge unless we, as individual scientists, join hands with our colleagues and work diligently in an effort to resolve personal disagreements and controversies and strive to promote a spirit of tolerance, understanding, and cooperation. This is our challenge; let us continue to face it!

MELVIN J. WILLIAMS

Department of Sociology
Florida State University

Hg^{198} AS A WAVE-LENGTH STANDARD

THE alchemist's dream of transmutation has come true, and, following the well-known rule that dreams go by contraries, the transmutation is not as he hoped, from base to noble, but from noble gold to base mercury. As an accomplishment of theoretical science, the new mercury in the form of a pure isotope is important, but some of this mercury in electrodeless lamps made by Dr. William F. Meggers, of the Bureau of Standards, may soon have a profound effect on our statistical data on wave lengths and thus be of immediate practical use. To understand some of the implications, it will be useful to recall part of the history of the accurate measurement of wave length, and how the hidden complexity of some seemingly simple spectrum lines has presented real barriers to progress.

When Michelson made his famous count of the number of wave lengths of the cadmium red line in one meter, he was under a severe handicap that was not fully understood until the existence of isotopes was discovered. Michelson, and others, were aware that different lines acted differently when used in his interferometer. Some would give fairly sharp patterns for short separations of the mirrors, but at increased separations the concentric rings of light merged together so that they could no longer be observed and accurately counted. Other lines gave slightly better results, and cadmium 6438 was finally selected as being best suited or, more accurately, as least defective. Later, when the Fabry-Perot interferometer had been developed, the superior brightness and definition of the rings made wave-length measurements easier and more accurate. But no spectroscopist is ever satisfied with the last digit in a wave length, and it seems there must always be a last digit to annoy him. As the last digit moves to the right, however, it becomes less and less annoying, though not necessarily less important.

The green line of mercury, 5461A, on first inspection looks like a single sharply defined line, well separated from its spectrum neighbors, and lying in the middle of the visible spectrum where the luminosity is high. It is thus ideal in all respects except one—it is really not a single line. The Fabry-Perot interferometer is an excellent detector for discovering the fine structure of spectrum lines, and this very resolving power interfered with the measurement of precise wave length. The development of the theory of isotopes helped explain the character of 5461 by pointing out that the 10 isotopes of mercury would give 10 lines grouped near 5461 and that the accurate designation of the true center of the group was practically impossible. The red line of cadmium also has a fine structure of about 12 lines, and it is only by coincidence that the arrangement makes the whole group better suited for precision wave-length measurement.

The answer to this difficulty would be an ideal source that is composed of a single isotope giving a complete spectrum of truly single lines. Meggers has constructed lamps containing a single isotope of mercury, Hg^{198}, and he now has available a spectrum of some 33 lines, each superior in inherent sharpness to anything previously obtainable. His abstract of the paper presented to the March 1948 meeting of the Optical Society of America reads:

> Electrodeless lamps containing five milligrams of Hg^{198} and five millimeters on a mercury pressure gage of argon excited by 100-megacycle frequency are found to be highly satisfactory sources of exceedingly sharp spectral lines. Employing Fabry-Perot interferometers and prism spectrographs, preliminary values of the wavelengths of thirty-three lines (2537A to 6907A) have been determined relative to the red radiation from cadmium simultaneously by imaging one lamp in the other. Interference patterns of the stronger Hg lines can be photographed in less than one second. Etalons* of 5, 25, 40, 50, and 67 millimeters have been used thus far. The probable errors of these preliminary wavelengths emitted by mercury 198 range from one part in twenty millions to one part in one hundred millions. The relative values of these wavelengths will be refined by measuring them in vacuum.

The attainment of this ideal radiator will probably lead to a resurvey of our entire literature of wave lengths, and perhaps a redefinition of the length of the meter, probably in terms of the mercury line 5461 that has in times past been so disappointing to physicists.

FRANK BENFORD

General Electric Research Laboratory
Schenectady, New York

---

* *Etalons.* A pair of mirrors permanently spaced a known number (g) of wave lengths apart. By moving the etalons through their own length, the wave lengths can be counted in groups of g, thus greatly shortening the work of counting.

◆

ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

AUBURN CAMERA CLUB June 28–July 12
Bureau of Animal Industry
U.S.D.A.
Auburn, Alabama

AMERICAN OSTEOPATHIC ASSOCIATION ANNUAL CONVENTION July 19–23
Boston, Massachusetts

GENERAL FOODS CORPORATION August 5–25
Central Laboratories
Hoboken, New Jersey

ACADEMY OF SCIENCE October 1–15
Illinois Wesleyan University
Bloomington

* Dates available for showing 1948 Salon pictures.

BOOK REVIEWS

UNDERSTANDING SCIENCE THROUGH ITS HISTORY

Emanuel Swedenborg, Scientist and Mystic. Signe Toksvig. 389 pp. $5.00. Yale Univ. Press. New Haven, Conn.

THE strange and fascinating career of Emanuel Swedenborg (1688–1772) continues to baffle both the scientist and the theologian. He was one of those "universal" scholars that adorned earlier centuries. He knew astronomy, physics, chemistry, physiology, psychology, anatomy, mathematics, Latin, Greek, and Hebrew. He was a mineralogist, a builder of bridges and canals, an authority on mines and metals, a mechanical genius, a poet, a member of Parliament, a writer on good government and finance, and he traveled extensively throughout Western Europe. Some of his conclusions, such as the definition of matter as motion, his researches into the nature of blood, heart, brain, and the nervous system, and his delving into the realm of the subconscious mind, anticipated some of the achievements of present-day scientists and still have value.

Fortunately, or unfortunately, depending on one's interest in mysticism, Swedenborg's mind was not satisfied until it invaded that unseen world whose realities are not measurable by the instruments of science. He knew that "there are marvellous things occurring in the human mind, so marvellous indeed that they cannot be expressed," and he could not let his mind alone. He craved order and a realm of law in both the physical and the spiritual world, and he wanted to reconcile religion with reason and science, without offending either. Whether he succeeded, or went off the deep end, is still a matter of debate.

Swedenborg was engaged in a great quest for the soul, the formative force which causes the body, the immaterial which builds the material. He believed in a God who was the source of all living matter, and in his "mission" to cleanse all existing religions and introduce the new religion of the New Jerusalem based on a gospel dictated by the angels themselves. He entered the realm of mysticism by techniques not unlike those of Yogi; he had "God-intoxicated" dreams and visions, saw the divine flame of the mystic, and finally had the afterworld opened to him to such an extent that his spirit could commute freely among the planets and interview the characters of history from the Apostles to Newton and Louis XIV. As a result, he could describe heaven and hell in minute detail.

Swedenborg's literary productivity was enormous and phenomenal. Most of his volumes deal with his fantastic excursions into religious mysticism, and some are marked by the strangest symbolism and revelations. As a scientist he heaved "chunks of science" "into mystic joy." Many people have tried to find rational explanations for this dual personality. Some have talked about the sex suppression of an oversexed man; others have suggested paranoia and schizophrenia; still others have made comparisons with the telepathy and precognition of modern psychical research. The enigma and the strange duality of this exceptional person remain. Some of his writings are "scholastic sawdust" and incredible fantasy; others reveal a rich beauty and ethical content, and shrewd scientific observation on an amazing number of natural phenomena and human experiences.

One cannot do justice to so complex a character and so learned a biography in so short a review. If the great Swede remains an incomprehensible figure to this reviewer, who is neither a scientist nor a mystic, the fault is not the author's, for she has done a prodigious amount of research, has struggled sincerely, sympathetically, and with scientific objectivity with Swedenborg's many-sided career and his "revelations" and "systems," and she has produced the best analysis available.

CARL WITTKE

The College of Arts and Sciences
Oberlin College

The Beginnings of Modern Science. Holmes Boynton, Ed. xv + 634 pp. $2.39. Classics Club. New York.

THE book under review, although a scientific source book, differs from other publications of this character in that the latter are usually intended as reference books for the scientific specialist, whereas this volume is planned to give the general reader with an interest in the subject a better understanding of modern science. It contains writings of the sixteenth, seventeenth, and eighteenth centuries, covering the general subjects of physics, chemistry, geology, biology, medicine, and scientific philosophy. In each division the plan has been to choose reports on related topics in order to show step by step the growth of thought in each subject and the improvement of experimental technique. Each division of this book is prefaced by an introduction giving a brief summary of its contents, and explanatory footnotes are given where the original article might be difficult for a modern general reader to understand. The original articles, of course, have been in some cases considerably abbreviated, but this has been done without disturbing the logic of the writing. This plan makes the book as a whole quite easy reading.

As an example of the articles contained in this book, the section entitled Matter and Motion includes, among others, Tycho Brahe's *On a New Star,* Cavendish's *Experiments to Determine the Density of the Earth,* and extracts from Newton's *Principia.* In the section on Electricity we find Franklin's description of his famous kite experiment, and *The Discovery of Current Electricity,* by Galvani. The section How Plants Grow includes *The Breathing of Plants,* by Ingenhousz, and *A Dissertation on the Sexes of Plants,* by Linnaeus. In The Science of Healing we find *The Invention of Vaccination,* by Jenner, and *The Prevention and Treatment of Scurvy,* by Captain Cook. On the whole, the selection of the articles has been well done, and the book can be recommended to the class of readers for which it was intended.

PAUL R. HEYL

Washington, D. C.

Christian Huygens and the Development of Science in the Seventeenth Century. A. E. Bell. 220 pp. Illus. $4.50. Longmans, Green. New York. Edward Arnold. London.

IN THIS, the American edition of a book first published in London in 1947, the author, who is head of the Science Department at Clifton College, gives an interesting account of the life and scientific significance of that great seventeenth-century genius Christian Huygens. The book is appropriately divided into two parts, the first being more in the nature of a biography of Huygens, and the second dealing with a more detailed description of his most important contributions to knowledge.

In the opening sentence of the Preface the author describes Huygens as one of the greatest scientific geniuses of all time and then proceeds to substantiate this statement by enumerating Huygens' accomplishments: he "transformed the telescope from being a toy into a powerful instrument of investigation;" he is "rightly regarded as the founder of the wave theory of light and thus of physical optics;" he founded the dynamics of systems, cleared up the subject of the pendulum and the tautochrone, and discovered the rings and the brightest satellite of Saturn.

In reading this absorbing account of Huygens' life one cannot help but gain the conviction that it was this very versatility that prevented Huygens from reaching the heights that Newton did, though the same versatility formed the basis for the wide acclaim accorded him by his contemporaries. Originally steeped in Cartesian philosophy—Descartes was an occasional visitor in the home of his father—Huygens gradually emancipated himself from that point of view to such an extent that in his later years his position and his accomplishments were much abused by the then remaining proponents of Descartes in Paris.

Huygens' eminence naturally led to his attaining a guiding, and even commanding, position in the Académie Royale des Sciences, founded by Louis XIV (at the instigation of Colbert) in 1666. Reading of the difficulties encountered in its establishment reminds us only too vividly of the very similar birth pangs attending the establishment (we hope) of the National Science Foundation in our own day.

How Huygens was instrumental in having Roemer's discovery of the finite velocity of light accepted over Cassini's opposition; how he was obviously familiar with and used what are now known as Newton's first and second laws of motion; how he observed the rotation of the planet Mars, but, with characteristic caution and distrust of the accuracy of his own observations, delayed publication of it; how, regardless of Da Vinci's and Galileo's earlier work, it was Huygens who made the pendulum clock into a practical reality—all these things we are in need of being reminded of, for in spite of his great accomplishments it is "the accidents of history" which, as the author shows, are responsible for Huygens' not having attained a stature equal to that of Galileo and Newton.

The second half of the book goes into considerable detail on Huygens' scientific work, dealing especially with the pendulum clock and his wave theory of light and concluding with an evaluation of Huygens' place in the history of science. Appropriately, the clue to his own conception of scientific research is presented in his own words: the difficulties in the problems cannot be overcome "except by starting from experiments and then by conceiving certain hypotheses"—which would still be sound advice to any prospective scientist today.

W. J. LUYTEN

Department of Astronomy
University of Minnesota
Minneapolis

The Growth of Physical Science. Sir James Jeans. x + 364 pp. $4.00. Cambridge, at the University Press. Macmillan, New York.

PARALLELING the growth of physical science itself, this book moves leisurely through ancient and medieval times, then with increasing tempo arrives in the twentieth century with the same "breakneck speed" with which the author characterizes today's motion of "our material civilization."

This increasing tempo results obviously from the confinement of the total story within the covers of a single book. But the latter pages could perhaps have been put to a more useful purpose had the author granted the difficulty of an adequate coverage of the growth of modern physical science, and instead, with a few examples, sought to present the same perspective in modern times which he achieves so well in his portrayal of the growth of physical science in Greece, Alexandria, and even during the Dark Ages.

The continuous impact of this early history is seen forcing science from one center of learning to another and eventually into hiding. The tolerance of the Romans allows the University of Alexandria to flourish, but later the University declines as Christianity "must have provided a powerful deterrent to the scientific spirit of free inquiry." Some of its scholars migrate to Byzantium before its final end when the Mohammedans conquer Alexandria.

Later, physical science is seen taking root in Western Europe, but we learn nothing of the manner in which it subsequently spans the Atlantic. The slowness of America to achieve a significant role in the growth of physical science, as well as the migration of European scientists to America during the past few decades, receives no such analysis as do the early meanderings of science about the Eastern Mediterranean.

At the outset, Jeans states that he hopes "to trace out the steps by which it [physical science] has attained to its present power and importance," but nowhere does he mention the mutual invasion of the practical arts and physical science which has occurred during the past century. He does refer occasionally to practical contributions to science, such as the invention of printing and of the telescope. He may be right when he notes some of the writings of Francis Bacon and goes on to say that seventeenth-century science "no doubt lost much through this shifting of the emphasis from knowledge for its own sake to knowledge for utility's sake." But surely modern technology has had a sufficiently important role in the attainment of physical science "to its present power and importance" to at least deserve mention.

The book is intended for the educated layman, but even for those readers well acquainted with present-day high-school mathematics, it is not always light reading. The thinking of many of the great minds in the growth of mathematics, physics, chemistry, and astronomy is clearly described. The author obviously delights in presenting the reader with some of the mathematical problems solved by Euclid, Leonardo, and others, but the reader is never left in doubt, for the solutions are always provided.

PHILIP N. POWERS

U. S. Atomic Energy Commission
Washington, D. C.

The Green World of the Naturalists. Victor Wolfgang von Hagen, Ed. xix + 392 pp. $5.00. Greenberg, New York.

CONSIDERING the extent of the literature on the natural history of South America, Mr. von Hagen has chosen wisely for his splendid anthology. The selections, arranged in chronological order and carefully annotated, cover the continent from Panama to Tierra del Fuego, from Brazil to the Galápagos. The subjects include trees, plants, fruits, animals, insects, birds, reptiles, fish, and even geology and anthropology.

The early reports of the world behind the "Green Curtain" are enchanting for the archaic spelling and quaint expressions, and equally so for the amazingly acute—although not always accurate—observations. Pietro Martire d'Anghiera opens the book with his fifteenth-century Of the Supposed Continent. Next comes Gonzalo Fernández de Oviedo y Valdés with a wondrous description of ant (eaters) bears extracting termites from a nest of "suche hardeness that it may seeme a stronge pavement." Then José de Acosta ponders—close to the later-developed theory of evolution—"how it should be possible that at the Indies there should be anie sortes of beasts, whereof the like are no where else."

Exactly as it was impossible to include all the great ones in this book, so it is difficult here to do more than hint at the contents. There is Félix de Azara's remarkable story of the wild horses of the

Argentine and his eighteenth-century interest in genetics. You will find Charles Robert Darwin's account of the inhabitants of Tierra del Fuego in 1832. Alcide Dessalines D'Orbigny writes the beautifully poetic story of his travels. There is Henry Walter Bates for army ants, ants, and termites; Richard Spruce for rubber collecting and fabricating; Edward Whymper with a marvelous ascent of the towering mountains of Ecuador; William Henry Hudson in Patagonia; William Beebe on Falling Leaves; and H. M. Tomlinson in the "infinite, lofty" jungle. You would expect a fine article on birds from Frank M. Chapman; there is, too, his Making of a Cayuco. The latter part of the book is rich in fact, not only in natural science but also in literary merit. Thus, the final selection, Ivan T. Sanderson's story of monkeys, grison, opossum, and jaguar, is superb. The book as a whole is an interesting—even thrilling—addition to any library.

MARJORIE B. SNYDER

Washington, D. C.

The Royal Society: Newton Tercentenary Celebrations. xv + 92 pp. $3.00. Macmillan. New York.

PROBABLY no name in the chronicles of science is more universally recognized than that of Sir Isaac Newton, born at Woolsthorpe in 1642. His many contributions to mathematics, mechanics, and optics are probably overshadowed in the public mind by his outstanding discovery of the universal law of gravitation.

It is eminently fitting that the three-hundredth anniversary of the birth of this genius should have been celebrated in London and Cambridge. Postponed because of the war, the Tercentenary Celebrations actually occurred in July 1946. The volume is published under the auspices of the Royal Society of London, at whose invitation the national academies of science of the world joined in paying homage to Newton's memory.

In less than one hundred pages there are contained the program of the celebrations and the following addresses: Address of Welcome to the Delegates, by the President of the Royal Society; Newton, by Professor E. N. da C. Andrade; Address of Welcome to the Delegates, by the Master of Trinity (Dr. G. M. Trevelyan); Newton, the Man, by the late Lord Keynes (read from Lord Keynes' ms. by Mr. Geoffrey Keynes); Newton and the Infinitesimal Calculus, by Professor J. Hadamard; Newton and the Atomic Theory, by Academician S. I. Vavilov (read on behalf of the author by Sir Henry Dale); Newton's Principles and Modern Atomic Mechanics, by Professor N. Bohr; Newton: the Algebraist and Geometer, by Professor H. W. Turnbull; Newton's Contributions to Observational Astronomy, by Dr. W. Adams; and Newton and Fluid Mechanics, by Professor J. C. Hunsaker.

Throughout, one is reminded of the human traits of an individual who was at once a scientist and a man of affairs. It is a bit surprising to those less well acquainted with Newtonian literature to find that so many of his outstanding attainments represented so small a proportion of his lifetime career.

It is gratifying to note that the Royal Society has taken steps toward the establishment of an Isaac Newton Observatory to contain a 100-inch reflecting telescope that, it is hoped, will be granted by H. M. Government and that will be made accessible to the personnel of university observatories for the purpose of astronomical investigations, to which Sir Isaac Newton so significantly contributed.

The book contains six illustrations and is an important contribution to the history and bibliography of science.

HARLAN T. STETSON

Cosmic Terrestrial Research Laboratory
Massachusetts Institute of Technology

* * *

AFTERNOON ACTIVITIES

An unusual feature of the Centennial Celebration in September will be the opportunity to see "One World of Science" as it is illustrated in action in the nation's capital. The government of the United States has a tremendous investment in scientific research, and, although a major part of it is carried on elsewhere, it is only in Washington and its vicinity that examples of nearly every type may be found.

More than forty institutions are cooperating in arranging local functions, and they will be hosts to the visiting scientists. These institutions include American University, George Washington University, Georgetown University, Howard University, and the University of Maryland; societies affiliated with the AAAS who have headquarters in or near Washington: Academy of World Economics, American Association of Economic Entomologists, American Chemical Society, American Home Economics Association, American Pharmaceutical Association, American Psychological Association, American Statistical Association, Metric Association, National Association of Science Writers, National Education Association, Society of American Foresters, and Society of Rheology; governmental organizations: Bureau of Agricultural Economics, Bureau of Animal Industry, Bureau of Entomology and Plant Quarantine, Forest Service, U. S. Coast and Geodetic Survey, Weather Bureau, National Bureau of Standards, Bureau of Mines, Fish and Wildlife Service, Food and Drug Administration, National Institute of Health, U. S. Office of Education, U. S. Public Health Service, David Taylor Model Basin, National Naval Medical Center, Naval Ordnance Laboratory, Naval Research Laboratory, National Academy of Sciences, Office of Naval Research, U. S. Naval Observatory, National War College, Library of Congress, and Smithsonian Institution; also, Carnegie Institution of Washington and the National Society of the Daughters of the American Revolution.

For relaxation, those attending the Celebration will have an opportunity to make unscheduled trips to the many art galleries, libraries, museums, and historic monuments located in Washington. A guide to these and a map of the city of Washington will be provided at the time of registration. Since no activities have been scheduled for Monday afternoon, September 13, those arriving in Washington early may avail themselves of this opportunity to study the superb cultural displays situated along the Mall. The opening session, in Constitution Hall, will not begin until 8:30 P.M.

Special tours of institutions engaging in scientific research are being planned for the afternoons of Tuesday, Wednesday, and Thursday, September 14–16. The National Institute of Health, Agricultural Research Center at Beltsville, National Naval Medical Center, National Bureau of Standards, and many others will welcome visiting scientists and their families and conduct them on inspection trips through laboratories housing demonstrations that feature current research programs. Those who wish to participate in these tours may make their reservations at the time of registration. A schedule of trips to institutions will be published in the forthcoming announcement of the meeting soon to be mailed to members of the Association.

There will be much to attract specialists who may desire to see advancements in their own fields of science, but the important purpose of the afternoon activities will be to enlarge the horizons of all who participate in the Centennial Celebration, to bring together educators, research workers, and others interested in advancing human welfare.

As chairman of the Afternoon Activities Committee, it gives me great pleasure to invite the members of the Association and the members of its affiliated societies to join in the commemoration of the founding of the AAAS, and to extend to them a cordial invitation to visit the great scientific and cultural institutions in and about Washington.

Raymund L. Zwemer

Executive Secretary, National Academy of Sciences, and Chairman, Afternoon Activities Committee, AAAS

THE

SCIENTIFIC MONTHLY

AUGUST 1948

THE MAGNIFICENT RODENT*

H. RAYMOND GREGG

Mr. Gregg, a graduate of Hendrix College, Conway, Arkansas, is chief of the Naturalist Division, National Capital Parks He has made many contributions to the field of nature education, including the Junior Nature School, Rocky Mountain National Park, one of the pioneer efforts in outdoor education for juveniles. His "Nature Sketches" broadcast from the Park were an NBC network feature for six years prior to his transfer to Washington.

AS RECENTLY as forty years ago, when few people had the opportunity to visit the remote remnants of wilderness in our country, it was only an occasional person who had seen a beaver in the wild, or observed its works. The once-fashionable gentleman's beaver hat had become a collector's item, and a fine beaver coat for milady was a faint social will-o'-the-wisp. To most people, the beaver had become largely a legendary symbol of industry. Yet, in the America of our fore fathers, this oversized rodent inhabited the whole continent north of Mexico, except the riverless wastes of the Great Basin, the frigid Arctic tundras, and the extreme southeastern coastal plain.

The sheer weight of aggression by a people who cleared the land, polluted streams and diverted their waters, and exercised strong intolerance of wilderness neighbors inevitably must have reduced greatly the range and population of beavers. But it was not slow pressure of displacement that brought the beaver to its sorry plight in 1900. A hundred years of persistent trapping for the market dangerously lowered populations while vast acreages of habitat suitable for beavers remained relatively unimpaired. Four phenomenal decades of expansion, exploitation, and social excitement between 1805 and 1845 brought feverish and far-flung activity by trappers and fur traders. By 1850, almost throughout the United States, beaver trapping had reached a point of diminishing returns, and the species approached or had reached extirpation over much of its former range.

There are published estimates of aboriginal population as high as 400,000,000, but 60,000,000 is a more acceptable figure. Such estimates necessarily are based upon limited data, and probabilities. However, on a comparable basis, possibly the 1900 beaver population for the United States proper could be covered in five figures.

The ground swell of conservation consciousness of the early 1900s brought attention to the beaver as a vanishing species. Writers, some of them more enthusiatic than accurate, stimulated widespread interest in the beaver and its way of life. By World War I, stringent or total protection was provided by law almost everywhere. Widespread efforts were begun to restock depleted territories, or even to extend the known range of beavers. The ensuing years have brought fruitful results. Today, relatively few Americans live more than a day's drive from a wild-beaver colony.

A 1940–41 survey of eleven Pacific Coast, Rocky Mountain, and Great Basin states reported a beaver population of 324,000. During that same year, removal of nuisance beavers in those states produced 53,936 pelts, worth $1,096,659. Also in that year,

---

*** Photograph on page 78 by Paul Nesbit. All others by National Park Service.**

even in the former "dust-bowl" state of Kansas, 368 beavers were trapped. New Mexico and North Dakota, unassociated in popular thinking with beavers, harvested 1,160 and 1,800 beavers, respectively. The year before, 72 were taken in Texas! Restoration of beavers has not been in the West only. In Maine, 3,049 beavers were taken during 1940–41; 1,023 in New Hampshire; and 1,195 in Pennsylvania. At the Sixth North American Wildlife Conference in 1941, Miss Fannye A. Cook, of Mississippi, reported removal of beavers to placate complainants in that state. Virginia was progressing with re-establishment of beavers.

Table 1 presents data for 1946 from states that responded to a questionnaire. Since the early 1930s, Michigan and Wisconsin have developed a sustained-yield fur resource and have compiled a wealth of information basic to sound management. G. W. Bradt (*Michigan Beaver Management*, Michigan Conservation Commission, 1947) and N. R. Barger (A Peak in Wisconsin's Beaver Harvest, *Wis. Conservation Bull.*, November 1947) have described the programs in those states.

In Michigan, 15,296 beavers were trapped in 1945. In 1946, it was still possible to harvest 9,859 pelts, although the season was closed in 1947 as a precaution against overdrawing breeding stock. Price stimulus (the 1946 take, at an average of $40 per pelt, grossed $394,360) may have induced harvesting beyond the productive capacity of the population. However, in seven years, 1938-44, with the Upper Peninsula closed in 1942 and 1944, there was an average annual yield of 5,663 pelts. It is likely that improving management and trapping practices will bring consistently higher yields.

Between 1936 and 1943, Wisconsin averaged 4,571 beaver pelts, including a zero figure for the closed season of 1940. The annual harvest varied in direct proportion to pelt prices. This was most striking when the 1944 figure jumped to 7,720 from a total of 4,564 in 1943. The season was closed in 1945. Then, in 1946, a harvest of 15,280 beavers was taken, bringing gross revenue of $702,880!

As a final statistical tribute to the capacity of beavers to thrive when aided by man, consider the case of Montana, where 10,200 pelts were taken in 1946, even after peak harvests of 12,752 in 1940; 17,717 in 1941 (a modern record for any state); and 11,161 in 1942.

Probably two factors contribute most to the beaver's capacity for survival and recovery: adaptation to life in an aquatic environment, and partially concomitant relative ineffectiveness of predators other than man.

Under conditions favorable to the predator, most of the land carnivores can kill a beaver. Probably wolverine, cougar, lynx, bobcat, coyote, and wolf have been most effective. Fisher, otter, and even mink, all capable swimmers, are proved or probable predators, but the extent of their predations is unknown. The primitive abundance of beavers, when predators also were abundant, indicates good native ecological adjustment.

The mature beaver is a formidable adversary when attacked. Even man is not immune (although game wardens are not taken in by trappers, caught with illegal pelts, who plead self-defense). The beaver has strong, sharp incisor teeth and powerful jaws. The body is stocky, thick-necked, tough-skinned, with low center of gravity—all excellent defensive traits. A record California Golden Beaver exceeded 80 pounds. A male, drowned in live-trapping operations in Rocky Mountain National Park, weighed 55 pounds; it was not unusually large for that region. Accidents such as crushing by falling trees, internecine warfare, and internal parasites probably account for more beavers than predation.

Adaptable to a great variety of aquatic and wet-association food plants, and tolerant of great variations in temperature, the beaver had one of the greatest aboriginal territorial ranges among mammals of this continent. Southern sloughs, rivers penetrating desert lands, and cold streams and lakes of high altitude and latitude alike are capable of supporting colonies. It may almost be said that in the extreme environments the beaver inhabits, there are in common only water and others of its kind.

Large, webbed hind feet are powerful means of propulsion. The broad, horizontally flattened, nearly hairless tail serves as rudder and balance. The dual coat consists of long guard hairs that mat down into a "diving suit," and soft, shorter inner fur. Flaps behind the strong, orange-colored incisors effectively exclude water during cutting operations under water. Nostrils are furnished with valves, and beavers can retain and conserve oxygen enough to remain submerged for reported periods of five minutes or more. The ears also are equipped with "valves." These all are obvious adaptations for water life. There may be subtler factors of metabolism, body temperature control, ability to receive vibrations under water, and underwater vision, of which there is insufficient knowledge.

There is neither need nor space here for detailed physical description or thorough discussion of life history and habits. On these phases of beaver life, Morgan, Seton, Mills, Bailey, Warren, Grinnell, and others provide most of the known facts. Personal observations and deductions, and acceptable

TABLE 1

COLLECTED DATA ON POPULATIONS, HARVESTS, REVENUES, TRAPPING METHODS, PELAGE

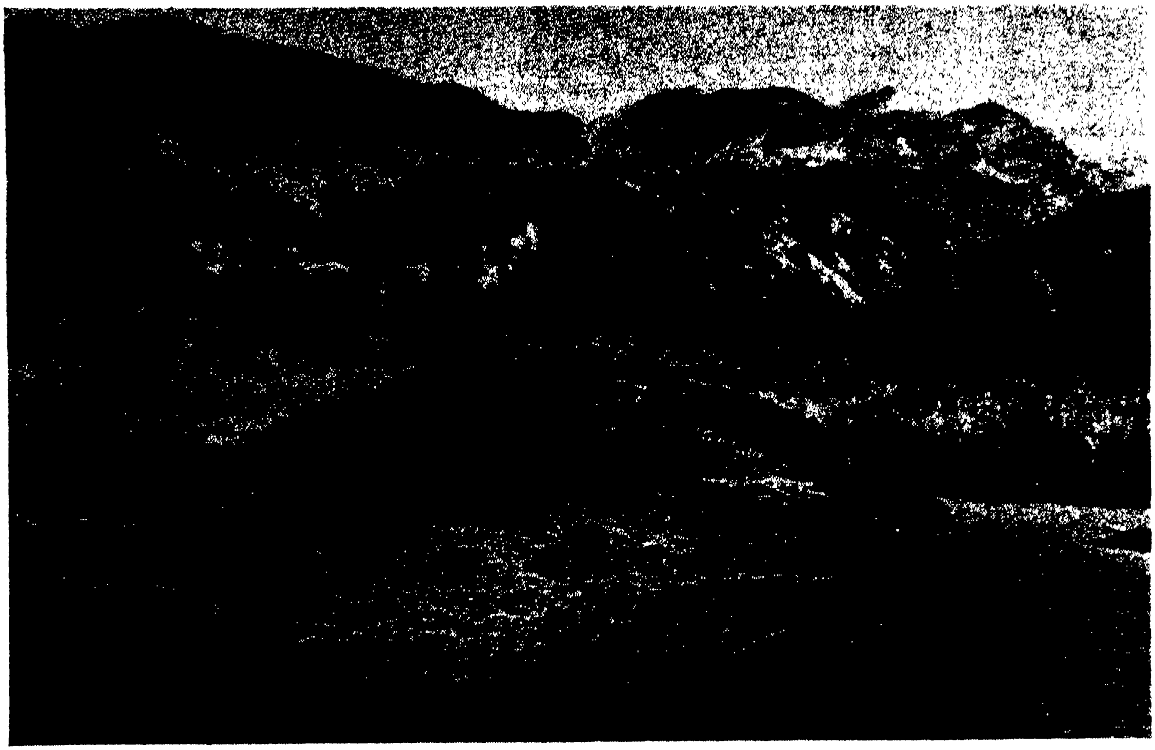
State	1946 Population	1946 Harvest	Gross Value	State Proceeds	Method of Trapping	Do Black Pelts Occur?	Do Albino Pelts Occur?
California . .	25,000	400	$ 4,800	None	Damage permits, licensed trappers	No	No
Colorado	40,000	8,640	$ 272,323	½ private, all public	State trappers	1–1,000	"One since 1941" (1 in 50,000)
Idaho .	42,500	7,192	$ 144,147	$45,140	State and farmer trappers	Frequent in No. Idaho	No
Indiana	5,000	None	None	None	None	No	No
Maine	30,000	4,658	$ 130,000	$ 9,316	Licensed trappers	No	No
Michigan . .	50,000–100,000	9,859	$ 394,360	No figure	Licensed trappers	1–1,000	Not definitely known
Minnesota	40,000–	8,283	$ 172,935	$1 per pelt	Permittees, licensed trappers	Occasionally	Yes, rarely
Montana	No figure	10,200	$ 255,000	No figure	Permittees	1–200	Very rarely
New York .	6,358	5,567 !	"$ 1 per sq. inch"	None	Licensed trappers	No records—very dark individuals occur	No records
Oregon	30,000	3,005 289	$ 141,940 None	All None	State trappers Indians	1–30	2–3,000
Utah .	No figure	3,000 (est.)	Unknown	No data	Permittees	No	No
Washington	100,000	5,248	$ 162,000	All	State trappers	10%	No
Wisconsin	No figure	15,280	$ 702,880	No data	Licensed trappers	1–1,000	None recorded
Wyoming .	No figure	10,761	$1,188,925	$ 9,457	State trappers, permittees	5–1,000	No

secondhand information concerning beaver "engineering" and other works, accruing through ten years in Rocky Mountain National Park, Colorado, largely compose the remainder of this article.

FOOD being of first importance, some observations on feeding are apropos. Generally, summer diet includes much more herbaceous material than that of winter. However, considerable cutting and some consumption of woody plants, even trees of some size, take place in summer. It is known that the rapid growth of incisors in rodents generally requires that a certain amount of gnawing be done to keep a balance of wear and growth. Likewise, in winter, when bark and tender wood are favored staples, rhizomes and fleshy roots of plants growing in frost-free soil or pond muds are eaten in surprising quantity. On many occasions, especially in late fall and early spring, when water is partly open, fresh refuse of herbaceous foods has been found. Once, at midday in early winter, two beavers were under observation for more than an hour as they repeatedly dived, cut pond-lily stems, and brought them out onto the edge of the ice to feed. The upper stems and fragmentary leaves were not used. Incidentally, these animals almost invariably folded the tail under and sat upon it during periods of as much as five minutes on the ice between dives.

Aspen and, in lower elevations, cottonwood are staple food trees in the Rockies. In high-altitude valleys where aspen is scarce or extirpated, willow is used almost exclusively. Everywhere, willow is consumed regularly even if cottonwood or aspen is abundant. In summer it commonly is used as a convenient browse, taken most often from stems growing at the water's edge. Cutting of aspen and cottonwood, involving more time on land, is at a minimum during the summer, perhaps because of greater predator activity.

There are few species of woody plants growing near beaver-inhabited waters which are not used at some time. Cutting of species of alder, birch,



BEAVER ISLAND LODGE WITH WINTER FOOD PILE BESIDE IT

CLEARED PATHWAY FOR TRANSPORTING BRUSH AND POLES FROM AN ASPEN GROVE

THE BEAVER'S TAIL IS FUNCTIONAL

FLAT, AND NEARLY HAIRLESS, THE TAIL IS USED AS A BALANCE AND RUDDER IN SWIMMING, AS A PROP WHILE CUTTING TREES, AND FOR THUMPING THE GROUND OR SPLASHING WATER, PRESUMABLY AS A SIGNAL.

maple, serviceberry, *Cornus, Sorbus,* and *Prunus* has been observed. Cutting or gnawing also has been found on all conifers native to the central Rockies. Use of less-preferred hardwoods and conifers is not necessarily from hardship. Often they are taken while cottonwood, aspen, or willow is still readily available. Such feeding has been described as tonic, parturient, or connected with fertility. Seasonal occurrence of use does not satisfactorily bear out these explanations.

Woody plants are important not only as food, but both directly and as by-products of feeding, in construction of dams and lodges. These structures contain quantities of mud, rocks, vegetable debris, even animal remains, and, in these civilized days, tin cans, beer bottles, wire, saddle stirrups, burlap bags, roofing paper, and other movable items left within reach. The supporting strength and bulk of most structures, however, are provided by poles, predominantly 1–5 inches in diameter and 3–6 feet long.

In initial construction, a dam contains many poles with bark on, cut and transported exclusively, or almost so, for this use. Much fine brush also is used at this stage. Later repairs and gradual increments are likely to be made with poles peeled during the preceding winter.

In late summer or early autumn, the male of the family or his successor ends his summer wandering. The colony starts preparation against the hazards of winter. Dams weakened by summer floods, ignored while constant flow provided stable water depth, now receive attention. New mud is added to the outer walls of the lodge. Winter temperatures change this to the consistency of concrete, providing additional structural strength and making it difficult for land predators to dig into the lodge.

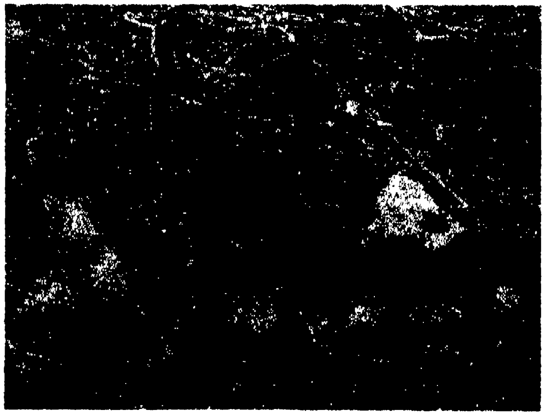
In the vicinity of the home pond, great numbers of trees are cut in the energetic harvest. This is done with little application of skill beyond patient biting and prying of chips with teeth until the tree falls. Allowing for wind, balance of crown and angle of inclination of the trunk are the only acceptable controlling factors in the direction of fall. Many trees entangle with others near by when cut, and are useless unless, by chance cutting, the supporting tree or trees are felled. Several instances have been discovered of trees with crown so tightly enmeshed that when the trunk was completely severed, it dropped from the stump, remaining suspended vertically, responsive to touch or wind, as a free-swinging pendulum.

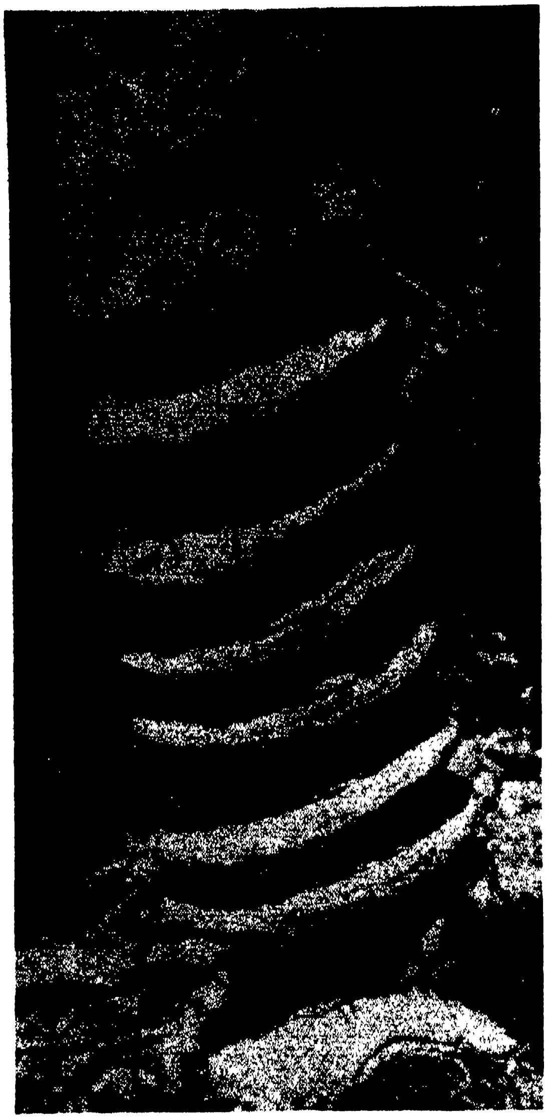
One instance was encountered where felling of a second tree released one previously entangled. The former was trimmed of branches, the trunk cut into sections and removed. The released tree, still fresh, was left untouched. So much for the fancy that cutting one tree to release another is a studied act. The tree was not a total loss, however. Wapiti made short work of most of the bark and twigs of the neglected tree.

It is common to find cuts of varying depth and height on a tree, as if beavers of different size had worked each at its own level in a race to cut it; or as if a single beaver had sample-cut at various levels until the most comfortable and efficient working position was attained. Trees with half a dozen or more "trial cuts" are not unusual. Winter cutting above deep snow leaves tall stumps, some as high as 6 feet.

Perhaps the beaver's reputed poor vision explains this action. One autumn, I discovered a 5-inch aspen stump about 3 feet high, cut during the preceding winter. Shortly before this observation, a beaver had almost severed it at a lower level before abandoning the job or discovering that this was a stump with only about two feet of trunk as

DEBRIS REMOVED FROM A BEAVER DAM





TREE SPIRALLY WRAPPED WITH WIRE

BEAVERS CHANNELED OUT BARK AND SAPWOOD BETWEEN THE LOOPS OF WIRE SEVERAL FEET ABOVE THE GROUND.

reward for effort that as well could have cut an entire tree.

Cutting on hard deadwood, as suggested earlier, probably is done to condition the teeth, and could be called "exercise cutting." It has been observed where no other apparent purpose could be served, unless there is nutritive or medicinal value in such wood. Pitch-hardened fence posts have been cut at the base, although they were no obstruction or hindrance to work under way. In one case, such a post was cut at three places, below and between three strands of wire, so that three fragments dangled independently, each suspended by a staple. This reflects persistence difficult to explain, in that at least the top cut must have been made at a time earlier or later than the others, when snow was deep enough for access to that level.

Paul Nesbit, of Estes Park, Colorado, has photographed an unusual example of cutting. To protect a valued tree, the owner wrapped heavy wire spirally around the lower three or four feet. Beavers meticulously cut out a spiral channel between the twists of wire.

In the museum at Longs Peak Inn, near Estes Park, is a small log gnawed into a series of wooden "beads" of croquet ball size, "strung" on a narrow core of uncut heartwood.

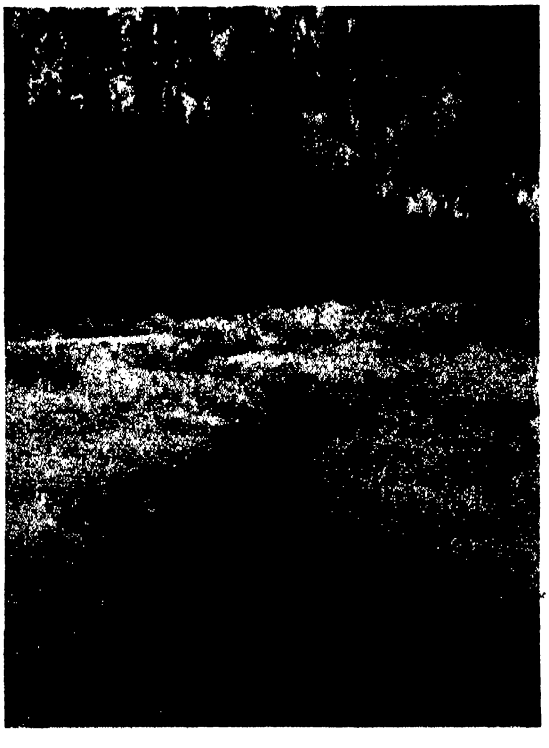
Felling is accomplished by beavers singly or cooperatively biting into the trunk until the tree falls. The cut may encircle the trunk, leaving a near-symmetrical stump, or it may be entirely from one side, producing a beveled stump. When the tree is prostrate, branches are cut and consumed or removed for storage or immediate construction work. The trunk is cut into sections, usually up to 4 feet long in trunks averaging 3–5 inches in diameter. Because of natural ground slope toward water, transportation is largely downhill. This makes slightly less astounding some of the pushing and dragging feats of beavers.

The food storage pile consists of log sections, poles, branches, and finer brush, adjacent to the lodge. Larger, tender-barked poles predominate near the bottom. At the top, brushy tips extrude from the surface of the water, where ice grips them to anchor the pile securely through the winter. The beaver has only to descend by a passageway from the living chambers to the floor of the pond, swim to the food pile, cut off a desired portion, and bring it into the den to be eaten. Uneaten remains are released outside beneath the ice. Toward spring, raftlike masses of peeled sticks may be seen through the ice, drifted against the dam.

Materials may be transported, largely by water, more than half a mile upstream or downstream. Overland transportation more than 400 yards has been observed. Cleared pathways often are prepared. Usually they are well-worn dragpaths from groves that recede from the water's edge as cutting progresses. Clearing these paths involves such things as removing sections of fallen logs, cutting trench-shaped troughs across them, or tunneling under them; tunneling through an abandoned dam; removing or gnawing away exposed roots; cutting off projecting snags or brush that overhang or intrude into the pathway; and excavating an inclined ramp down a steep bank to the water.

Water transportation, safer and easier than land travel, is much used in bringing food and construction material to its destination. If land is level, canals may be dug. The simplest form is a landward extension of a natural indentation in the bank. Earth and stones are excavated with the paws. Roots and buried logs are gnawed away. In extensive flat land, canals may be several hundred feet long. Lateral canals may extend harvesting into various parts of a grove. Dry or watered tunnels often connect near-by canals or laterals. Canals vary in depth from 3 feet or more to a few inches at the shallow end farthest from the pond. They range from about a foot to more than 4 feet in width. Excavated troughs on the pond bottom usually connect the canal with food pile, dam, lodge, and through channelways along the stream axis.

When rising ground or bedrock conditions stop landward extension of a canal, it may be carried farther afield by excavating at a higher level. This involves construction of separate canals connected by portages. More commonly, the canal floor rises gradually, and at a point where water becomes too shallow for safety, a cross-dam is installed, acting as a "lock." Usual objective is a grove of water-loving species of trees, where the high water table provides adequate water for the upper level.



BEAVER CANAL

SEVERAL HUNDRED FEET LONG, WITH A MAXIMUM DEPTH OF ABOUT 30 INCHES AND A MAXIMUM WIDTH OF OVER 5 FEET.

PENDULUM TREE

TIGHTLY ENMESHED CROWN PREVENTS TREE FROM FALLING.

In Windy Gulch, Rocky Mountain National Park, in 1939, there was a dug canal with three successively higher cross-dams, connecting a home pond with a grove of aspens on the east slope of the valley, about 175 feet away. At the grove end, beavers had excavated a natural spring to make a pond about 15 feet across and more than 2 feet deep. A low dam around this pond actually created a fifth level to the waterway, just a few inches higher than the water impounded behind the uppermost cross-dam.

In Mill Creek Valley, a few miles away, other beavers built an arc-shaped dam around a muddy spring that had been an elk wallow. Between this developed reservoir and the main stream below were canals and small pools created by damming the overflow from the higher reservoir. Trees near the upper pool were being cut and transported to the home pond, a hundred yards distant, using this artificial waterway. In view of these observations, stories of beavers diverting stream water into parallel canals supplying high-level ponds downstream, in much the same way man diverts and impounds irrigation water, seem reasonable.

An entire stream course may become in effect one extensive system of "lock levels." On Cub Creek, Rocky Mountain National Park, in 1938, there were seventy-six impounded levels within about two miles, between its junction with the Thompson River and its source in Cub Lake. The dams ranged from over a thousand feet to only a few feet in length, but none was more than 6 feet high. Some ponds covered more than an acre, others only a few square feet. Almost without exception, water behind one dam backed directly against the dam above.

Dams may be quite long, as on Cub Creek, or may be 10 feet or more in height on the downstream face. Such massive structures usually are the cumulative product of generations of beavers. They seldom are begun in water over 2 feet deep, especially in swift water. Dams may be constructed "from scratch," with all the material brought in, or advantage may be taken of snags, fallen trees, and projecting or island boulders.

The beaver dwelling varies from a simple bank burrow with living chamber above water level to a complex, several-chambered island lodge which may be quite large. One lodge in Cub Creek Valley, at its prime in 1936, was over 35 feet on its longest axis at water level. It stood approximately 8 feet above the surface in water almost 4 feet deep. During a short-wave broadcast from this lodge in 1939, a radio announcer and I, together weighing over 400 pounds, and some forty children averaging about 60 pounds each, put almost 3,000 pounds on it without damage. Incidentally, muskrats were living in tunnels in the outer walls, in apparent mutual tolerance with the beavers. In grass on the lodge just above water line was a spotted sandpiper's nest.

Comment on the complex ecology of the beaver environment can receive only passing notice here. No valid evidence of beaver eating fish is known to me. If such behavior occurs, it is not typical. The beaver probably is as nearly a strict vegetarian as any mammal.

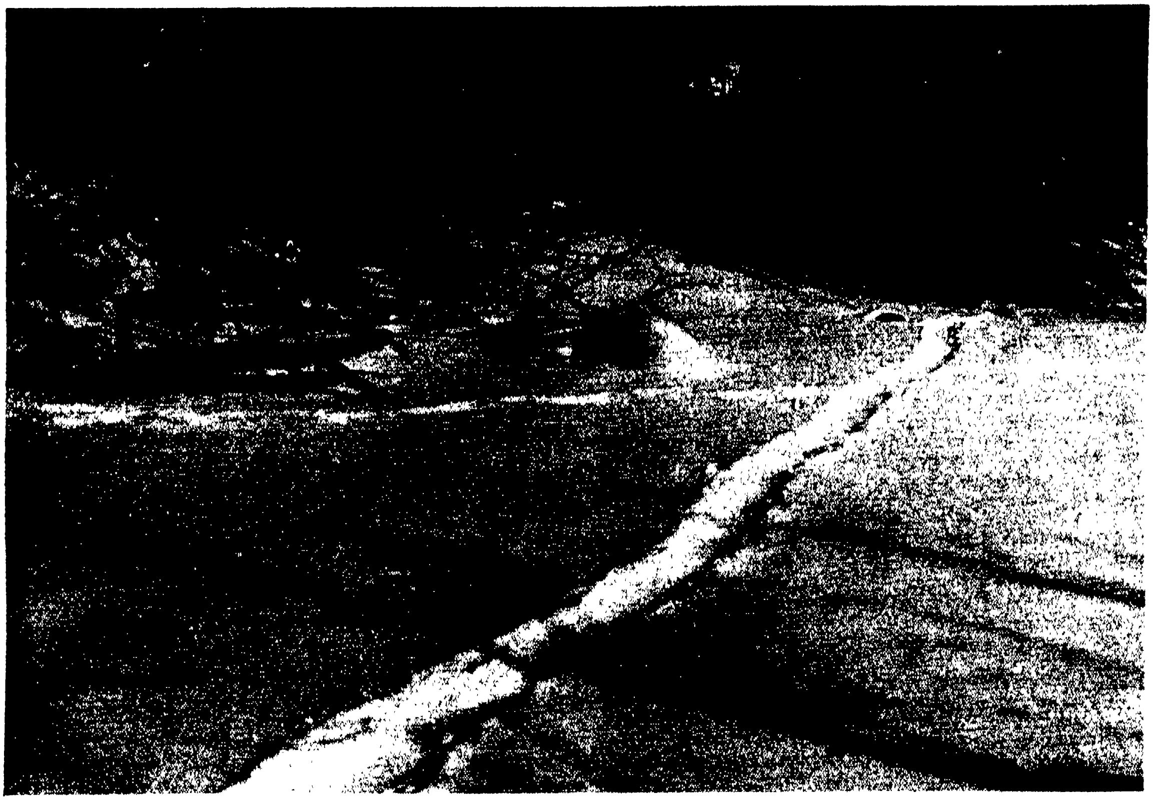
Deeper water impounded by beavers normally provides richer plankton and other fish food. Flanking marshes support increased insect life around beaver ponds. Warmer summer water temperatures and diminished current are detrimental to some species and certain activities of fish. However, fishermen generally consider active beaver colonies an asset on a stream.

Impoundment of water affects man when roads, trails, buildings, wells, and agricultural or grazing lands are flooded. However, the beneficial effects of beaver-made lakes and ponds upon water table and stream flow more than compensate for local damage in ordinary wild lands. Had there not been thousands of generations of beavers to construct myriad dams to slow channel cutting in several of the most picturesque glacial valleys in Rocky Mountain National Park, unchecked erosive forces long since would have reduced them to gullied, impassable, boulder-strewn wastes.

A colorful account of beaver stubbornness in maintaining impounded water was related by the late Abner E. Sprague, of Estes Park. It is repeated here from a manuscript:

> In order to build a road to our place in Glacier Basin, we had to cross a brook, of which the pesky beaver had made a slough. To save labor, I used an old dam as part of the roadbed. It was decayed and partly washed out, abandoned by the beaver. I made a small bridge for the water to pass through, and constructed a road, about fifty feet long, over the miry ground. Shortly after it was done, Mr. Beaver found it was a snap to fill under, and at the upper end of the bridge, thus forming quite a pond, but not enough; so to make a good job they began to build the roadbed higher.
>
> I fought them for a few summers, tearing out their work two or three times a week. Finally, with chicken wire and heavy rock, I kept them from filling under the bridge. Then they built a dam about fifty feet below the road and flooded it again, and again began a dam on the road. . . . I had to tear a hole in the lower dam at least two or three times a week to keep the road dry. I sent men down once who tore out and piled up the entire works; no good—in a week the water was over the road again, and in repair work they had not used a stick of the old dam.
>
> What to do but keep on tearing a hole in the dam I could not think until one day I saw along the road a length of corrugated culvert pipe, left by the National Park road men. I borrowed this iron pipe which was about sixteen inches in diameter, and fourteen feet long. I dug a hole in the dam, and laid the pipe in, making it tight, neither the upper or lower end being supported. Enough water ran through so the material they put in would wash through. I visited the dam for a few times, and passed along the road nearly every day. I began to feel proud of myself in outwitting their chief engineer. But in about a week as I came in sight of the dam, I saw the upper end of the pipe, high and dry, aimed about forty degrees above the horizon and the road covered with water. They must have sent an old fat Beaver or two to sit on the lower end and tip it up, while the workmen stuffed sticks, moss, and mud under the upper end to hold it up. We gave the pipe a bearing the entire length, except about three feet that stuck out into the pond, so they would not build a dam around the end.
>
> . . . . I thought we had them stumped, but not for long. One day in passing, the road was covered with water, and the pipe was jammed full, not a drop of water going through. How did they do it? I found out in about an hour's work cleaning it out. Either by accident or design they had cut a small sprangly lodgepole pine and drawn it into the pipe butt end first; the spring of the limbs holding it in place, they had a foundation. The rest was easy. If the pine was an accident the first time, it was not after that, for that pipe had to be cleared every day, and always one little pine tree was used.

Mr. Sprague's experience truly presents beavers



BEAVER TRACKS IN SNOW, SHOWING COYOTE TRACKS CROSSING AT CENTER

CONICAL MUD HEAPS ON ROCKS IN BEAVER POND, ROCKY MOUNTAIN NATIONAL PARK as ingenious fanatics about loss of water. An unfortunate incident resulted from this propensity some years ago in Rocky Mountain National Park. A beaver colony flooded private land, requiring removal of the offenders. In urgent cases, the animals are live-trapped for transplanting. On this occasion, a section of dam was removed. A Bailey trap was placed in the opening so a beaver would enter it in placing sticks in the dam. This worked perfectly—except that after one beaver was trapped, others repaired the dam in such a way that the cage was completely submerged. The imprisoned beaver drowned, victim of the well-meaning labors of its co-workers.

Beavers make careful preparation against the necessity of going about in winter, but there is some activity outside at that time. One has but to make frequent winter visits to colonies to find where beavers have emerged from the home pond. This usually is done at or near the spillway, where lowered water level may allow egress, or where thinner ice may be broken through by pushing with head and back. On one occasion in early winter, an almost perfectly round hole some 14 inches across, and about 2 feet upstream from the dam, was found in ice more than 2 inches thick. There were freshly peeled sticks upon the ice beside the opening, where a beaver had sat to feed. How such a perfectly round hole was made is puzzling. The fact that it was maintained through 2-inch ice by regular use reflects the reluctance of beavers to retire for the winter to the confines of the lodge and underwater environs.

Even in deep snow, beavers occasionally flounder about, cutting and eating small trees and shrubs, even while there is a superabundance of food in the home pile, much of which may be abandoned unused in the spring. Tracking has shown that winter travels occasionally cover some distance. Longer trails more often have been observed along pondless stretches of streams, suggesting that wider-ranging travel is more common among lone males which frequently live in banks of deeper holes and are less provident in storing food near the home den. Fred M. Packard in "Beaver Killed by Coyotes" (*J. of Mammal.* 21, 359, 1940) describes a case of winter predation upon a vagrant beaver, reconstructed from evidences examined by Packard and Gregg.

In the autumn of 1936, some unusual work was observed on Cub Creek. In a roughly circular pond, about 35 feet across, twelve to fifteen boulders rose slightly above the surface. Upon most of these rocks, and at points along the banks, were conical heaps of mud, small sticks, and trash. They averaged 20 inches in base diameter and 12–14 inches in height. One heap, apparently four separate piles fused into one, was nearly 2 feet high, and over 4 feet on the longest base line.

The normal "sign heap" is a flat, "slicked-off" mass of mud almost devoid of sticks and debris, upon which excretion of the castoreum glands is spread, presumably as an olfactory advertisement associated with mating. Usually it is in shallow water or upon the adjacent bank, round to oval in outline, and about a foot in diameter. One on Fall River in Rocky Mountain National Park, about 12 by 20 inches, was unusually large.

The Cub Creek heaps may have been merely excavated material, fortuitously disposed in many similar lots. This seems improbable, in the light of extensive examinations of beaver colonies without discovery of such heaps elsewhere, although innumerable instances of disposal of debris have been observed. The "sign heap" is the most logical explanation of these heaps, justifying suspicion that this was an abnormal manifestation of the reproductive pattern.

Compared with the phenomenal fecundity of some lesser rodents, the reproductive rate of the beaver is low. Its adequacy, however, is attested by cited figures of current populations and harvests.

Young are born in the spring, usually two or three in young females; as many as six in prime maturity. The presence of four developed mammae suggests this as near the number of the average litter, a presumption supported by observations and review of the literature.

The young of the previous year normally remain in the lodge, largely self-sufficient, through the summer of their second year. They live compatibly with the mother and offspring of the current year. The male either leaves or is expelled from this company and spends a presumably pleasant summer as a vagrant, free of domestic responsibilities. This relationship better supports the now generally accepted belief that the species is polygamous than the earlier concept that beavers are strictly monogamous. In any event, it has its points for the male of the kind and, more important, it seems to have succeeded for beavers.

A magnet of wealth that was important in drawing westward the vanguard of a growing nation; a creature wondrous in way of life; at once a benefactor and a nuisance in its stubborn hoarding of water; accepted friend of the fisherman; land-builder and counteragent against erosion; an indomitable race that has defied extinction to become an important economic asset today—the beaver well deserves to be called the magnificent rodent.

A SIGNIFICANT DECADE IN SCIENCE

JOHN W. OLIVER

Dr. Oliver (Ph.D., Wisconsin, 1917), whose special field is the history of applied science and technology in the United States, is head of the Department of History at the University of Pittsburgh. He is chairman of the Advisory Board for the Historical Staff writing the History of Research and Development for the Navy and was formerly director of two Army historical research projects.

Senator John C. Calhoun, of South Carolina, greatest philosopher-statesman of his time, declared late in the year 1841: "We have, Senators, reached a remarkable point in the progress of our civilization, and the mechanical and Chemical arts, which will require a great change in the policy of our civilized nation."

Senator Calhoun was merely echoing what many of his contemporaries were saying during the opening years of that decade. The scientific interests of the young nation were beginning to stir. A whole host of scientists, inventors, and engineers had for years been laying the foundation for a scientific awakening. Not only had they been pleading for a better understanding of science, but, more significant, they were, a century ago this decade, pointing to the great possibilities that lay ahead.

There was abundant reason for this optimism. A brief roll call of some of the nation's top scientists and of a few major inventions of that period may be of interest as we now consider the background of the organization of the AAAS, on September 20 one hundred years ago.

Charles Goodyear had just recently announced his success in vulcanizing rubber—an invention that ranks among the greatest in the history of American technology. Cyrus McCormick's reapers were put on the market in 1840, and he sold them that year for $50 each. Four years later, he had raised the price to $100—and sold fifty-two reapers. Farmers and agricultural inventors were beginning to think and plan in terms of machinery rather than hand tools.

1840

The most significant event of the year 1840 was the founding of the National Institute. Its object, as announced by Joel R. Poinsett, one of its founders, was "to promote science and the Useful arts." Its plan of organization was so comprehensive and so well thought out that it served as a model for the organization of the Smithsonian Institution six years later. In the beginning the idea was to have the National Institute take over the Smithson bequest and administer it for scientific work. Its officers included many prominent government officials—cabinet members, congressmen, and senators. President Van Buren became "The Patron *ex officio*." Joel R. Poinsett, Secretary of War, was its president from 1840 to 1845. Several bills were introduced in Congress aimed at having the National Institute take over the recently acquired Smithson bequest. But Congress would not go along, and, after four years of fruitless effort, the National Institute went out of existence. But it had served a useful purpose: it had awakened an interest in science. John Quincy Adams said he could anticipate no happier close to his public life "than to contribute by my voice and vote in sustaining the National Institute, devoted as it is to the cause of science."

Other important scientific developments during the year 1840 included an announcement by Professor John W. Draper, of New York City University, that he had achieved first real success in taking an excellent photograph of his sister. Also, the first furnace in the United States using the hot-blast method of smelting iron ore with anthracite coal went into operation that year. It was set up in the Lehigh Valley and remained in blast until the flooded Schuylkill River quenched its fires in 1841. In 1841, Douglas Houghton issued his first topographical report of the Lake Superior region, thus informing the outside world of the importance of the copper deposits in that territory.

1843

A major scientific event of the year 1843 was the survey James Pollard Espy—a distinguished mathematician and meteorologist, known as "The Storm King"—made in laying the groundwork for a National Weather Bureau. Winner of the Magellan Prize, awarded by the American Philosophical Society, Espy had been invited to address the British Association for the Advancement of Science in 1840. He was later appointed meteorologist by both the U.S. Army and Navy. He established a series of daily weather observations, compiled weather maps, traced the progress of storms, and, in 1843, submitted the first Annual Weather Report in this country.

1844

The year 1844 witnessed a number of important scientific developments. This was the year that S. F. B. Morse finally succeeded, after a long series of experiments, and with the aid of Federal money, in setting up his telegraph line from Baltimore to Washington, and ticked out the historic words "What hath God wrought."

The same year, Alvan G. Clark and Sons ventured into the business of manufacturing telescopes. During the year that followed, the company was asked five different times by the Navy Department to grind "a telescope lens more powerful than any in existence." The company soon became known as the most expert lens grinder in the world, and orders came in from all parts of the globe.

The most significant scientific event in 1844 was the holding of America's first scientific congress, called by the National Institute. Several attempts had been made prior to 1844 to organize scientific congresses on the plan of the British Association for the Advancement of Science, but without success. Finally, the National Institute decided to attempt a meeting in Washington on a nation-wide scale. The date was set for the first week in April 1844. Invitations were sent out to members of the American Philosophical Society, the Association of American Geologists and Naturalists, and all other scientific and learned societies. More than one hundred persons were invited to prepare and read papers.

On April 1, 1844, at ten o'clock, the members and guests of the National Institute, led by President John Tyler and accompanied by the National Marine Band, formed in procession and marched to the Presbyterian Church on 4½ Street. After the opening prayer, President Tyler spoke, calling attention to the fact that this was the first general scientific meeting of men from all parts of the United States.

President Tyler was followed by the Honorable Robert J. Walker, United States Senator from Mississippi. In his keynote address, Senator Walker paid grateful tribute to "the untiring votaries of science." "The greatness of our nation," he continued, "depended upon the advances it made in the field of science. Science, by subjecting nature to the wants of mankind had enlarged our knowledge and provided man with increased comforts."

Senator Walker demonstrated a thorough acquaintance with the accomplishments of American men of science. He reviewed in detail the work of Benjamin Franklin in the field of electricity; of Joseph Henry and his current experiments, also in electricity, especially in electromagnetism; of Robert Hare on the calorimeter and the galvanic battery; of Professor John W. Draper; of S. F. B. Morse on the telegraph; of Dr. Alexander D. Bache in his extensive magnetic observations; of Joseph Saxton in providing for a continuous current; of Thomas Godfrey in making the mariner's quadrant of practical use; of Professor James Espy in meteorology; and others. Walker concluded with a stirring appeal to all scientists, urging them to unite their efforts in making "this country the greatest and freest nation of the world."

The meeting lasted from April 1 to April 8, inclusive. There were ten sessions, during which papers on astronomy, geology, natural history, physics, chemistry, meteorology, paleontology, entomology, pharmacology, physiology, and other subjects were presented and discussed. John Quincy Adams, ex-President and at that time a member of Congress, presided over the closing session. He announced that he was proud to have been a member of this First National Congress of scientists, and offered his apology for not having been present at all the sessions, owing to his presence being required at the Capitol.

Too little credit has been given to this early scientific congress, for it marked a distinct milepost in our history of science. Here was a group of men eager to unite the scientific forces of the young nation through the diffusion of scientific knowledge. This effort stands as "a landmark in the scientific and cultural history of the United States."

1845

The year 1845 witnessed a number of developments that might be briefly noted. First, this year marked the beginning of the *Scientific American,* one of the notable scientific periodicals in American historiography.

In February of that year, the Commissioner of Public Buildings in Washington was directed by Congress to negotiate a contract for lighting the Capitol with gas, provided it could be done at an expenditure of not more than one half the cost for lighting the building with oil.

A definite advance in road building occurred this same year, namely, the construction of plank roads. A plank road fourteen miles in length was built from Syracuse, New York, to Oneida Lake. This new road was hailed as a great improvement over the old dirt and corduroy roads.

This same year, Stephan Fitch introduced a new device for directing several tools successively against a piece of wood (later iron), thereby inventing the turret lathe. This was hailed as the greatest development in machine tools since the slide rest. With the turret lathe began the series of "automatics," in which tool after tool mechanically comes into play. From the turret lathe came the modern automatic-screw machine, and the typically American grinding machine.

For the boot and shoe industry the "rolling machine," invented in 1845, proved of great benefit. It soon took the place of the lapstone and hammer, used in pounding and toughening leather. This, together with the coming of the Howe sewing machine the following year, marked a great advance in leather technology.

1846

This was a momentous year in our nation's history. So many and so important were the happenings that Bernard DeVoto has devoted a whole book of some 540 pages to recording and interpreting them. He entitled it *The Year of Decision* (Boston: Little, Brown, 1943). DeVoto deals mainly with national and geographic expansion, but a similar volume could be written dealing with the scientific, engineering, and technological advances.

Congressman Cathcart, of Indiana, was moved, as several of his colleagues were, to ponder upon the rapid pace with which the nation was advancing. Addressing his fellow-congressmen, in February 1846, he reminded them that

> An omniscient God has given to man control of the elements [referring to the telegraph], the effect of which is to bring the remotest parts of our vast country into practical propinquity. The iron horse, the steam car, with wings of wind, his nostrils distended with flame, salamander like, vomiting fire and smoke, trembling with power, . . . flies from one end of the continent to the other with less time than our ancestry required to visit a neighboring city. . . . Truly it may be said, that with the social influence of these two great inventions [the telegraph and the railroad], all the people of our continent may be moulded into one mind.

The greatest scientific event of this year, however, was the final establishment of the Smithsonian Institution. On August 10, 1846, President Polk signed the bill which, twenty years after James Smithson had written his will, brought into existence one of the greatest scientific organizations in history.

The Board of Regents held its first meeting in September 1846. Within a year, the Regents had organized plans for establishing the Institution. After drafting plans for the building that was to be erected, they elected Professor Joseph Henry, of Princeton, as the Smithsonian's first secretary. No better choice could have been made. For thirty-two years, until his death in 1878, Joseph Henry planned and directed the work of the Smithsonian Institution, firmly establishing it as one of the world's greatest scientific institutions.

This is also the year that marked the appearance of Howe's sewing machine. For years he (and others) had been working on a machine that would combine the eye-pointed needle with the shuttle, for forming a stitch, and the intermittent feed for carrying the material forward as each stitch was formed. The device for feeding the cloth consisted of a thin strip of metal, provided with a row of pins on the edge, upon which the material to be sewed was carried in a vertical position. These, plus a few later improvements, gave Elias Howe a machine that sewed perfectly, at the rate of 250 stitches a minute—seven times faster than by hand. Such a machine was needed to keep pace with the rapid output of cloth that was being produced in this period.

It should be noted that 1846 was also the year when the great Jean Louis Rodolphe Agassiz arrived in the United States. He came to "a land where Nature was rich, but tools and workmen few, and tradition none." With his arrival, American natural history had found its leader. The following year, the Federal government supplied him with a Coast Survey steamer, and he set out upon a series of explorations that was destined to bring honor, not only to himself, but to the nation.

1847

The year 1847 marked a turning point in scientific education. The Sheffield Scientific School, which claims to have been the first to introduce courses in geology, paleontology, and physiological chemistry, and the first to establish an agricultural experimental station, was founded in 1847.

In June 1847, Abbott Lawrence gave $50,000 to Harvard College to be used to encourage the teaching of science, mechanics, chemistry, and engineering. This marked the beginning of the Lawrence Scientific School.

This same year witnessed a revolutionary speed-up in printing. The Hoe Lightning Press set a record by turning out 2,000 small newspapers per hour, an improvement over the nearest rival, the revolutionary cylinder press, which could print only 1,000 papers per hour.

This was also the year that Cyrus McCormick moved to Chicago, built a three-story brick building, equipped it with six forges, employed thirty-three men, and turned out 500 reapers. The following year, he produced 1,500 reapers. Mechanized farm labor had definitely arrived.

1848

The year 1848 was a memorable one in American history. The war with Mexico was drawing to a close; the United States had acquired the second largest amount of territory ever added to the national domain; and the discovery of gold in California set off the greatest migration in the nation's history. The revolutions of 1848 in Europe drove thousands of liberal-minded men out of their native land, and many of them sought shelter in the "Land of Free."

That keen French observer De Tocqueville had recently noted:

> If their democratic principle [speaking of America] does not on the one hand induce men to cultivate science for its own sake, on the other it does enormously increase the number of those who do cultivate it. . . . Permanent inequality of conditions leads men to confine themselves to the arrogant and sterile research of abstract truths, whilst the social conditions and institutions of democracy prepare them to seek the immediate and useful practical results of the sciences. The tendency is natural and inevitable.

In June 1848, the distinguished educator Horace Mann uttered his famous eulogy on the scientific and technologic advance which our people had made, declaring that

> caloric energy, gravitation, expansibility, compressibility, electricity, chemical affinities and repulsions, spontaneous velocities—these are the mighty agents which the intellect of man harnesses to the car of improvement. The application of water, and wind, and steam, to the propulsion of machinery, and to the transportation of men and merchandise from place to place, has added ten thousand fold to the actual products of our industry. . . . God, instead of giving us a telescopic and microscopic eye, has given us the power to invent the telescope and the microscope. Instead of 10,000 fingers, He has given us genius to invent the power loom and the printing press. Without a cultivated intellect, man is among the weakest of all the dynamic forces of nature; with a cultural intellect, he commands them all.

Indeed, the time was ripe and the groundwork well laid for such an organization as the American Association for the Advancement of Science.

◆

AT THE MUSEUM

This is the past I crown. Within these halls
The devious currents that have borne me here
Repose. Here are the stars, and here the earth,
Rock, fruit, and beast; and of all lore the first.
Here, in their art and science, lies revealed
The outreach of ten thousand years of men
Struggling, as I, to grasp the nascent truth
Behind the wonder of the world they knew.

These are my fellows, and the urge is strong
To linger here and learn what each has done.
But such were dalliance, with so little known
And so much yet to probe; I must persist
In my own search, nor let their monuments
Beguile me from the work I have to do.

CLARENCE R. WYLIE JR.

THE DOCTRINE OF *AHIMSA* AND CATTLE BREEDING IN INDIA

BURCH H. SCHNEIDER

Professor Schneider (Ph.D., Cornell, 1931), whose special interest is animal nutrition, was a representative of the Harvard Mission in Albania, 1927–29. He was at the Allahabad Agricultural Institute for five years, and is now professor of animal husbandry at West Virginia University.

IN EXAMINING the effect of the doctrine of *ahimsa* on cattle breeding in India, one must first understand its significance. This part of Hindu philosophy prohibits the killing of any animal life. It has far-reaching consequences: it has greatly affected the political policies of Indian nationalists and it is the basis of the nonviolence movement, or *Satyagraha*. When Gandhi was taken to prison in 1942, his last instructions to his followers were: "Do everything possible under *ahimsa*. . ." His assassination may be interpreted as a revolt against this ancient doctrine, which he practiced and advocated. A group of Hindus wanted to fight the Mohammedans, but the Mahatma, who proclaimed *ahimsa*, stood in the way.

The doctrine has its roots in the belief in reincarnation. The orthodox Hindu abhors the idea of killing an animal because he believes it might have the soul of a man from a previous incarnation. The taboo applies most strongly to cattle and to certain other species of animals. The modern Hindu points out that the early lawgivers provided protection for cattle and gave the rule religious significance in order to preserve this essential draft animal. The farmers of India are dependent on their oxen for plowing and all motive power in cultivation. It is reasoned that the ancient wise men thought that in times of drought and famine a flesh-eating people might kill all their cattle for food; then when the rains came again they would have no oxen to cultivate their fields.

Whatever the origin of the doctrine of *ahimsa*, it must be taken into consideration in any plan for general livestock improvement in India. There are few countries in which the lives of the people are so closely associated with their cattle. Approximately one third of the cattle of the world are in India, and, in many sections of that densely populated country, the number of cattle closely approximates the human population. These humped Brahman, or Zebu, cattle have been imported in large numbers into the Western Hemisphere, and they are rapidly increasing in almost all tropical and semitropical climates because of their ability to survive, and even thrive, under difficult environmental conditions to which other breeds succumb.

Western observers have been outspoken in their condemnation of the doctrine of *ahimsa* and its effect on cattle breeding in India. One world authority on livestock breeding told me that he had refused a position in India because he understood that the people would not kill their inferior animals. Thus, he concluded, he could do little professionally, and it would be useless for him to go there. Even among leading Hindus, one can recognize a note of hopelessness with regard to cattle improvement. Although they may be committed to the doctrine of not killing cattle, nevertheless with their background of education in Western practices in animal husbandry, they cannot conceal their conviction that the task of breeding improved cattle in a Hindu society is impossible. I have come across this attitude on many occasions in conversation with employees on government cattle farms, large landholders, and rulers of native states—all interested in cattle improvement, but unconvinced that it is actually possible under India's socioreligious system.

I have been tempted to concur in the outspoken opinion of some other Westerners that the best way to breed improved cattle in India might well be to slaughter all of the poorest animals. Nevertheless, before one has lived many years in close harmony with Hindu cattle breeders, he realizes that it will not be possible in our generation to begin cattle improvement in India by wholesale slaughter, or even by killing a limited number of the worst specimens. Contact with the Occident may bring greater "enlightenment," but one need not hope to see a willingness to kill excess inferior cattle in the near future.

The handicaps of the *ahimsa* philosophy are very evident. The possible advantages have never been pointed out. Breeders everywhere must consider the adaptability of their livestock to the climate, the feeding stuffs available, and the suitability of the animals for the purposes for which they are intended. Also, they must consider the habits, social

INDIAN ZEBU, OR HUMPED CATTLE

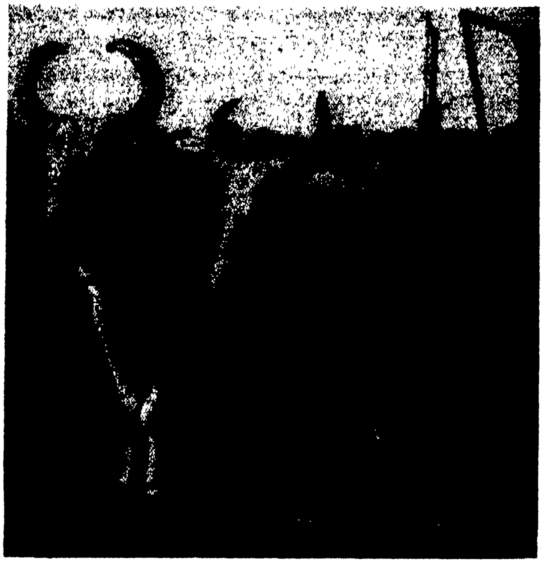
customs, and economic conditions of the people for and by whom the animals are bred.

In India, cattle are bred for draft and for milk production. There has been no need to improve their beef qualities, since a large proportion of the population eat no beef. Hindus, Sikhs, Jains, and Parsis (Zoroastrians), which together compose two thirds of the population of India, all respect the doctrine of *ahimsa.*

Some of the finest draft cattle in the world are to be found in India. It is evident that selection for draft type has been highly successful. To raise the general average of draft cattle, it has appeared to be necessary only to breed the small, less desirable cattle of some sections to the more drafty, improved types found in other parts of the country. The breeding of better cattle in any case must, of course, be accompanied by improved feeding and management practices in those areas that have previously had inferior cattle. Provision for good feeding is one of the imperatives of the livestock industry in India. The improved cattle should be well fed even though the others are denied enough to eat.

Indian cattle show less improvement in dairy qualities than in draft type. The average milk production of Indian cows has been estimated at 600 pounds per year. Milk is of the greatest importance

INDIAN OXCART



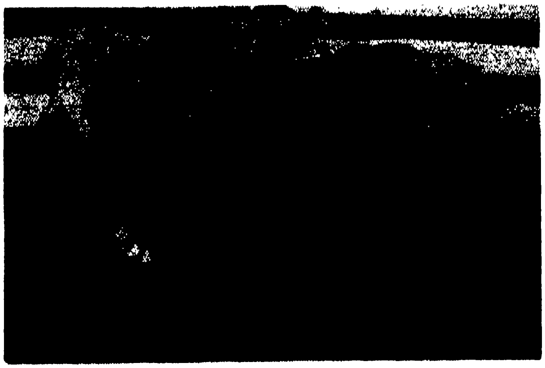
ZEBU, OR HUMPED OXEN

THESE CATTLE ARE THE ONLY PRACTICAL SOURCE OF FARM POWER FOR INDIA'S SMALL FIELDS. HEAVY DRAFT HORSES ARE UNABLE TO WORK IN THE EXTREME TROPICAL HEAT.

because of the doctrine of *ahimsa*. Since meat eating is forbidden, milk becomes the only common food of animal origin that the religion of more than 200 million people permits them to eat. Furthermore, Indian people are very fond of dairy products of all kinds, of which they have several unknown to Americans. It is generally recognized that the proteins of animal origin average higher in biological value, and that such foods contain more minerals, particularly calcium and phosphorus, than most foods of plant origin. There are other dietary advantages of a mixed diet from both plant and animal sources that emphasize further the great importance of breeding for higher milk production wherever milk is the only animal product consumed by great masses of the population.

AN ONGOLE, OR "NELLORE," HEIFER

The greatest objection to the doctrine of *ahimsa* is found in the excessive numbers of cattle permitted to live and compete with other animals and with man for the fruits of the land. It is surprising to note the congestion of animal and bird life of all species in some sections of India, which are among the most densely populated parts of the earth. Throughout all seasons of the year, neither man nor beast is able to obtain an adequate food supply. For those engaged in cattle breeding as a business, it is obvious that the excessive numbers of cattle tend to decrease their value, and the attendant shortage of cattle feed increases its price. These two factors make commercial dairying a difficult enterprise unless high-quality milk is sold at high prices in urban areas to Europeans or to well-to-do Indians. The millions of inferior and unproductive cattle that must be fed constitute a huge economic drain on the country.



GIR BULL, ONE OF INDIA'S FINE BREEDS

Under present economic conditions in India few cattle owners have the means to purchase fence or to employ herdsmen to keep their cattle entirely separate from other, inferior cattle. In other countries, also, where public grazing grounds were formerly the rule, general improvement of cattle was not begun until laws were passed requiring individual owners to fence in their livestock. (Many countries having no taboos restricting the slaughter of cattle have fully as poor animals as India.) It is the custom, but not religious law, for all cattle in an Indian village to graze together. Poor cows, good cows, calves of all ages, scrub bulls, and government-approved bulls are all in the same herd. Fencing cattle is not as feasible in India as in some other countries, because each owner often has only two or three animals.

MATAPALUT, WITH "SIND QUEEN"

FOREMAN, ALLAHABAD AGRICULTURAL INSTITUTE. A MEMBER OF THE PANCHAYAT (COUNCIL OF 5 MEN) OF THE AHIR ("CATTLEMEN") CASTE IN THE TRANS-JUMMA REGION.

Fencing materials are expensive, and the relative cost per animal would be exorbitant for the poor peasant.

Any attempt to improve livestock must have the full cooperation of the entire village. Education and confidence are necessary. When public sentiment swings in favor of any reform, pressure may be brought to bear on backward individuals in India as effectively as in any other country. It is not necessarily inherent in the Hindu system that inferior cattle be permitted to propagate themselves promiscuously. Social custom, not religion, has permitted promiscuous breeding. It is possible to remedy this defect without violating the Hindu conscience.

KANKREJ, OR "GUZERAT," BULL

Cattle improvement may be undertaken from either the lower or the upper end of the scale. In Western countries we have raised the average of our cattle by culling the poorer individuals. We have culled our cattle almost invariably by killing them. This is, of course, very effective in making changes quickly in the average merit of any group of individuals. Consequently, our thinking regarding cattle breeding is such that most of us have the idea that this is the only way to improve cattle. Actually, this kind of mass selection is not very effective in producing better cattle, i.e., in improving animals at the upper end of the scale.

The excellent work in culling at various government farms in India illustrates the effect of culling the poor cows. It has been the custom on such farms to measure progress in breeding for milk production by the average daily milk yield per cow. Improvement of this kind, however, is not a function of breeding, other than that replacements in a herd are obtained from among offspring of cows remaining in the herd after culling. It is possible to increase the average by discarding the poorest producers year by year, yet breed no better cattle. There is evidence that this occurs. For instance, there are records of Indian cattle producing 7,000 pounds of milk per lactation thirty years ago, yet as late as 1932 no better yields had been obtained by breeding. Since that year, changes in feeding and management have resulted in increases in production not due to breeding. Culling out the poorest cows raised the yearly averages of a herd, but did not breed better cattle.

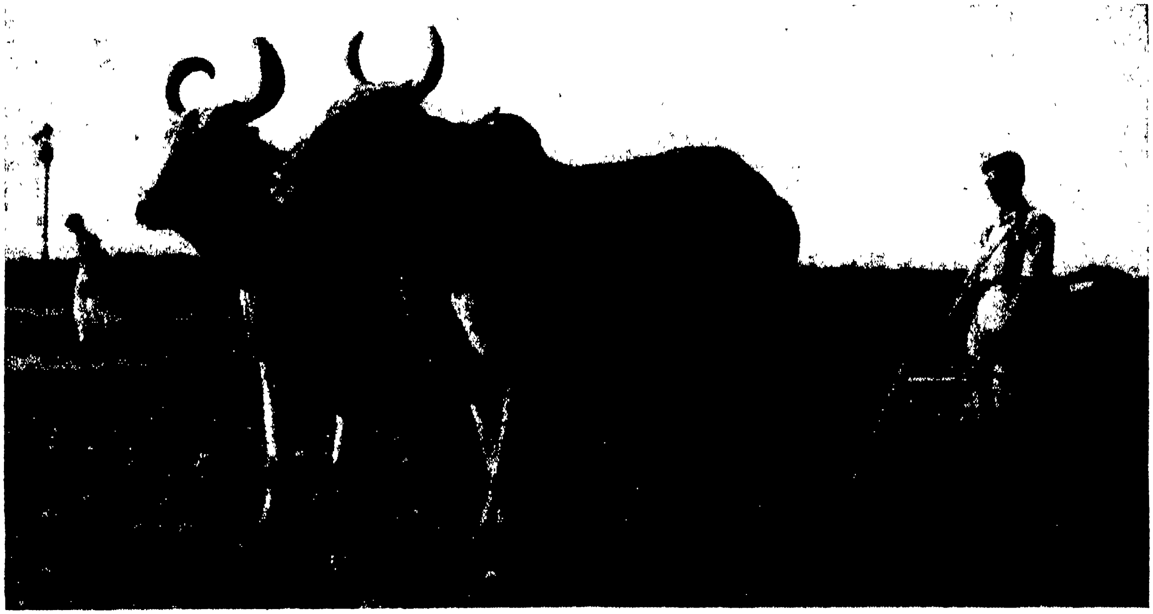
PLOWING CONTEST AT FARMERS' FAIR

Our Hindu brother, who is pessimistic regarding the improvement of cattle in his country because of a fundamental religious philosophy that he is reluctant to give up, may well ask: "What is accomplished by 'murdering' cows?" For each inferior cow killed, we prevent the birth of an average of less than one inferior calf annually. This practice, therefore, which is opposed to *ahimsa,* if carried out wholesale, reduces the numbers of existing inferior animals, but it is not as effective in preventing the propagation of inferior cattle as another practice: castration. Castration can be far more effective in reducing the numbers of poor calves. A scrub bull may sire twenty or more scrub calves per year. Castration is not as offensive to the religious sensibilities of the Hindus as the killing of cattle. It is well known and is practiced in all parts of India. Although there might be difficulty in carrying out wholesale castrations in certain parts of the country, I am of the opinion that there is no part of India where general castration of scrub bulls cannot be introduced, especially if the cooperation of intelligent leading Hindus is obtained. The use of the "bloodless" Burdizzo castrater is particularly helpful in dealing with peasant prejudice. Castration *is* practiced widely. Where it has been objected to on religious grounds, this has been only an excuse advanced by Hindus who did not understand the program to improve their cattle, and who were fearful of the consequences. Without understanding, they might well fear the possibility of being left without adequate numbers of breeding bulls.

KANKREJ COW FROM NEAR AHMEDABAD

Hinduism is not opposed to selection of cattle with the viewpoint of increasing the number of progeny from superior bulls. *Ahimsa* is opposed only to killing; it is not opposed to castration. It is not opposed to selection if it can be practiced without "murdering" cows. Our Western practice of slaughtering cattle culled from breeding herds has made culling, killing, and herd improvement appear inseparable.

It is possible to practice selection by means of castration, and permit superior bulls to sire more



OX TEAM WITH STUDENT DRIVER

THE AMERICAN STEEL PLOW IS GRADUALLY SUPPLANTING THE WOODEN PLOW USED BY 90 PERCENT OF INDIA'S FARMERS.

calves, without coming in conflict with Hindu religious thought. In fact, *ahimsa* permits the more accurate selection of superior bulls than is possible in Western countries. The progeny test has proved itself particularly valuable in recent years with dairy cattle. The increased use of proved bulls is the goal of dairymen throughout the United States. However, it is recognized that many bulls may be rated more favorably than they deserve because their inferior offspring have been slaughtered. This difficulty would never occur in a Hindu community. All heifers strong enough to mature and produce milk are permitted to do so. None would be eliminated without having had an opportunity to obtain a measure of her milk-producing ability. Furthermore, a measure of longevity of production and longevity of life of certain families might be obtained from the records, as no cows are killed even when they are past the age of economical production.

The practice of having public bulls at stud in India would supply the machinery for progeny testing in a way not found in many other parts of the world. It has long been a custom for a Hindu gentleman to honor a deceased relative by giving a bull to the community for public service. It is said that formerly superior animals were given, but it appears that the custom has now degenerated so that only cheap, inferior bulls are purchased and dedicated to the honor of the dead. This function of supplying bulls for public service has largely been taken over by the local governments.

It would be possible to maintain liaison between the peasant breeders and the central breeding farms that supply the bulls. The bulls should be used only in one-bull herds to prevent uncertainty regarding paternity, and they should be moved from time to time to prevent their breeding their own daughters. Bulls may be passed on to other villages or, if they have sired superior offspring in the villages, returned to the central breeding farm as proved sires. All scrub bulls running in the same herds should be castrated. It is also possible that such a well-organized scheme might achieve sufficient prestige in certain areas that service could be refused to inferior cows.

Certainly, tremendous effort and great leadership would be required to launch a comprehensive system of cattle improvement based on progeny testing in the villages surrounding even one cattle-breeding farm from which bulls are supplied. I know of only one such attempt having been made. The plan is entirely feasible, however. Every Indian village has at least one literate man who, with proper supervision, meticulously keeps certain village records. It is far from impossible to conceive of such a man maintaining accurate breeding, production, and other records necessary for progeny testing. Thus, bulls could be regularly proved in all the villages in the area. It is important that such a local area be saturated with good breeding bulls. Such a plan is better than spreading the available bulls out thinly by scattering them over an entire province. It is also desirable that such an area be near the farm on which the bulls are bred. It is possible that progeny testing may not be effective in the beginning if the native cows are very heterogeneous. This is a difficulty that will be progressively overcome, however, generation by generation, as bulls of similar breeding are provided.

Cattle could be improved by breeding the best better. Improving the cattle at the central breeding farm would in turn enable better bulls to be supplied to the villages. Culling and killing of poor individuals would not be a requirement of this plan, for all offspring of every bull would be permitted to live as complete samples of all progeny capable of survival. Proper recording and application of genetic principles can go far toward making the best of a bad situation. Selection would take place on the basis of these samples of germ plasm more complete than anywhere else in the world. Improvement would take place at the top, not by eliminating the lower-producing individuals. Such a system could operate as economically and effectively in India as in any other country, with many points in its favor.

Although the doctrine of *ahimsa* is a handicap to phenotypic selection as practiced in Western countries, where inferior cattle are culled and killed, it appears to be of value to genotypic selection and the breeding of superior animals, provided good records are kept and genetic principles followed. Occidentals consider *ahimsa* a great disadvantage from the Western point of view, but it need not be an unsurmountable obstacle to cattle improvement in India. Newer practices of progeny testing may be carried out more completely in a Hindu society than where there are no inhibitions regarding slaughter of cattle. Genotypic selection for milk production may then be found to be more effective by not practicing close phenotypic culling of both sexes. This should be a challenge for agricultural officers in the newly created Dominion of India.

TECHNOLOGICAL INNOVATIONS AND THE CHANGING SOCIOECONOMIC STRUCTURE

A. J. JAFFE*

Dr. Jaffe (Ph.D., Chicago, 1941) is Statistician at the U. S. Bureau of the Census, Washington. He is the author of numerous papers in the fields of demography, the labor force, and related topics. This is the second article in the sociological series.

SOCIETY is in a constant state of change; we are always doing things somewhat differently from our predecessors, living a little differently and thinking a little differently. Although such change has probably been occurring in all societies at all times, prior to the industrial revolution change took place gradually. By contrast, it is particularly noticeable in our modern industrialized society—or so we like to believe—where changes of all sorts are swiftly taking place. Social change has been recognized by every adult who has ever said, "Things were different when I was a youngster," and probably the vast majority of all adults have made some such comment.

It is no accident of nature that social change occurs. Changes in biological life due to mutations perhaps can be attributed to chance factors. But not so with changes in society. Of the factors causing change in our society, one of the most important is technology—important new inventions as well as minor technical innovations, some so minor as to hardly merit patenting.

> Invention is a great disturber and it is fair to say that the greatest general cause of change in our modern civilization is invention; although it is recognized that social forces in turn encourage or discourage inventions. Certainly developments in technology cause a vast number of changes in a great variety of fields. A banker once defined invention as that which makes his securities insecure. Hence a study of the trends of inventions furnishes a broad perspective of many great movements of change and basic general information for any planning body, however general or specific their plans may be (*Technological Trends and National Policy,* edited by W. F. Ogburn).

In this short article it is proposed to trace the effects of technological innovations in only a very limited area—namely, that in the plane of living and the class structure of our society. It will be shown how new inventions and new techniques affect and cause changes in the basic occupational and industrial structure of the nation, and how the plane of living has gradually been raised to the highest average level ever experienced in this world; the possible repercussions of these changes upon the class structure will then be examined. Emphasis is thus placed on the sociological aspects of the picture rather than the economic or productivity aspects per se. (For an excellent discussion of the economic aspects of technological change in the United States, see F. C. Mills: "Technological Gains and Their Uses.")

The data presented here refer to the United States, in which the application of modern technology has been carried to the greatest extreme. The conclusions, however, apply to other industrialized countries, as far as can be determined, and they also give a suggestion as to the future of other less industrialized nations.

TECHNOLOGICAL progress is sometimes thought of in terms of outstanding and newsworthy inventions such as the automobile, airplane, motion picture, and radar. However, the term means much more than simply such few outstanding developments; it encompasses the great multitude of small inventions that improve the efficiency of some minute aspect of a particular job without necessarily introducing anything new. These inventions are seldom newsworthy, and not always patentable. In general, they may be said to consist of two types—procedural and material inventions. Both contribute to increased productivity, defining this term very broadly in the words of W. Duane Evans, "We are interested in determining whether a given job takes more or less labor over a period of time."

In the first category is included much of the know-how of industry and business—the administrative organization, the laying out of efficient and orderly work flows, the establishment of improved labor-management rapport—all of which increase efficiency and productivity. Thus, for example, the introduction of the moving assembly line was originally a procedural invention, and in itself increased the output per worker. Such innovations are not always thought of as inventions, despite the fact that they do ultimately contribute to increased productivity.

The second category—material inventions—includes items and processes that are generally patentable, but not necessarily newsworthy. A new lubricating oil, for example, which will permit a machine to operate at increased speed, may cut the labor required at some one particular point in a complex manufacturing process. Or the raising of a new type of seed may permit increased agricultural production per acre without the necessity of increasing the expenditure of man power. Any single one of these inventions in itself may have an effect on the ultimate end product, although by itself it is hardly—if at all—measurable by any of the known measuring instruments. The sum of many such individual items, however, becomes apparent in even the crudest of productivity measures.

---

* This article is not written in the official capacity of the author and is not to be interpreted as representing any official opinions of the Bureau of the Census.

The volume and regularity of production can also have an effect on measures of productivity; a large volume of products being produced without interruptions will often require less labor per unit than in the cases of smaller volume or interrupted production. As W. Duane Evans wrote:

> The cement industry is an example. Output per man-hour continued to increase through the year 1942, as production expanded. After the bulk of war construction was completed, cement production dropped sharply, and with it productivity. By 1944, output was only half as great as it had been in 1942, and output per man-hour was 20 percent lower than in 1942.

As can be seen in Figure 1, there have been considerable increases in productivity during recent years. Increases undoubtedly occurred in the years prior to those shown here, but comparable data are unavailable.

Output per man-hour approximately doubled between 1919 and 1939 in all manufacturing industries combined, in mining, in the electric light and power industry, and in railroad transportation. During this same period productivity in agriculture as measured by output per worker increased only about 25 percent, but since it is probable that working hours in agriculture declined, as happened in manufacturing industries, output per hour probably increased somewhat more than 25 percent. In the telephone and telegraph industry (for which data are available only for the period 1935–46) there has also been some increase in productivity.

During the war years productivity increased in some industries and declined in others. Owing to the radical changes in the nature of the manufactured goods produced during wartime, comparisons of the prewar and war periods for manufacturing are not permissible.

As for the future, there can be no doubt but that productivity, regardless of how it is measured, will continue to rise.

Perhaps the single most important hiatus in the field of productivity measures is for those industries which do not produce tangible goods. Manufacturing, mining, electric light and power, and

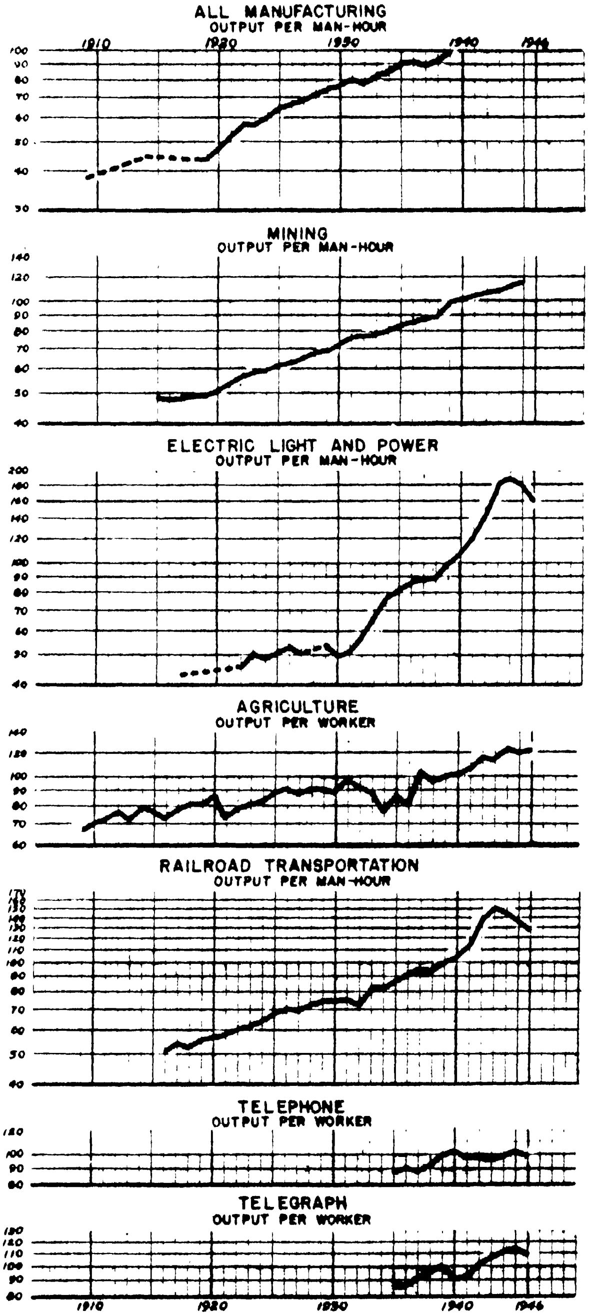


FIG. 1. PRODUCTIVITY INDEXES*

Sources: C. S. Gody and A. D. Searle, "Productivity Changes Since 1939," *Monthly Labor Rev.*, Dec. 1946; *Productivity in Agriculture: 1909–1946*, U. S. Bureau of Labor Statistics, Dec. 1947; *Productivity and Unit Labor Cost in the Telephone and Telegraph Industries: 1935–1946*, U. S. Bureau of Labor Statistics, Nov. 1947.

* (For selected industries, 1909–46. 1939 = 100.)

agriculture all result in tangible goods; railroads and the telephone and telegraph industry are more in the nature of service industries, although they do supply a standardized service which can be measured—ton miles of freight carried, message units provided, etc. For the great bulk of the service industries (the supplying of a service—such as trade, professional or domestic service, etc.—rather than a tangible good), unfortunately there are no productivity indexes, owing in large measure to the lack of a standardized measurable product. Yet, as will be shown, some half of the present labor force is engaged in producing services rather than goods. Thus, in order to appreciate fully the effects of technology and inventions on our society, it will be necessary to devise means of measuring trends in productivity in those industries now engaging half or more of all workers.

The need for improving the present measures, especially those which represent statistical aggregates of several individual products and industries (as that for all manufacturing activities combined) is the second major problem facing students now engaged in this field. The U. S. Bureau of Labor Statistics, which has done most of this work to date, is constantly engaged in attempting to improve its measures.

The thesis of this article is that one of the important elements in precipitating social change is technological innovation. Technological innovations, taken together, have produced the vast increases in productivity, or output per man-hour (or per worker as the case may be). Such radical changes in the efficiency of production and the utilization of man power, in turn, have affected the social structure in various ways. Let us examine several of these derivative effects.

THE CHANGING INDUSTRIAL AND OCCUPATIONAL COMPOSITION

Industrial composition. One of the immediate effects of changes in productivity is upon the industrial composition of the labor force. In Table 1 the industries of the United States have been classified into two major groups: those which produce tangible goods (agriculture, forestry, fishing, extraction of minerals, manufacturing, mechanical industries, and construction), and those which largely produce services (transportation, communication, public utilities, professional service, clerical services, trade and finance, domestic and protective service, government service, etc.). At each date the proportion of the working population engaged in these two broad classes is shown. (Detailed statistics are given in Table 2.)

TABLE 1

Type of Industry	Percentage of Workers					
	1947	1940	1930	1910	1800	1870
Production of goods	44	49	53	62	68	75
Production of services	56	51	47	38	32	25

In 1870, with the relatively inefficient means of production then available, three quarters of all workers in the United States were engaged in industries producing goods, and of these the bulk were in agriculture. (In 1840 almost 80 percent were engaged in agriculture alone.) By 1947, with the remarkable increases in productivity in these industries, less than half—some 44 percent—were engaged in producing goods.

The substitution of more efficient machines and processes for less efficient ones and for man power resulted in the freeing of millions of workers from the necessity of producing the physical goods requested by our society. These workers recruited from farms and factories, as well as the farmers' sons and daughters and other new workers entering the labor market for the first time, plus many of the millions of immigrants, found jobs in the ever-expanding service industries.

Agriculture in particular was able to free workers for nonagricultural pursuits. In 1870 more than half of all workers were engaged in agriculture (including forestry and fishing); in 1947 less than 15 percent were so engaged. In actual numbers there were about as many farmers in 1947 as in 1900, despite the fact that the total population almost doubled in size between these years.

Occupational composition. The occupational composition of the working population permits classifying the workers into two major groups on the basis of the kind of work they actually perform rather than the type of product resulting from their labors. Thus we can delineate two broad groups: those who work mainly with their brains, and those who work mainly with their hands.

In 1947 white-collar workers comprised an estimated 36 percent of all workers (fourteen years of age and over); in 1910 they comprised some 21 percent. The population engaged in manual work (including agriculture) thus decreased significantly from some 79 percent in 1910 to 64 percent in 1947. The increases in productivity during these decades largely affected those occupations in which manual labor predominated and thus permitted increasing numbers to enter commercial, professional, and clerical occupations (Table 3).

TABLE 2

ESTIMATED INDUSTRIAL DISTRIBUTION OF THE WORKING POPULATION FOURTEEN YEARS OF AGE AND OVER, UNITED STATES, 1870–1947*

Industry	1947	1940	1930	1920	1910	1900	1890	1880	1870
Number (in thousands)	60,650	52,022	48,593	41,798	36,634	28,273	22,634	16,887	12,581
Percentage, total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Agriculture, forestry, fishing	13.0	17.6	21.6	26.5	30.3	36.5	41.7	48.4	52.2
Extraction of minerals ...	1.5	2.1	2.0	2.6	2.6	2.5	2.0	1.8	1.5
Manufacturing, mechanical ind., construction	29.6	29.3	29.0	30.8	29.1	25.5	24.4	22.7	21.0
Transportation, communication	6.7	6.6	7.9	7.4	7.3	6.9	6.2	4.9	4.3
Professional service	6.4	6.8	6.7	5.2	4.7	4.2	3.9	3.3	2.7
All other service and white-collar	42.8†	37.6	32.8	27.5	26.0	24.4	21.8	18.9	18.3

*Alba M. Edwards: *Comparative Occupation Statistics for the United States, 1870 to 1940,* U. S. Bureau of the Census, Washington, 1943. 1947 estimates made on the basis of current (post-1940 U. S. Census) data published by the U. S. Census Bureau and the U. S. Bureau of Labor Statistics.

† Including armed forces and those of unknown industry.

Increases in productivity have affected the service industries and white-collar occupations, as even casual observation will reveal, but probably not to the extent to which the production of goods has been affected. Thus, for example, the introduction of the cafeteria and the self-service grocery store must have increased the productivity of workers in such establishments. The modern typewriter and calculating machine have increased the productivity of some clerical operations. The physician with his automobile very probably is more productive than one equipped with a horse and buggy in that he wastes less time simply riding from one patient to the next. The major problem, then, is to determine just how much increase there actually has been in these various fields in order to hazard some evaluations of the future.

TABLE 3

SOCIOECONOMIC CLASSIFICATION OF THE GAINFUL WORKERS, 1910–30, AND OF THE LABOR FORCE, 1940, UNITED STATES*

Socioeconomic Class	1947	1940	1930	1920	1910
Nonagricultural:					
White-collar					
Professional	6.8	6.5	6.1	5.0	4.4
Proprietors, managers and officials	10.1	7.6	7.5	6.8	6.5
Clerks and kindred workers	18.7	17.2	16.3	13.8	10.2
Subtotal	35.6	31.3	29.9	25.6	21.1
Manual					
Skilled workers and foremen	13.9	11.7	12.9	13.5	11.7
Semiskilled workers	20.6	21.0	16.4	16.1	14.7
Unskilled workers	15.6	18.7	19.8	20.0	21.5
Subtotal	50.1	51.4	49.1	49.6	47.9
Agricultural:					
Farmers, owners and tenants	8.2	10.2	12.4	15.4	16.5
Farm laborers	6.1	7.1	8.6	9.4	14.5
Subtotal	14.3	17.3	21.0	24.8	31.0
Total	100.0	100.0	100.0	100.0	100.0

* Same source as Table 2.

We can predict confidently that tremendous increases in productivity will take place in the future in manufacturing, mining, agriculture, and those other industries for which productivity indexes are already available. And, for some time in the future, the proportion of the workers engaged in producing goods, or in manual occupations, will continue to decline. Thus, increasing numbers (as well as proportion) of workers will be found in the service industries and white-collar occupations.

We may now ask what the probable effects may be in the future, if the service industries should experience vast increases in productivity comparable to those experienced in the manufacturing industries. That there probably will be considerable repercussions upon the occupational and industrial composition of the nation's labor force goes without saying. But what form will these repercussions take? And, furthermore, what might be the eventual effect on other sectors of the social structure?

INCREASE IN THE PLANE OF LIVING

Technological innovations are introduced generally for one of two reasons. On the one hand, the new invention may permit an improved product, either at a decreased cost or at the same cost (or occasionally even at a higher cost). A second reason for introducing a new invention or process is to reduce costs. These are the only two classes of reasons that will induce a concern to go to the trouble of changing its machines or work procedures in order to increase the productivity of its operations. Of course, in certain circumstances a new machine or device may be introduced for health or safety reasons; in other circumstances, a manufacturer may purchase an improved machine simply because such was the only type available when he found it necessary to replace old equipment. But as a rule the prospect of increased profits is the primary motive for introducing new techniques.

The net result of these technological innovations, then, has been a gain in the plane of living of the entire population, as is well known. We are all familiar with the general appearances of such increases in the plane of living, but specific statistics that would measure such changes precisely are largely lacking. It can be pointed out, for example, that the index of manufacturing production (calculated by Solomon Fabricant)—that is, the estimated volume of physical output—rose from 100 in 1899 to 373 in 1939, or almost a quadruple increase. During the same period, the total United States population increased by only some 75 percent—from about 75 millions to 131 millions. Such an increase in the physical output of industry must have been accompanied by an increase in the amount of goods available per person.

Another index of the increase in the plane of living is the estimated per capita value of all goods available to consumers at several periods (based on data by Simon Kuznets). Owing to changes in the purchasing power of a dollar, such value figures have been estimated in terms of 1929 prices. Over the past seven decades or so the estimated per capita value of such consumer goods has about tripled (Table 4).

TABLE 4

Period	Estimated per Capita Value
1929–38	$562
1909–18	444
1889–98	297
1869–78	183

The mechanism by which the accumulated increases in productivity ultimately result in an increase in the plane of living (that is, the volume of physical goods and services available per person) has been described by C. S. Gody and A. D. Searle as follows:

> Output per man-hour, together with average hourly earnings, determines unit labor cost—the wage payments made per unit of output. If average hourly earnings remain unchanged, unit labor cost is reduced as productivity is increased, and prices can be reduced without any decline in profits. Where there is competition, it is likely that savings in labor cost made possible by productivity increases will be reflected in lower prices, which benefit all consumers. If effective competition does not exist, however, and if average hourly earnings are not increased, the result of gains in productivity may be an increase in profits.
>
> Workers benefit through price declines, together with all other consumers. A more direct way in which workers share the benefits of productivity gains is by means of wage increases. As output per man-hour increases, average hourly earnings may also increase without any increase in unit labor cost and without any upward pressure on prices or any reduction in profits. In 1939, unit labor cost in manufacturing industries was 44 per cent lower than in 1919 and wholesale prices of manufactured goods, 38 per cent lower. Average hourly earnings, on the other hand, were 28 per cent higher. The basis for the wage increases and the declines in unit labor cost and prices was the large rise in output per man-hour.

Theoretically, there is nothing mandatory about this process whereby increased productivity must result in an increase in the plane of living. Conceivably, it is possible for increases in productivity to result in increased leisure. Indeed, in this country, simultaneously with the increase in the plane of living, there has also been an increase in the amount of leisure. In 1909 the estimated average number of hours worked per week in manufacturing industries was around 51; this figure fell rather consistently to a low point of 35 hours in 1938. During the war years, of course, the length of the work week increased; but with the resumption of peace the length of the work week has again declined and in 1947 was around 40 hours. At the time of the Civil War the average length of the work week in all nonagricultural industries seems to have been between 60 and 70 hours.

Another indication of the increase in leisure time is afforded by statistics showing the proportions of young and old persons who were workers (gainfully occupied or in the labor force). At the older ages—sixty-five years and over—some three quarters of all the men were gainfully occupied in 1890. In 1947 a little less than half the men in this age group were in the labor force.

Among boys ten to fifteen years old, about one quarter were gainfully occupied in 1890. By 1940 the incidence of boy employment had decreased to such an extent that the U. S. Census Bureau excluded the age group ten to thirteen years from its 1940 labor force statistics. It would appear that perhaps not over 5 percent of all boys ten to fifteen years of age were employed or seeking work in 1940. The later age of entry into the labor force is, of course, accompanied by a lengthening of schooling and an increase in the educational level.

Thus, the average age for entering the labor force has been increasing over the past several decades, and the average age for leaving has been decreasing. The net result is that the average male who survives beyond age sixty-five today will have spent a smaller number of years in the labor force than he would have several decades ago. In addition, while employed in the labor force he works fewer hours per week.

It would appear, then, that, in theory at least, society can take its choice as to the ultimate goals to be achieved by increased productivity. On the one hand, the working population can continue to work just as many hours per week and just as many weeks out of the year and just as many years out of a lifetime as it always has and enjoy a continuously increasing plane of living. On the other hand, a society can elect to maintain its plane of living at any given level and enjoy its increased productivity in terms of increased leisure. Of course, any combination of these two alternatives is also possible. Whether the nation is now in the position of having to make a choice can be debated. The fact does remain, however, that the various labor groups in the United States have already exhibited an interest in increased leisure.

According to E. M. Bunn, about three out of four manufacturing establishments had formal paid vacation plans in 1945–46 for their plant workers who had had at least a year's service. Almost nine out of ten establishments provided paid vacations for office workers with a minimum of a year's service. In contrast, paid vacations in 1937 were provided for plant workers by only one in four manufacturing establishments, and for office workers by eight out of ten establishments.

It must be remembered that there is some positive relationship between productivity and volume of production (varying from industry to industry); accordingly, it is possible that certain measures designed to increase productivity are not worth introducing unless there is an increase in volume of production. Theoretically, then, in a case of this nature, perhaps the benefits of increased productivity must be taken in the form of an increase in the plane of living rather than in an increase in leisure. No adequate study has been made of this area.

TECHNOLOGY AND THE CHANGING CLASS COMPOSITION

An income distribution of the total population at any given moment of time can locate an individual at some particular point—such as, for example, in the upper 10 percent, or the lower 25 percent. As we know, however, "class" involves not only the person's position in the economic scale but also his attitudes toward his position therein and the attitudes of others toward him. Therefore, unless information is available to permit determining the attitudes and to state precisely at what points a continuum of incomes can be cut meaningfully, it is impossible to state statistically what the class composition of a nation is.

No data are available that would permit exact description of the class composition of the United States either at present or at any time in the past. Three general types of statistical data are often used as indexes of class composition. These are: occupational data, income and plane-of-living data, and educational data. There is some positive correlation among all these indexes. Each has considerable limitations upon its use and interpretations; nevertheless, the use of any one or a combination of these items does permit some analysis and generalization with regard to certain aspects of the class composition.

Effects on the class composition. We have seen how technological innovations and increasing productivity have radically altered the industrial and occupational distribution of the nation, how they have increased the plane of living and the average income level, and how they have led to considerable increases in educational levels. We have also seen that these factors are the ones often used for differentiating the population into classes. We can infer, then, that the class composition has probably also changed gradually.

However, most of these available statistics have grave defects especially apparent when attempting to study historical trends; accordingly, it is next to impossible to state exactly how the class composition of the nation may have changed. It was seen, for example, that the estimated per capita value of goods available to consumers (in 1929 dollars) about quadrupled in the period between 1869–78 and 1929–38. But we have no notion of whether the distribution of such values at both dates was approximately similar, or whether some segments of the population experienced no changes, whereas other segments more than quadrupled their per capita receipts.

Implications of class composition. Despite the fact that we cannot establish precisely the size of the population in each class either today or in the past, we do know that there are considerable differences among the classes with respect to many very important facets of the social structure. And, speaking very generally, we have reason to believe these differences assume varying degrees of importance as the social composition of the nation varies. Thus we know, for example, that a century ago and more, when the great majority of the population was dependent on agriculture for its livelihood, agrarian problems were among the most important facing the nation; problems of the industrial element were of importance only in localized areas.

As industry and commerce became more important, the tariff question emerged as one element of contention between this group and the agriculture segment. And, finally, as manufacturing and industrial development grew to its present size, industrial problems assumed ever-increasing importance. Whether a new set of problems centering about the growing importance of the white-collar group will become of predominant importance in the future is a question well worth considering.

Class differences. Our knowledge of the differences between the classes with respect to many social phenomena is derived from two main sources. Certain class differences have been observed on the basis of data obtained from the United States decennial censuses and certain other types of large-scale surveys that have covered the entire country or a major segment of it. Other differences have been observed by studying small local communities and selected groups of individuals. Each investigator has had to organize his study along the lines permitted by the data and interpret his results in terms of the types of populations he was able to study. As a result, very few of these studies are sufficiently comparable in design so that their results are additive, and so as to permit exact calculation of the size of the class

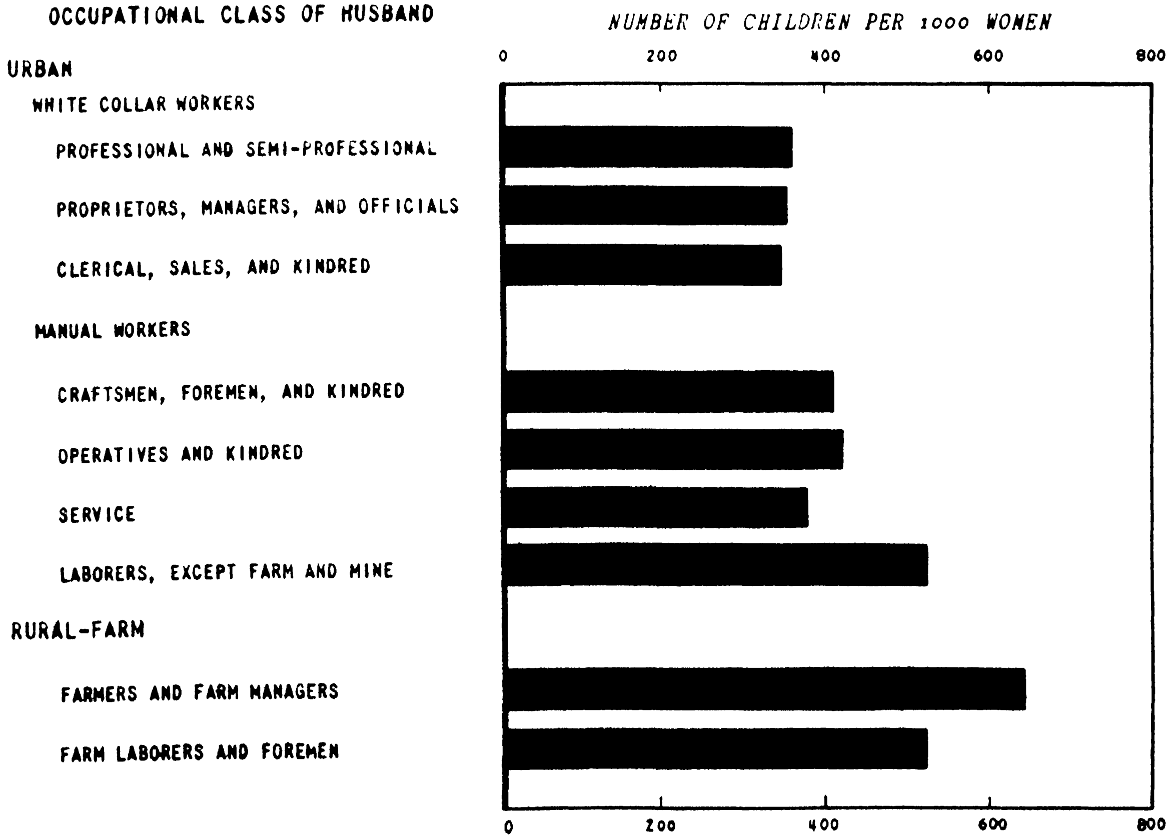


FIG. 2. BIRTH RATE BY OCCUPATIONAL CLASS OF HUSBAND, U. S., 1940

Standardized number of children under five years of age per 1,000 native white women, married once, husband present in household. Standardized on basis of age distribution of total native white women, married once, husband present. Data from *Differential Fertility, 1940 and 1910, Women by Number of Children Under 5 Years Old,* U. S. Bureau of the Census, Washington, 1945, Table 41.

differences observed. Nevertheless, certain differences have been repeatedly observed—differences that are almost always in the same direction. Because of this we can speak with confidence about the nature of some of the class differences even though their present and past sizes are undetermined and we cannot analyze the trends in these differences.

Demographic differences. These are the differences that have been observed in the way people act with regard to the population phenomena described below. The way people act indicates their underlying attitudes and beliefs; hence, although we cannot always understand these attitudes by simply observing the actions, we are sure that differences in action do indicate differences in underlying attitudes.

Differences in the birth rate have existed for a very long time. Figure 2 shows differences in fertility among women classified according to occupation of husband in the United States in 1940. Some studies have classified the population according to rental value of home, and others have used different indices. The same general pattern of inverse relationship between fertility and class level has been observed in practically all such investigations, some of which studied the population of over a century ago.

Inverse relationships have also been observed consistently between the class level (however measured) and the death rate, as the data in Table 5, from Dublin and Lotka, illustrate.

TABLE 5

Social-economic Class	Standardized Death Rate per 1,000 Males (1930)
All gainfully occupied males	8.7
Professional men	7.0
Proprietors, managers, officials	7.4
Clerks and kindred workers	7.4
Agricultural workers	6.2
Skilled workers and foremen	8.1
Semiskilled workers	9.9
Unskilled workers	13.1

The same inverse relationship is found with the infant mortality rate. Among a sample of urban families studied by R. M. Woodbury, in which the father's income was under $450 per year, the death rate of infants (deaths under one year of age per 1,000 live births) was 167; families in which the father's income was $1,250 a year or more experienced a death rate of 59.

The marriage rate is also inversely related to social class in so far as degree of formal education can be considered as an index of class. The higher the educational level, the greater is the percentage of native white women past the child-bearing age (forty-five to forty-nine) who have remained single, as data from Irene B. Taeuber and Hope T. Eldridge show (Table 6).

TABLE 6

Educational Level	Percentage Single
Grade school: 1 to 4 years	4.1
7 and 8 years	7.6
High school: 4 years	12.5
College: 4 years or more	30.1

Attitudes. It is impossible to describe fully the various attitudes of the different classes of a population. About the only data in existence which claim to be valid cross sections of the total population are those obtained from the various opinion and attitude polls. Whether these materials can be analyzed so as to afford a real knowledge of class differences and attitudes is difficult to say, since relatively little work has been done in this direction. General perusal of some of the data released by various organizations suggest the following two very general conclusions.

When persons are asked a question of immediate and direct economic interest to themselves, they tend to reply in a manner favoring themselves. Consider the example from the American Institute of Public Opinion (January 12, 1947).

Question: Many unions of factory workers are going to ask for a pay increase within the next month or two. Do you think these factory workers should get more pay?

Occupation	Percentage answering yes
Farmers	19
Professional and business	31
White-collar	37
Manual workers	50
Union members	59

Although there is room for argument as to how accurate the above figures are or exactly how they should be interpreted, there can be little if any doubt but that the underlying attitudes of the manual workers—the working class—differ significantly from those of the white-collar and farmer groups, and that this question was answered in terms of self-interest.

Another question that may illustrate the role of assumed self-interest is from the Fortune Magazine Poll (November 1946).

Question: The Political Action Committee of the C.I.O. (usually called the P.A.C.) has been active all over the country in supporting candidates for political office that labor approves of and opposing those labor disapproves of. On the whole is the P.A.C. the kind of organization you would like to see continued, or not?

Answers	Salaried executives	Professional men	Wage earners
Continued	16%	23%	31%
Not continued	72	67	37
Don't know	12	11	32

The above question also illustrates the second very general conclusion that seems permissible—that among the poorer classes the proportion of the population who have no opinion seems to be considerably greater than among the richer or more highly educated segments of the population. Whether these members of the poorer classes truly hold no opinions, or do not care to impart them to the interviewer, is a subject which requires much further study.

Consider the proportion of "no opinion" answers to the following question in which the respondents have been classified by educational level (American Institute of Public Opinion, January 3, 1947):

Question: Do you think Russia will cooperate with us in international affairs?

Answers	No opinion
College education	7%
High school	11
Grade or no school	23

In so far as a knowledge of the pertinent facts in any given issue helps the person to form an opinion or attitude, it would be expected that the "upper" classes would contain smaller proportions of "no opinion," since formal education is rather highly related to class level. And, in regard to many issues, the better-educated and more prosperous elements have a greater factual knowledge than do the others. A very illuminating study of this aspect of class differences is that of Genevieve Knupfer, "Portrait of the Underdog."

There appear to be class differences in child-rearing habits also, although definitive analyses have not as yet been made; the extent of membership in clubs and other voluntary associations appears to be positively related to class level; sex attitudes and behavior vary among the different educational levels, as shown by Kinsey and his associates; indeed, there probably are significant class differences with respect to practically every aspect of human behavior.

As we have seen, inventions and increasing productivity ultimately affect the class composition of a society, and the attitudes held by the various classes. Not only may the attitudes of the society as a whole be affected by changes in the proportional weighting of the different classes, but the attitudes themselves may be affected directly by the technological changes, or indirectly through the increase in leisure afforded by these innovations. Thus, for example, the growth of Parent-Teacher Associations and the increase in the employment of women are both in part due to the various inventions that have decreased the amount of housework necessary and so afforded women free time to devote to other than housekeeping activities. Unfortunately, our knowledge of the ultimate effects of technology upon attitudes is still very limited. Theoretical formulations exist, but only very limited research has actually been carried out.

Before closing we must note that the relationship between technology and attitudes is reciprocal. To a considerable extent the adoption of new devices and the full utilization of existing ones are inhibited by both irrational and so-called rational attitudes, as has been shown by Bernhard J. Stern. Perhaps the best current example of this is the housing problem. Our society wants more housing, but prevailing attitudes do not permit the production of standardized prefabricated houses on a mass-production basis. Society has accepted such mass production in the automobile and in the newer durable consumers' goods (refrigerators, radios, etc.), but not in housing.

IMPLICATIONS

Perhaps the single most important reason for studying technological change is to afford society a mechanism for predicting the social changes which are expected to occur, and to formulate such policies as circumstances may seem to warrant. For change in itself is disconcerting, notwithstanding the fact that the American people pride themselves on their so-called desire for the "newest" and "latest." Change is often considered "bad" for no other reason than that it is change. Thus the oldsters of every generation when speaking of "modern youth" are sure that the youth of today are not up to the standard of youth when they were youngsters, for the "youth of today" are different. Hence, any thinking that will permit a society to better adapt itself to the inevitable changes which will occur—changes stemming in large measure from technological innovations—will be better able to meet such changes.

Specific discussion and "planning" are called for with respect to the areas previously described. By "planning" is meant only achieving full cognizance of the ultimate developments in a given area, and evaluating them in the light of society's values. Thus, it is possible for the "planner" to predict that such and such will occur, and that various alternative action programs are possible, depending on the ultimate goals or values of society. If and when society adopts some ultimate goal, then that program can be implemented which will most closely fit the impending changes to such goal. This is not to say, however, that goals and values will necessarily be decided on a conscious and rational basis, but rather that every society definitely does have values, and that these values often change with the passage of time.

◆

DAYLIGHT SAVING

Now man has urged the heavens to his need,
Spurred forth the sun, harnessed the restive moon,
Coralling time like an unbridled steed,
Displacing midnight, and transposing noon.
The stars must wait to mount the Milky Way,
Slacking their silver pace, and as they climb
Man tries to trick them ; to expand his day
He meddles with the secret springs of time.

Reverse the dawn and dusk ! Recall the sun
At midnight ; toy with time ; it shall not matter.
Implacable, the hours glide, one by one,
Beyond man's paltry strength to grasp or shatter.
Time will elude him still. Not in his power
To cheat death by a single, stolen hour !

MAE WINKLER GOODMAN

THE HUMAN BRAIN IN THE LIGHT OF ITS PHYLOGENETIC DEVELOPMENT

FRANZ WEIDENREICH

Dr. Weidenreich, a native of The Palatinate, Germany, has taught anatomy at the Universities of Strassburg, Heidelberg, Frankfurt am Main, Peiping, and Chicago. Since 1941 he has been associated with the American Museum of Natural History.

THE discovery of the remains of Peking man in the cave of Choukoutien, and evidences of a relatively advanced culture at the same site, confronted the paleoanthropologist with a new, unexpected, and vital problem. The find of ash layers and burnt stones and bones revealed that the man who lived there had knowledge of fire; and the find of stone implements, some of them skillfully chipped, proved that this man was already an able artisan.

On the other hand, the anatomical record of the skulls shows that the cave dwellers represented a very primitive type, morphologically inferior to any fossil human type unearthed up to that time. The cranial capacity of the first skull to be found is not much over 900 cc. Davidson Black, who described the first finds, had no scruples about identifying the human individuals whose bones were dug out together with the cultural objects as the bearers of the Choukoutien culture. However, Marcellin Boule, the French paleontologist, thought otherwise. He argued that a human individual whose brain was not larger than a little over 900 cc could not be credited with the degree of intelligence that would be necessary to produce such a highly developed culture. From this premise, Boule deduced that a man with the physical appearance of modern man must have lived contemporaneously with Peking man, and that this advanced human type, not Peking man, must have been the bearer of the culture of Choukoutien. Boule regarded Peking man with his small brain as a savage brute who was hunted and killed like any other game by an unknown, more advanced human type. The irony of the implication that the mental and cultural superiority of this man would be evidenced by his chasing, killing, and probably also eating his physically and culturally less advanced fellow-man, did not embarrass Boule. In all the years during which the cave of Choukoutien has been explored, no trace of a second human type has ever come to light; therefore, there is no reason to doubt the identity of Peking man and the culture of Choukoutien. But Boule's objection brings up a general biological problem that has broader implications than may at first be seen.

Is it possible to infer from the size of the brain the degree of intelligence and cultural efficiency of its bearer, regardless of whether this bearer lived several hundred thousand years ago or lives today? Almost everyone, layman as well as scientist, seems to be convinced that such a correlation is a well-established fact. Some time ago I came across a pamphlet, published in 1934, which was written by an English physician. In the author's opinion the only factors that determined man's evolution since his beginnings as a primitive primate are environment and natural selection. But his starting point is the premise "Cranial capacity is a fairly accurate measure of the mental status from the most primitive primates to *Homo sapiens.*" The self-confidence with which this statement is made is typical.

Of course, there is no doubt that the size of the primate skulls from the lemurs of the Eocene up to modern man has greatly increased. Compared with the great apes, the average cranial capacity of modern man is three times greater—*ca.* 1,350 cc against *ca.* 450 cc. Skull II of Java man (*Pithecanthropus erectus*), the smallest skull of an adult fossil hominid thus far found, has a capacity of only 775 cc (Fig. 1, *B*). This is 43 percent smaller than the average skull of mankind today, or a little more than half the size of the brain of an average American male adult. The cranial capacity of that Java man corresponds to that of a child of today of eleven to twelve months of age. The average capacity of the skull of Peking man is a little over 1,000 cc. This is still one quarter less than the average capacity of modern man. The apelike ancestors of man are unknown. The fossil Australopithecinae of South Africa, which are regarded by Broom as forms lying directly in the human line, have a cranial capacity of not more than *ca.* 450 cc. This is about the average capacity of living anthropoids. If Broom is right in his phylogenetic ranging of the Australopithecinae, then the human brain would have increased considerably since the

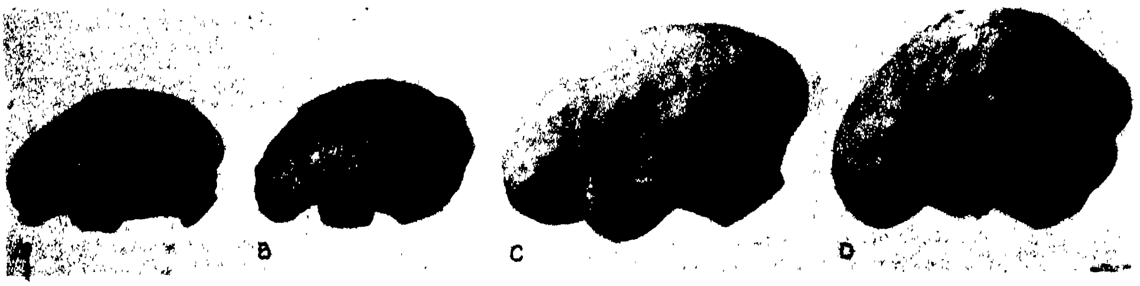


Fig. 1. Casts of the interior of the skull cavity, showing size and form of the brain and its surface pattern from the left side. *A*, adult gorilla; *B*, *Pithecanthropus erectus* II; *C*, Neanderthal man from La Chapelle-aux-Saints; *D*, modern man. About 1/4 natural size. The brain of Neanderthal man had already attained the size of that of modern man, but the contour of the latter is more circular, and it has developed a distinct hump at the vertex.

evolution of man from the South African ape men.

However, we do not know of any fact which proves that the mere increase of the size of the brain is tantamount to an advance in mental ability. Is modern man really more intelligent than Peking or Java man or any great ape only because his brain is larger? This is what Boule's argument implies. More than sixty years ago Marsh claimed that the mass of the brain, particularly that of the hemispheres, has increased considerably in some mammalian orders since early Tertiary times (Fig. 2). Tilly Edinger, who quite recently studied the evolution of the brain of the horse from Eohippus, its earliest recognizable ancestor in the Eocene, to the modern equine form, found that its brain, especially the neopallium, has been enlarged, although the horse did not change its basic organization. This suggests the question: What about the progress of the "mental" qualities of the horse? Is there any indication that the enlargement of the brain implies a greater advantage in the struggle for survival and that the horse only survived because its higher intelligence, bound to the enlargement of the neopallium, conducted it safely for millions of years through the perils lurking about it? Since the horse has survived, it must have been equipped from the beginning and at all times with a certain quantity of brain sufficing to let it find its food and evade its enemies. Were the living conditions of earlier horses much easier? Did they therefore require less attention and precaution than in later times so that the horse could survive in earlier times with a minimum of neopallium, whereas later on it could do so only because the brain enlarged in the meantime and gave it a greater chance? The extinction of the horse in North America was already an accomplished fact by the late Pleistocene, although the animal had already acquired a well-developed neopallium.

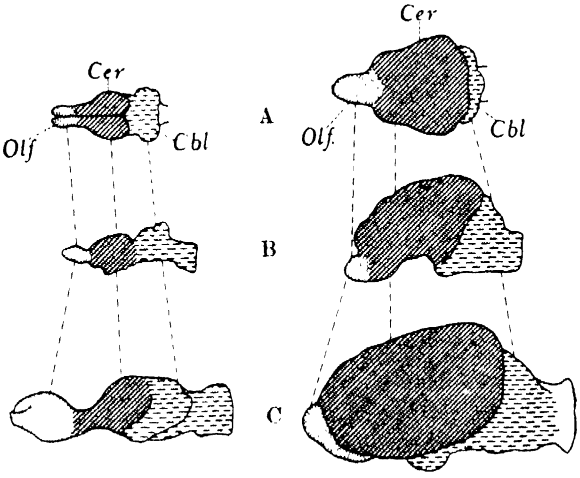


Fig. 2. Enlargement of the brain in mammals from Eocene up to today. *Right*, living animals: *A*, dog; *B*, pig; *C*, rhinoceros; *left*, Eocene type of corresponding mammalian category: flesh eater, small ungulate, large ungulate. All reduced at same rate. *Olf*, olfactory bulb; *Cer*, neopallium (hemisphere); *Cbl*, cerebellum and medulla oblongata (cerebral end of spinal cord). (After Henry Fairfield Osborn.)

On the other hand, increase of body size is always accompanied by an increase of brain size. The elephant, for example, has a brain that weighs almost 5,000 gr, and the brain of one of the big whales (Fig. 3) weighs around 10,000 gr. But the increase of the body alone cannot be made responsible for the growth of the brain; for, in proportion to the weight of the body, the whale has a much smaller brain than man. The whale has 1 gr of brain substance for 8,500 gr of body substance, whereas man has 1 gr of brain substance for 44 gr of body substance. Nevertheless, man is far surpassed in this respect by the dwarf monkeys of South America, the marmosets, which have 1 gr of brain per 27 gr of body substance; and man is surpassed even more in these proportions by the capuchin monkey (Fig. 4) with 1 gr of brain substance for 17.5 gr of body substance.

Therefore, neither the absolute nor the relative size of the brain can be used to measure the degree

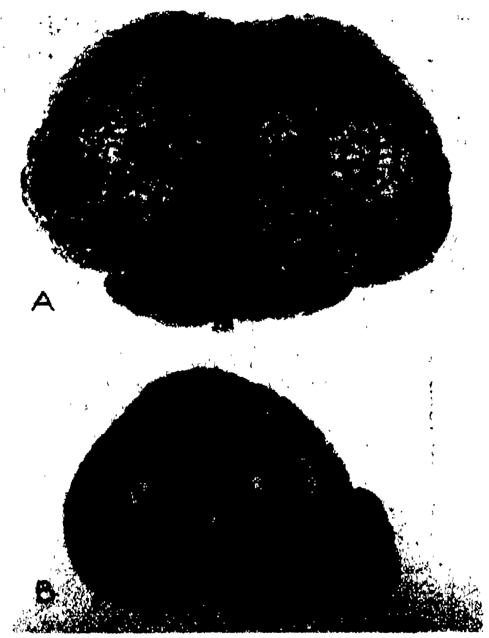


Fig. 3. Brain of a whale viewed from above (*A*); from the left side (*B*). The entire surface of the two hemispheres is partitioned by innumerable large and small fissures, which do not allow any identification with those in *Cebus* (Fig. 4) or chimpanzee (Fig. 5). Note also special form of the hemispheres—they are almost globular, although broader than long and very high. 1/4 natural size. (After F. Tilney.)

of mental ability in animal or in man. So far as man is concerned, the weights of the brains or the volumes of the cranial cavities of a hundred celebrities of all branches of knowledge all over the world have been listed (Spitzka and others). At the bottom of those lists are Gall, the famous phrenologist, Anatole France, the French novelist, and Gambetta, the French statesman, each with about 1,100 cc brain mass. The lists are topped by Dean Jonathan Swift, the English writer, Lord Byron, the English poet, and Turgeniev, the Russian novelist, all with about 2,000 cc. The latter group has nearly double the amount of brain substance of the first group, although the size of their respective bodies does not justify such great differences. Now our mental test! Had Turgeniev really twice the mental ability of Anatole France? If not, and if Turgeniev's body was not of elephantine proportions, which structure of the brain was increased in Turgeniev and the others to such an extent that it answers for the surplus weight, obviously dispensable even for uncommon psychical functions? Gall, Anatole France, and Gambetta, together with innumerable modern human individuals of all races, could perform mental deeds by means of a brain mass which does not surpass the brain mass of Peking man. In addition, the enlargement of the brain does not seem to have reached its climax with modern man. Provided the paucity of the material available warrants such far-reaching conclusions, the climax may already have been reached in the Neanderthal stage, the evolutionary stage that precedes that of modern man (Fig. 1, *C*). In any case, the average cranial capacity of the Neanderthal skulls available is greater than the average capacity of modern human skulls. On the other hand, it can be stated that, at least for the majority of modern mankind, there is no increase of brain mass when compared with that of Neanderthal man.

Fig. 4. Brain (two hemispheres) of a capuchin monkey (*Cebus*) viewed from above. The surface is not corrugated except for a few fissures on both sides. About 1/2 natural size. (After G. Retzius.)

Thus far only the enlargement of the brain, as such, has been dealt with. There is in addition, however, another kind of enlargement of the brain that goes hand in hand with it but obviously depends—at least in part—upon the space of the cranial cavity in which the brain has to be accommodated. This enlargement concerns the surface of the hemispheres. Their outer layer, the cortex, is the most vital part of the brain, since it is the main seat of the nervous cells. Together with their connecting fiber system, it represents the switchboard at which all the stimuli arrive from the periphery of the body and from which reactions and impulses emanate. The expansion of the cortex is brought about by a series of foldings which transform the originally smooth surface into a confusing maze of wrinkles (convolutions and fissures). The greater the number of wrinkles, the larger will be the surface area and the more cortex elements—cells and fibers—could be accommodated. Among the primates, the more primitively organized the forms, the smoother the surface, and the more advanced the forms, the more complicated are the convolutions; the climax is reached in man.

Fossil brains, of course, have not been preserved, but the wrinkles of the surfaces of their hemispheres have left their imprints on the cerebral side of the brain case, where they now appear as ridges. These negatives tell the same story as the wrinkles of the brain surface of living creatures (Fig. 1). In primitive fossil forms, the pattern of the wrinkle system appears simpler than in more advanced primitive forms. This can only be ascertained provided the wrinkles have left their marks on the walls, which, however, does not occur in all cases. In any event, the hominid pattern differs from the simian pattern. Each of the living anthropoids has its characteristics (Fig. 5), although they have some features in common that make possible their distinction from man (Fig. 6).

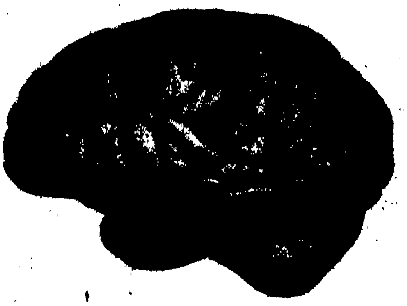


Fig. 5. Brain of a chimpanzee viewed from the left lateral side. The surface of the hemisphere is wrinkled throughout. About 1/2 natural size. (After G. Retzius.)

Again we have to state that when discussing the pattern of the surface of the hemispheres, primates and man do not differ from other mammalian orders with regard to presence and abundance of the wrinkle system. In the evolution of the horse, the surface of the hemispheres is smooth in the beginning, and their folding becomes more complex the more the neopallium increases in size. Indeed, this process is an almost general phenomenon, and any explanation confronts us with the same difficulties encountered in the attempt to interpret the increase of the brain size itself. We are lost again if we suppose that the number or the complexity of the wrinkles is correlated with progress or perfection of the mental faculties. The capuchin monkey (Fig. 4), which many experimental psychologists and physiologists regard as equal in docility to any highly gifted chimpanzee (Fig. 5); possesses an almost smooth brain surface, whereas the chimpanzee has a wrinkled one that comes close to that of man. The whale and its relatives, however, again steal the show. They have the greatest number and finest wrinkles all over the hemispheres, and the most intricate arrangement, in the whole animal kingdom (Fig. 3). Many human and comparative anatomists have spent years classifying and identifying all the individual convolutions and fissures occurring in the different orders of mammals. It may be possible to distinguish the brains of gorilla, orangutan, chimpanzee (Fig. 5), and man (Fig. 6) from one another by this method, but only main fissures which represent the first folds both in ontogenetic and phylogenetic evolution are recognizable and comparable. Their location obviously depends on the manner and rate of growth of the hemispheres in relation to that of the brain case. Except for this basic pattern, the variability is enormous; it differs greatly with regard to the number, length, breadth, depth, and arrangement of the wrinkles, not only between two individuals of the same zoological group or race, but also between right and left hemispheres of one and the same individual (*cf.* the two brains of modern man in Fig. 6). It is therefore hopeless to expect any result or even indication with regard to certain mental faculties—degrees or differences—by such a comparison.

In the face of all these facts it is hard to understand why people cannot get rid of the idea that mere size or configuration of a special convolution or fissure must give a clue to the mental qualities in general and to those of certain individuals in particular. The desire to gaze into the crystal ball seems to exist not only among the clients of fortunetellers but also among scientists.

One hundred and fifty years ago phrenologists indulged in palpating the head and the skull to look for bulges on its outside; from their presence or absence they deduced the presence or absence of psychical qualities which they regarded as

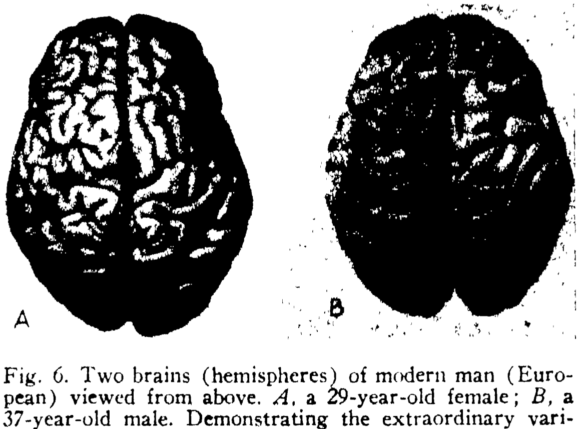


Fig. 6. Two brains (hemispheres) of modern man (European) viewed from above. *A*, a 29-year-old female; *B*, a 37-year-old male. Demonstrating the extraordinary variability of the wrinkle system. The left brain exhibits almost double the number of convolutions and fissures of the right brain, and its convolutions are correspondingly narrower. Note, in addition, the differences in the extension and arrangement of the wrinkles between the left and right side of the same brain. About 1/4 natural size. (After G. Retzius.)

strictly localized. The basic conception of the phrenologists which underlay this reasoning has not changed in the course of time in spite of the great progress made in our knowledge of the structure of the central nervous system. The search has only shifted to a different type of indicator of hidden ability; now it is supposed to be manifested by bulges on the surface of the hemispheres themselves, which allegedly tell of right- or left-handedness, the faculty of speech, musical or mathematical talents. One wants to find out whether certain human races differ from others in their wrinkle patterns and whether those differences are indicative of spiritual inferiority or superiority, etc. Of course, there may be more justification today for such a search than in former times. Owing to pathological incidents and experiments, it is known that certain areas of the cortex are the centers of certain perceptions and impulses. They can be localized fairly accurately, and their nature can be defined. It is furthermore known that those centers are marked by characteristic microscopic structures which permit identification wherever they may be found. It is also known that those differentiations in the structure are not at all bound to the convolutions as such but only to certain areas of the hemispheres, regardless of whether this area is smooth or wrinkled and, if wrinkled, to what degree or extent. As a matter of fact, surgeons are able to remove large portions of the hemispheres, which may result in a temporary or permanent loss of sensibility or power of movement if certain areas are affected. But the defect, as such, does not necessarily interfere with psychical functions or endanger purely mental ones. Modern "psychosurgeons" deliberately destroy fiber systems of the frontal lobe, widely considered the main seat of the intelligence, effecting a return to almost normal reasoning power of some persons formerly considered incurably mentally deranged.

Therefore, the claims of paleoanthropologists, for instance, to the effect that Neanderthal or Peking man was right- or left-handed, was able to speak or write or could only stammer, all deduced from shallower and narrower or deeper and broader impressions on the inside of the brain case, have no scientific basis even if the interpretation of the imprints could be accepted as correct.

If the variation of a normal human brain from average to almost double its usual size and the multiplying of the individual wrinkles of the hemispheres have no bearing on mental functions, how can a strictly localized bulge produce such an effect? On the other hand, thorough studies have revealed a surprisingly perfect equality in weight and surface area between the two hemispheres of the same individual, irrespective of all eventual inequalities in details of their forms (Le Boucq, H. H. Donaldson). Unilateral bulges are the result of asymmetries of the brain case or, if they are bilateral, the result of changes of the whole skull form. Artificially deformed heads or skulls enclose brains with abnormal bulges, depressions, or asymmetries on the surface of one or both hemispheres, according to the character of the applied deformation. But it has never been reported that the people so deformed behave conspicuously differently from others of the same population with normal skulls; nor do they show any change in their mental faculties.

The faculty of the brain to adjust its form and that of its main parts to any form enforced on the enclosing brain case by altering body conditions is extraordinary. One of the most characteristic features of all known earlier hominids, including Neanderthal man, is the flatness of the brain and brain case in proportion to the length, irrespective of the size of the brain and the capacity of the brain case (Fig. 1). In all modern human races—whatever the brain size or skull capacity—the vertex region is distinctly elevated (Fig. 1, *D*) and the length of the skull base is shortened. These differences are the effect of a sharp bend of the base of the brain case (Fig. 7) which runs in a straight line in quadrupedal animals (Fig. 7, *A*), even in the great apes (Fig. 7, *B*). Early hominids (Solo man) are in an intermediate position in this regard. The transformation of the modern human skull is the final result of a change in the static and dynamic conditions entailed by a more perfect adaptation to man's upright position (Fig. 7, *C*). That this alone is responsible for the alteration of the form of brain and brain case can be deduced from the fact that in whales and related forms (Fig. 7, *D*)—regardless of the size of their brains—almost exactly the same change takes place, with one remarkable difference—that is, the bend is reversed. The base of the brain case is curved upward in accordance with a perfect adaptation of the skeleton of these animals to fast and powerful swimming and diving.

All recorded facts indicate that neither the size nor the form of the brain or the surface of the hemispheres or their wrinkle pattern in general or in detail furnishes a reliable clue to the amount and degree of general or special mental qualities. Nevertheless, there seems to be a parallel between the expansion of the hemispheres, the advance of the bodily evolution of man, and the increase of mental qualities. But no one can tell what the nature of those correlations may be. In any case, this is a problem that principally concerns neurologists and psychologists. Fossil human material can, if at all, furnish only circumstantial evidence. There have been suggestions that blood supply may play a decisive role in the differences between the psychical functions of notable brains and ordinary ones. If this is so, it may also be true for the difference between the brains of early hominids and those of modern man. Indeed, the holes and gaps of the bony case which lodges the brain and through which both arteries and veins pass are distinctly wider in modern man than in early hominids. There is a definite tendency for these ways of communication between the cranial cavity and the outside to widen in the course of evolution as part of a general trend to reduce the thickness of all cranial bones, which are surprisingly massive in early hominids. In general, the passages through the brain case are distinctly narrower in skulls of

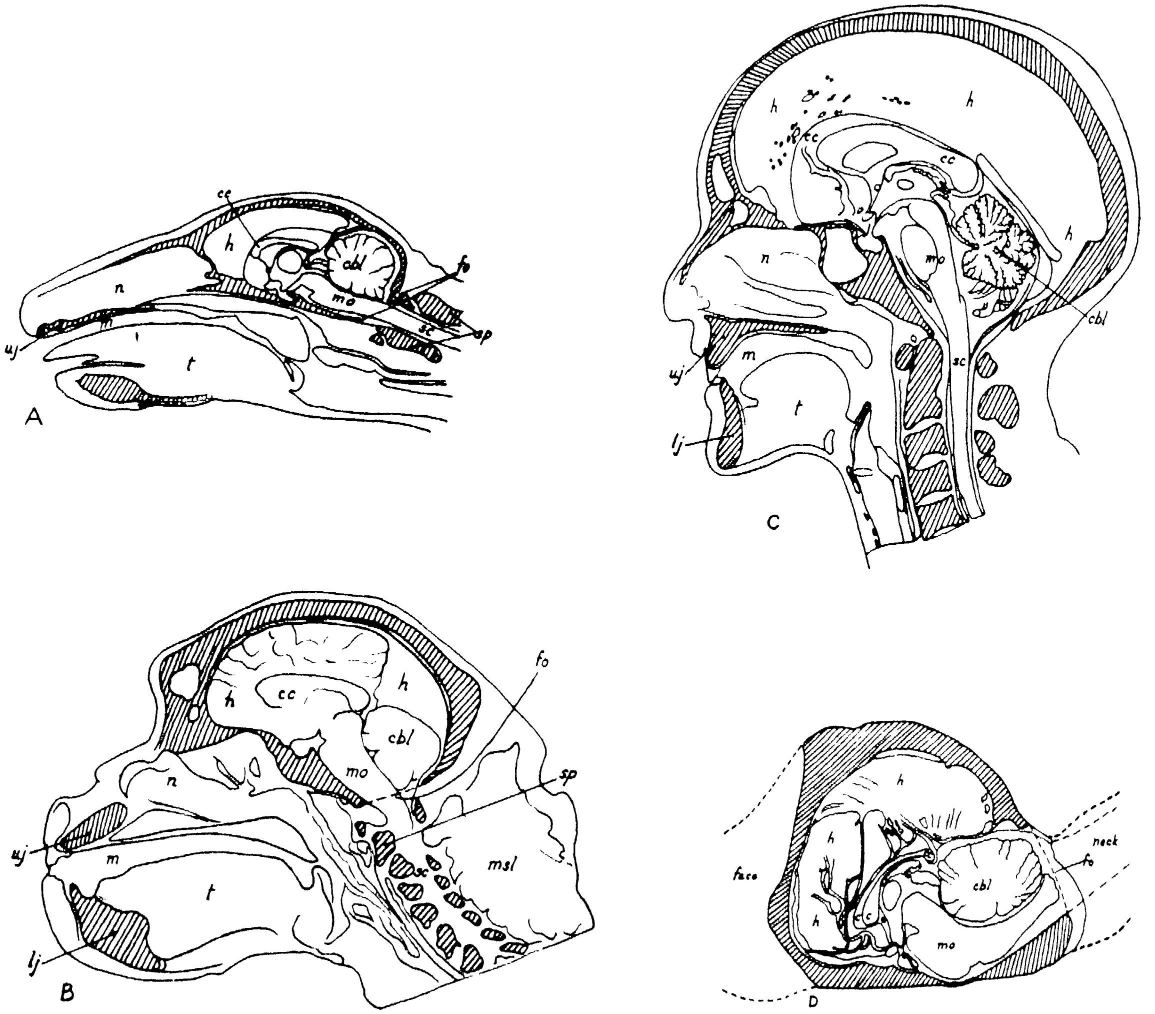


Fig. 7. Demonstrating on mid-sagittal sections through the head the extent to which form and orientation of the brain and its parts depend upon the form of the enclosing brain case and its adaptation to a change in the posture of the body. *A*, example of quadrupedal horizontal orientation of brain case and spine, adult lemur; *B*, example of semierect posture, young chimpanzee. *C*, example of perfectly erect posture, adult European. *D*, example of a fast-swimming and diving animal, adult dolphin. In *A*, spine and spinal cord meet brain case and brain directly from behind. In *B*, they meet brain case and brain from behind and below. In *C*, they meet brain case and brain directly from below. In *D*, they meet brain case and brain from behind and above. The base of the brain case (*hatched as the entire brain case*) forms a straight horizontal line in *A*; a straight inclining line in *B*; a downward bend to nearly a right angle in *C*; and an upward-directed curve in *D*. The brain and its parts: (*cc*, corpus callosum; *h*, hemispheres; *mo*, medulla oblongata; *sc*, spinal cord) closely reflect in course and topographical relations the changing conditions. (*A*, *C*, and *D* after F. Hochstätter.) *A* reduced to 1/2 natural size, *B* to 1/4 natural size, and *C* and *D* to 1/3 natural size. Other abbreviations; *cbl*, cerebellium; *fo*, occipital foramen; *lj*, lower jaw; *m*, mouth; *msl*, cervical muscle; *n*, nose; *t*, tongue; *uj*, upper jaw.

races morphologically more primitive than others. But no data with regard to differences in their width are available for small- and large-brained individuals.

Another point may be of some interest. It is known that in lower vertebrates the mass of the spinal cord is greater in proportion to that of the brain than in mammals. Even compared with the great apes (gorillas), the mass of the spinal cord of modern man seems to be much smaller. This suggests that the brain tends to "swallow" the spinal cord and thereby to bring more reflex centers under control of consciousness. This certainly broadens the basis of psychic reaction.

As to the size of the brain, its white substance (medullated nerve fibers) profits most by the enlargement. But the great accumulation of myeline in these fibers used as insulation material may not be its only purpose. Hollow spaces within the bones are used in all mammals as storage rooms for fat and for the lodging of organs of blood formation. It may be that the central nervous system not only has a nervous function but also serves as storage for some stuff which is an essential in the metabolism of the organism.

To return to our starting point: the course of man's bodily evolution can be clarified by the study of fossil human remains if it is done by people trained in comparative human anatomy. But studies made on skeletons alone will never enable us to make statements about either the mentality of the individuals concerned or about mental change or progress over a period of time. Cultural objects are the only guide so far as spiritual life is concerned. They may be fallacious guides, too, but we are completely lost if those objects are missing. And the closer we come to more primitive stages, the less likely we are to discover cultural objects.

◆

THE SCIENTIST

As from the darkened and bewildered deep,
Soundless and slow the ocean currents rise,
And breast the night, and climb into the skies,
Till black waves break against the whitened, steep,
Desired cold continent of the moon, and leap
(The starry lights about them, all the dyes
Of heaven flashing where the spent spray flies)
And, foaming earthward, fall at length to sleep—

So man above the ocean of his days
By search and knowledge climbs until he sees
About his head the stars. About his knees
The world awaits, and when he's home again,
Though he grow humble, earthly all his ways,
A shining's there, and the high stars remain.

HAROLD LEWIS COOK

THE BASIC POSTULATES OF PSYCHOLOGY

W. EDGAR VINACKE

After receiving his Ph.D. from Columbia in 1942, Professor Vinacke spent two years with the Civil Aeronautics Administration in research on the problems of selection and training of aviators. He has been assistant professor of psychology at the University of Hawaii for the past two years.

THE science of psychology has been developing and growing for almost seventy years. Its origin, in modern terms, is generally dated from the year 1879, when Wilhelm Wundt established the first psychological laboratory at Leipzig. That historic event marked the culmination of a long gestation, during which psychology was grown out of physics, physiology, philosophy, and even astronomy. Wundt started psychology off on its extensive experimental career, but the psychology of today would hardly be recognizable to him and his colleagues. A great deal of the period since 1879 has been devoted to controversy, as everyone knows, between a succession of comparatively narrow viewpoints. The most famous of these in America are existentialism, functionalism, behaviorism, and gestalt psychology. Psychoanalysis and related schools should be added to this list, although they were developed by psychopathologists, since they have profoundly influenced (and frequently annoyed) psychologists of all kinds.

It is not my intention to review these controversies. That has already been well done by half a dozen psychologists. Such writers usually end by pointing out that each school makes its valuable contribution, but that most psychologists are eclectics and do not subscribe to any single viewpoint. They suggest that the future will evaluate various theories and ultimately combine them into a unified system. Perhaps the time has come to begin this task. This article is an attempt to formulate the main outlines of psychological thinking as it exists today, not with respect to specific problems, but in terms of the axioms, so to speak, that have become established during its formative period. Students in psychology classes frequently worry over the apparent lack of agreement among psychologists, and other scientists have reactions varying from amusement to disgust in this regard. My thesis is that, actually, all modern psychologists stand on approximately the same general platform, which may be called the "psychological viewpoint."

It is to be hoped that the formulation of this viewpoint will serve a double end. On the one hand, it may indicate that there *is* a "psychological viewpoint," the fruit of past thinking about human behavior. Thus, other scientists may come to understand better what psychologists are thinking nowadays. On the other hand, it may serve the function of an examination of where we stand on historical issues.

Many omissions will probably be evident. Some, to be sure, are intentional, either because no general agreement as yet exists or, perhaps, because psychology today does not deal with such matters. Running the risk, then, of incompleteness, the following postulates are advanced to define the modern "psychological viewpoint."

I SHALL develop thirteen general propositions which seem to me to characterize modern thinking in psychology.

Postulate 1: *The material of behavior.* The same fundamental variables are present in the total behavior of all human beings, although all degrees of these variables may be observed; differences between individuals are largely differences of degree rather than of kind.

The foregoing postulate is not intended to convey the idea that all people are alike; far from it. Apart from the fundamental variables, of course, there are innumerable other variables that may be unique to an individual, a group, a sex, or a race. The argument, on this point, largely concerns the formulation of what constitutes a basic variable. The problem is to isolate and state these variables in cooperation with the other sciences of man.

For the present purpose, a fundamental variable may be regarded as a broad, generalized determinant of behavior in the organic structure of man, or in the world that confronts him, or in a relationship between organism and world. A fundamental variable is, so to speak, a starting point of behavior.

The main point here is that psychologists think in these terms. Glance at any of the leading textbooks in introductory psychology. One finds treatments of a wide range of subjects, from emotion and motivation, through sensory processes and perception, thinking, learning and memory, etc., to individual differences and personality. In all these the assumption is tacitly made that, although people differ, they all function in the same general ways and face essentially the same problems.

The following examples illustrate this basic feature of psychological thinking:

a) *Organic drives.* Although no final list of organic motivating factors has been presented, and although different terms are used for the same phenomena, we can say that the sex, hunger, thirst, and oxygen-inspiration drives are universal, leading to positive approaching behavior. Contrasted with these are others leading to negative activity, such as excretion and avoidance of injury. The ultimate role in individual behavior of such drives is another matter; at least they enter into the development of each individual to some extent. They are starting points on the organic side of long chains of behavior processes.

b) *Group adjustment and institutionalized behavior.* All individuals are confronted by other people and organized group codes, if only during the helpless period of infancy. All individuals are influenced to some extent by these factors. The fact that established codes or norms confront the organism is a starting point on the side of the external, real world.

c) *Learning, memory, and higher thought processes.* All individuals are modified by dealing with the environment, retain some, at least, of this experience, and make use of it in dealing with later problems. Apparently, some specific laws can be stated in this connection, such as that forgetting is most rapid immediately after learning, and then slower; and that two tasks done in succession and similar to each other are less efficiently learned than two dissimilar tasks done in succession.

d) *Sensory processes.* All people possess variations of the same general sensory functions within quite specifiable limits. Thus, the range of human hearing is 20–20,000 cycles per second, and the average reaction time to a visual stimulus is 200 ms, which is somewhat slower than reaction time to sound (In this discussion, we have not considered some special qualifications, as with regard to feeble-minded or other exceptional persons who would represent the extremes. *Cf.* Postulate 3.) Examples *c* and *d* represent factors of organism-world relationships as determinants of behavior.

Postulate 2: *Uniqueness.* Every individual, as a total personality, is different from every other individual, as a result of past experience in conjunction with heredity, because no combination of factors is ever precisely the same for any two individuals.

The foregoing axiom is not a contradiction of the first, but a further, and essential, development of it. The chief argument is the dividing line between an individual and general characteristic, or how general a psychological law can be. As G. W. Allport has pointed out, "A general law may be a law that tells how uniqueness comes about." The latter kind of law is necessary, as the modern and complex study of personality is repeatedly demonstrating.

We can say that psychologists do not ignore the uniqueness of the individual, even when thinking in terms of Postulate 1. They recognize that the interplay of basic behavior variables, cultural and group influences and disciplines, and "accidental" situations and experiences add up to unique total personalities, if only because the combinations and permutations are so enormous in number. This feature of the psychological viewpoint is a prime reason for the inclusion of long chapters on personality in modern textbooks, which are otherwise framed in terms of Postulate 1. The more advanced student is far more extensively indoctrinated in terms of Postulate 2. The modern development of the field of personality is largely concerned with it.

Postulate 3: *Normality.* "Normal" behavior is a relative matter; it is that behavior which characterizes most people within a given cultural group (with certain privileged exceptions, such as the artist). "Abnormal" behavior is extreme deviation from a given average point in terms of a defined variable, or group of variables.

This postulate is primarily statistical. It explodes the fallacy that people are separable into distinct types (a feature of naive, everyday thinking, by the way) and states that people who differ widely, say, in introversion-extroversion, are simply extremes of a continuum where most people are neither markedly "introverted" nor markedly "extroverted." In other words, the normal probability distribution is now regarded as characteristic of most, if not quite all, psychological variables.

In discussing "normal" people, therefore, the psychologist is making no qualitative distinctions, but is simply dealing with the average person. Since it happens that modern psychology is primarily a development of Western culture, we should qualify Postulate 3 in terms of its frame of reference. Implicitly, the American psychologist is thinking about that behavior characteristic of most American individuals (in terms of Postulate 2—i.e., over and beyond the fundamental variables). It would be a valuable project for a group of Hindu or Chinese psychologists to develop a normal psychology of Hindu or Chinese people. To some extent this kind of development is no doubt occurring in Soviet Russia. In any event, it is evident that normality, as we have defined it, may be very different in different cultures.

The concept of "abnormality" is a corollary of that of "normality." It is that behavior which deviates to an extreme degree from the average, whether with respect to intelligence, adjustment to the group, emotional-motivational behavior, etc. The "abnormal" is therefore only an arbitrary and fairly extreme distinction from the normal.

Postulate 4: *Physiological concomitants.* Every manifestation of behavior in an organism (here, the human being) occurs in association with some physiological change; no human behavior can take place without some change in the organism.

Not much comment is required here. Stemming largely from the arguments of the behaviorists, modern psychological thinking rests upon the assumption (as yet it is often only an inference) that the organism functions in terms of its structure and the properties of its organized tissues. Even a thought, however original or untraceable in origin it may seem, nevertheless depends upon some activity of the nervous tissues. Even an emotional impulse is somehow a chemicophysical response.

Postulate 5: *Heredity-environment.* The human organism, like all living things, is the joint product of heredity and environment; no aspect of the behavior of an individual is entirely independent of either heredity or environment.

One of the great controversies in psychology, and one vital to most psychologists, concerns the contribution to behavior of "nature" and "nurture." In a sense, there are two aspects of the problem. On the one hand, the effects of hereditary and environmental factors on behavior may be rather neutrally examined; on the other, an inevitable assessment may be made of their relative importance. In any event, despite wide divergences of opinion on the latter point, nearly all psychologists today recognize the joint contribution to development of both factors. If the current standard textbooks be regarded as representative of psychological thinking, Postulate 5 must be regarded as valid.

Evidence for the truth of Postulate 5 is singularly difficult to obtain, to be sure, because of the difficulty of controlling and measuring the development of human beings. One of the wisest and most careful of psychological observers, R. S. Woodworth, has made an admirable study of the evidence. Reviewing the studies made of twins (identicals and fraternals), foster children, and children in institutional homes, he presents a clear analysis of the present status of the problem. The total picture is one of the inseparability of hereditary and environmental factors. In any event, we cannot now say that either heredity or environment is *the* important factor in development. Both jointly determine ultimate behavior, in a complex interaction, in a multiplicative (since neither can be zero) rather than additive sense. It must rest here until further work has been done.

Postulate 6: *Socialization.* Human behavior of any sort can be fully understood only in terms of the social context in which it developed and in which it functions.

It is in connection with this postulate that the anthropologists have made the most valuable contributions to psychological thinking. As was noted under Postulate 3, general psychology frequently takes the social context for granted, simply because it is common to both psychologists and students. In the fields of personality and social psychology, however, it cannot be tacitly assumed, but must be examined. The result is that "human nature" is shown not to be more or less the same the world over, but that the human individual is a complex "organism-in-society." Hence, the current interest in attitudes, for example, as one area in which the dependence of the individual upon his society is most readily (although not easily!) studied.

Postulate 6 may not seem to harmonize with Postulate 1, but there is no actual contradiction. In Postulate 6 we are dealing with activity in progress, so to speak, and the behavior that develops during the life history; in Postulate 1, we dealt primarily with the starting points of behavior, which are quite rudimentary and largely undetermined in their ultimate direction and expression.

Psychologists nowadays have learned to be cautious in ascribing to "human nature" any aspect of behavior without at least taking account of the implications of Postulate 6. Indeed, they have criticized Freudian psychoanalysis in precisely these terms. They beware, now, lest ethnocentrism of any variety befuddle their thinking.

Postulate 7: *Objectivity.* No interpretation of human behavior can be fully accepted unless it is founded on fact substantiated by scientific evidence.

No particular comment is necessary. Today's psychologists, keenly aware of the difficulties involved in the study of behavior, and strongly influenced by the standards of other sciences (more so, sometimes, than scientists in those other fields), think in careful methodological and evaluative terms. They beware just as much (more, in fact) of egocentrism as of ethnocentrism. True, there is still a long way to go, but every modern reputable psychologist receives intensive training in terms of Postulate 7. The behaviorists are especially to be thanked for this trend.

Postulate 8: *Finality.* In the study of human behavior, no interpretation, conclusion, or law is necessarily final, but is given in the light of the best present knowledge; allowance must be made for the possible operation of unknown or uncontrolled variables, and for other possible limitations of knowledge or outlook.

This statement, also, requires little comment, since it is no doubt basic to all fields of inquiry. It means that psychologists, on the average, do not pretend to have all the answers. They continually strive to improve their methods and to push further the frontier of our understanding of man; they continually examine and retest their existing experimental results and theoretical formulations. Psychologists are still hazarding hypotheses and conducting investigations. Psychologists are no more complacent, arbitrary, or pedantic than other scientists, who also retain a sense of curiosity, of criticism. The realm of understanding has not yet been conclusively defined.

Perhaps more than any other, Postulate 8 is the reason for writing this article. It reflects the fact that in the history of psychology we have often had to change our interpretations of behavior as new facts and theories emerged. Thus, it becomes valuable, as in the present instance, to formulate our position as of the present. But it is not necessarily an absolute or final formulation. The psychology of the future may force us to revise or reject our basic postulates or to add new ones.

Postulate 9: *Modification*. Human behavior is not static; the individual develops and his behavior is modified and remains modifiable as long as life continues.

Modern psychology, with its intense emphasis on learning, development, and activity, would not be recognizable to psychologists of fifty or sixty years ago. No modern psychologist thinks of a human being as a static, largely unvarying thing, to be described in terms of sensations, images, feelings, and ideas. Instincts, as universal, fixed determinants of behavior, have been rejected. Even if certain innate forces do exist, it is now being shown, even in lower animals, that they are modifiable when conditions change.

Similarly, the relationship between childhood, adolescence, and adulthood is now continually emphasized and studied. Nor is prenatal development ignored. Modern psychology is no longer that of an adult (primarily male), of twenty to forty-five years of age, who resembles a mounted specimen in a museum showcase. Rather, man is studied in terms of continuous growth and function, literally from conception to death. Thus, even when adult behavior is the focus of attention, it is not considered to be rigid, and senescence will become an important field of research in the future.

Postulate 10: *Measurement*. Potentially, all human behavior can be measured and described.

This postulate implies that, however mysterious some aspects of human behavior may be, they are ultimately understandable in scientific terms. All too frequently, the modern psychologist must qualify his interpretations with the statement "We are not yet sure," or "That has not yet been determined." However, most psychologists are certain that sooner or later investigation will uncover the still-hidden facts of behavior.

Postulate 11: *Diverse views*. Human behavior may be, and has been, interpreted from different viewpoints, no one of which is necessarily right or wrong, and all of which may contribute to complete understanding.

Postulate 11 is the modern eclectic viewpoint and reflects a moderate approach to the assertions and theories of different "schools." Glance through any standard textbook on psychology or the bibliography of almost any article that deals with a general problem. It will be apparent that all the "schools" are represented. In the former case, the history of the great controversies is presented in disguised form. For example, the chapter on motivation and personality will reflect psychoanalytic contributions; the chapter on learning will probably owe much to behaviorism; the chapter on sensory processes can be traced, in part, to Titchener; and the chapter on perception will lean heavily on gestalt psychology. Even William James will enter into the discussion, at least in the chapter on emotion. It is no wonder that the modern psychologist thinks in terms of Postulate 11. He is historically determined, as E. G. Boring has sagely pointed out.

Postulate 12: *Fractionation*. If, for convenience, aspects, or parts, of human behavior are studied separately (or if apparently divisible units are analyzed), it cannot be fully understood or described in terms of these elements, but must finally be viewed as a whole organization.

The above statement sums up the most significant contribution of the gestaltists to psychological thinking. So insistent have they been, and so articulate, that psychologists of the present think less in terms of units (such as sensations, images, and conditioned reflexes) and more in terms of patterns and sequences. No aspect of behavior is seen as separate and distinct from other aspects; the activity in progress at the moment is viewed in relation to the total state of the organism, as organized on the basis of past experience, other current functions, and even, when necessary, foresight or planning for the future.

Although the gestaltists have dealt most extensively with visual perceptual processes, psychologists have been markedly influenced by them in other connections also. Referring once more to standard texts, one would find in nearly every chapter sections devoted to the relationships between diverse phenomena, functions, and activities. There would be found many cautions against fractionating behavior, toward the end of showing the total individual, not as a sum of elements, or a composition of units, but as a complex interrelated organization of continuous patterns, functions, and processes.

Postulate 13: *Dynamics.* All human behavior has a cause.

Freud contributed to psychology, as much as anything else, the important viewpoint that mental processes, emotional impulses—indeed all activities—are dynamically caused, that there is a reason for their occurrence. No matter how irrelevant, accidental, or inexplicable a person's behavior may seem, it does'not occur simply at random, but stems from some actual, though perhaps concealed, source within the individual. Psychologists now appear to accept this idea.

Postulate 13 may, perhaps, be regarded as a correlate on the psychological side of Postulate 4.

In the foregoing discussion I have endeavored to set forth the broad outlines of the current psychological viewpoint, with some measure of historical perspective. The era of "schools" is about over in psychology, as Keller observes. The eclectic psychologist no longer troubles himself very much with them, save in historical surveys, and the standard textbooks represent fairly well a synthesis of them. I believe that the average student now gets substantially the viewpoint presented above.

This article is designed to present the broad framework of this thinking for the consideration of other scientists. All too often there is doubt as to what it is, and whether any progress has been made. Actually, psychology has shown a healthy development, as a result of a vast and lively body of experimentation and systematic thinking. Nor has this growth displayed the slightest sign of diminishing return. On the contrary, these paragraphs may be regarded as the discussion of a temporary condition, really the bare minimum of conceptualization in psychology as it exists today.

⁂

Photographic Exhibitions

Entries in the *Second Annual International Photography-in-Science Salon* will be received by The Scientific Monthly July 26–August 16, 1948, inclusive. They will be judged on August 21, and those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, September 1–21. Already booked through April 1949, showings after that date may be arranged.

The Biological Photographic Association will hold an exhibition in Philadelphia, September 8–10, in Houston Hall, University of Pennsylvania, at which prints, color transparencies, and motion pictures in the field of biological photography will be shown. There will also be organized symposia and demonstrations.

THE IMPORTANCE OF THE INDIVIDUAL IN HUMAN EVOLUTION[*]

HAROLD F. BLUM

Dr. Blum (Ph.D., California, 1927), who has taught physiology at Oregon, Harvard Medical School, and California, was associated during the war with the Naval Medical Research Institute, Bethesda, Maryland. He is now a physiologist at the National Cancer Institute and visiting lecturer in the Department of Biology at Princeton University. The article below was written while Dr. Blum was a Guggenheim Fellow.

For man is enabled through his moral faculties to keep with an unchanged body in harmony with a changing universe.—Alfred Russell Wallace (quoted by Charles Darwin).

MAN'S behavior is a part of man himself, and his history and culture, which have helped to shape this behavior, are parts of his evolution. This premise must, I believe, be accepted before attempting to view human evolution in its entirety. For purposes of analysis, one may choose to treat human evolution at a purely biological level, but in doing so it should be recognized that a part of the total picture is being omitted. Man's influence upon evolution in general, resulting from the environmental changes he has created on the surface of the earth, is a factor of rapidly increasing importance. Can we neglect this factor in evolution even at the biological level? And, conversely, it should be admitted that the factors that account for organic evolution may not be sufficient to explain the whole of human evolution in the above broad sense. If I view the matter correctly, misconceptions occur not infrequently because of failure adequately to analyze the factors involved; and it is with the hope of dispelling some of these misconceptions that this article has been written, not to introduce any novel biological principle.

What I shall emphasize is that certain main principles underlying the behavioral aspect of human evolution are not only different from those upon which organic evolution is based, but that they need to be regarded, quantitatively, in a quite different manner. In the end we shall arrive, I believe, at a point of view harmonious both with the ideas of the evolutionary biologist and of those more immediately concerned with the humanities.

Let us first examine very briefly the essence of the neo-Darwinian concept of evolution. Essentially, it involves variation by genetic mutation and recombination, and natural selection by elimination of the least-fitted to the environment and isolation of the old from the new. Strictly, the phenotype—the individual as outwardly expressed—is the point of impingement of natural selection; but it is the genotype—the genetic individual—that must be "selected" in order to accomplish evolutionary change. I shall assume without reservation that these principles apply to the evolution of man both in the past and in the present, rejecting the Lamarckian concept of inheritance of acquired characters in any biological sense. This is the point of view generally accepted by biologists, and I will not be concerned herein with the question of whether it is adequate to describe all organic evolution, which question has relatively little bearing upon my thesis.

In rejecting the concept of inheritance of characters acquired within the lifetime of the individual, it may be pointed out that the history of attempts to demonstrate by experiment the existence of such a mechanism has been one of consistent failure. Even more pertinent is the failure to formulate a reasonable theoretical explanation for such inheritance of genetic characters. The concept has always been a more or less tempting one because by accepting it all the difficulties of explaining adaptation to environment are removed—by definition, as it were. Charles Darwin himself—possibly in despair of any other explanation—tacitly accepted the concept in formulating his theory of "pangenesis," which he developed subsequent to the appearance of the *Origin of Species* in 1859, and set forth in his *Descent of Man* in 1871. Pangenesis was quite widely accepted until it was rendered untenable by the development of modern genetic theory. It is of historical interest that the concept of inheritance of acquired characters, to which modern Darwinism is directly opposed, flourished for a considerable period under the aegis of Darwin himself, and subsequent to his formulation of the concept of natural selection upon which his fame deservedly rests.

The individual. In comparing the individual of a species with his species mates, it is usual to study one aspect at a time. If this aspect is measured, it is always found to vary, within limits, from individual to individual. Thus, measurements of a given aspect X—which may be the height of a tree or the weight of a man—are found to distribute themselves among the population in a manner something like the curve A in Figure 1. For the greater proportion of the individuals in the population, aspect X has a value near the mean, the proportion decreasing as we move away from the mean in either direction, until the extremes are represented by only a very few individuals.

---

[*] From an address presented before Section L, AAAS, December 1946.

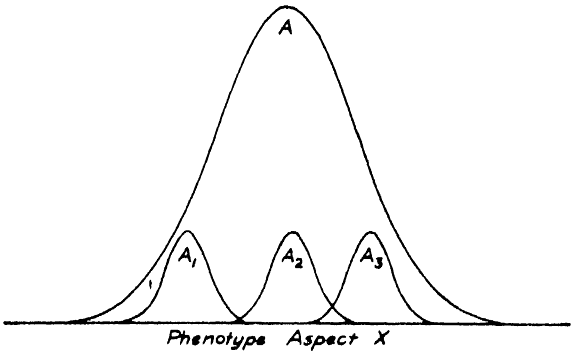


Fig. 1. Scheme to illustrate the distribution of a given aspect X, within a species, A, and within pure genetic lines isolated from that species, A_1, A_2, A_3. The curve drawn is that for a normal distribution and may be regarded as the "ideal," whereas those distributions actually found in nature are likely to be skewed or otherwise modified from this basic form; but for simplicity we may assume that this distribution is characteristic of any aspect with which we may be concerned.

If individuals from one extreme of such a distribution are selected and bred together, and selection repeated in like manner for the offspring, a pure breeding line may be obtained which will show less variation of the individuals with regard to this aspect. Such a pure line might have a distribution like that indicated by curve A_1 in Figure 1. The pure line is established within a few generations, after which further selection within this line will not result in further narrowing of the distribution, and it is to be concluded that this occurs when all the individuals with the pure line have identical genetic make-up, at least as regards the aspect under study. By this process of selection and inbreeding, it may be possible to obtain several pure lines, as indicated by the curves A_1, A_2, and A_3 in Figure 1. Within these pure lines, genetic uniformity has been arrived at, but not complete uniformity of the group as outwardly expressed by the individuals. At least some of the individual variation may be due to inequality of environment, but this of course has no influence on the genetic make-up of the individuals. Since its demonstration by Johannsen early in the century, such isolation of pure lines has been carried out in many species of plants and animals. No such experiment has been performed on man, of course, but there is every reason to expect the same kind of result.

There will occur rarely among the population of a pure line an individual with a somewhat different genetic make-up as regards a particular aspect. This is called a mutant, or sport. If a population composed of the offspring of such a mutant individual could be immediately isolated as a pure line, and compared to the original pure line from which it arose, the distribution of aspect X in the two pure lines might be respresented by the curves A_1 and B in Figure 2, this aspect being distributed about different means in the two cases. Actually, it is not possible to isolate the mutant line immediately, but by conscious selection on the part of man this could be accomplished within relatively few generations.

In nature, the isolation of such a line would take longer, but if the mutant line were in some way better fitted to the prevailing environment than the original line, its proportion in the population would tend to increase, and it might eventually dominate, or under proper circumstances eliminate its predecessor. This represents the process known as natural selection, and such selection of mutant forms is the basic mechanism by which evolution in the neo-Darwinian sense is accomplished. (These and some other of the statements regarding mutation and natural selection have been oversimplified here for the sake of brevity.)

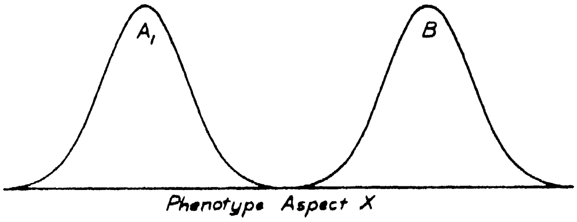


Fig. 2. Scheme to illustrate the distribution of aspect X, in a pure genetic line, A_1, and a mutant therefrom, B.

Thus far the discussion has, for the sake of simplicity, dealt with one aspect only. To describe a population completely would require the description of a myriad of separate aspects, and this could only be accomplished with a three-dimensional diagram. To make such a diagram one might place face to face a great number of sheets of paper each having the shape of, say, the area under curve A_1 in Figure 2, each sheet being supposed to represent the distribution of a different aspect, closely related aspects being placed next each other.

In this way might be constructed a block shaped as represented in Figure 3, which would describe the population completely as regards the aspects included. The block might be thought of as describing the whole of the population on which the aspects were measured, and then any individual from this population could be described by a contour line on the surface of the block, such as that indicated by *I* in Figure 3. For most individuals, this contour might be expected to follow the ridge of the block fairly closely throughout most of its course; that is, it would describe the individual as being close to the mean in most aspects. In some aspects, however, the individual might be close to an extreme, as indicated by the sharp swing of the contour represented at *E*.

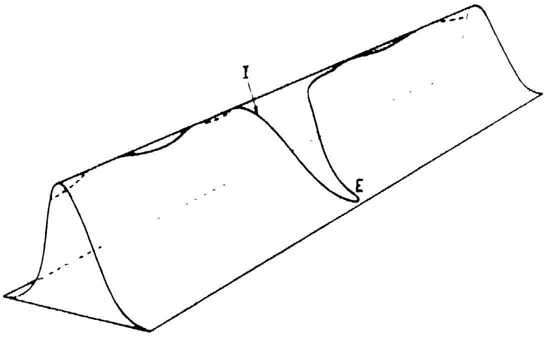


Fig. 3. Three-dimensional scheme to represent the distribution of many aspects of a population. Any cross section of the block may be thought of as representing a single aspect. Thus, the whole block describes a population. The diagram might be improved by having the block curved upon itself in a circle to represent an endless succession of aspects, those intimately related always lying adjacent.

Cultural evolution as contrasted to biological evolution. Let us now assume, for the purpose of developing our argument, that the block shown in Figure 3 presents a pure line of human beings, and that the particular aspect that is represented by the cross section in which *E* falls is something we shall call "inventiveness." If an individual displaying an extreme of this aspect mates with another individual who also displays an extreme of this aspect, the offspring of the mating would have no greater chance, biologically, of showing extreme inventiveness than the offspring of any other mating within the whole population. Remember that this is said with reference only to the case of a hypothetical pure line, for this need not be true for mating within the real population where the two individuals with extreme inventiveness might actually belong to genotypes superior in this aspect, although the chances of this happening would probably not be very great for any single mating.

On the other hand, if, under any circumstances of heredity, an individual actually creates an invention which becomes accepted by, and so has an influence upon, the culture of the population as a whole, the individual will have had a direct effect on human culture and will have contributed to cultural evolution. The same may be said, of course, for an abstract concept which becomes a part of human culture and which may have more far-reaching consequences than a material invention. In a cultural sense, the group as a whole has inherited a facet of culture directly from the individual, and with regard to this particular cultural facet the individual enjoying an extreme of a certain aspect may be more important than the group as a whole. This is a very different matter from genetic inheritance, and evolution in the strictly biological sense, where the individual displaying the extreme of any aspect is of no more importance than any other member of the genotype. On the other hand, the ability to transmit ideas and concepts from one generation to another is a form of "inheritance" which has no parallel in a strictly genetic sense.

Human evolution, in a biological sense, is no doubt going on continually. There is no such thing as a pure line of human population, and hence selection of characters having survival value of one kind or another may go on to a certain extent. Since new mutants may arise from time to time that have survival value, a certain amount of evolution toward better adaptation to the environment may be expected, but this is an extremely slow process in terms of human history. Mutations involving some aspects probably occur today about as rapidly as once per hundred thousand or million of the population per generation, but only a few of these can be expected to have survival value; probably most will be detrimental. For the most part, those that survive can be expected to provide only relatively slight alterations of type. The effect of these mutations on the character of the population as a whole must require a long time to manifest itself even under the most favorable circumstances.

Selection by man himself, if we could trust his knowledge and judgment, might cause slow improvement along special lines. That the eugenist's attempt to eliminate from the population certain clearly defined detrimental inherited characters may have its dangers when his methods are employed by irresponsible groups has, however, had recent frightening demonstration that need not be dwelt upon. And under the best handling this must be a slow method of improvement, as well as one to be approached with caution.

The idea that human evolution in a desirable direction is favored by encouraging severe competition between individuals of the human population to accomplish "survival of the fittest" is fallacious because it does not recognize the true implications of natural selection. ("Survival of the fittest" was Herbert Spencer's more dramatic synonym for Darwin's "natural selection.") Competition between species may eliminate the weaker species; competition within a species might eliminate the species itself. In its evolutionary effect, natural selection is not concerned with the individual, but only with its genotype:

> So careful of the type she seems,
> So careless of the single life.

In contrast, the individual would seem to play a dominant role as regards cultural evolution. To be sure, any contribution to the culture of the group must be taken up by the group as a whole in order to have any extended effect thereon, and whether a new invention or idea is taken up may depend upon whether circumstances are favorable to its reception; these circumstances may involve a complex of factors. But it would seem generally to require the contribution of a single individual to initiate such a cultural change. We are here concerned only with human culture, but we may speculate as to the possible role of the individual in the evolution of learned behavior in lower animals. This seems to have received relatively little attention, yet it might be subject to experimental study.

To return to human evolution, I should like to quote some statements from the *Descent of Man* (London: John Murray, Chapt. V, 161). All are from the same paragraph, but have been rearranged to separate two lines of thought:

> Now if some one man in a tribe, more sagacious than the others, invented a new snare or weapon, or other means of attack or defense, the plainest self-interest, without the assistance of much reasoning power, would prompt the other members to imitate him; and all would thus profit . . . . if the new invention were an important one, the tribe would increase in number, spread, and supplant other tribes.

This, I think, seems to be good reasoning, although Darwin may have underestimated the inertia of the traditional mind in matters of common defense as well as in many others. With certain other statements we cannot so readily agree; but I shall quote them for their historical interest, and because they may be significant in regard to ideas that have to a certain extent remained current.

> The habitual practice of each new art must likewise in some slight degree strengthen the intellect. If such men left children to inherit their mental superiority, the chance of the birth of still more ingenious members would be somewhat better, . . . Even if they left no children, the tribe would still include their blood relations. . . .

Clearly there is reference here to the inheritance of acquired characters. We need to remember that when he wrote this Darwin was thinking in terms of "pangenesis," which implied such inheritance. The theory of pangenesis dominated thought for about two decades, and I believe we still have in our social thinking evidence of the belief in a gradual improvement of the genetic character of the population in the way implied in the above quotation. Of course, no mechanism for such improvement has been demonstrated, and modern evidence is to the contrary.

So far as the selection of individuals is concerned, it would have no evolutionary effect unless the individuals selected were genetically superior. History seems to indicate that brilliant and effective individuals have all too often left offspring of much less talent and little historical significance—with meaning we may ask, "Where is the doughty Charlemagne?" On the other hand, certain families have contained a high proportion of effective individuals, although biological inheritance is certainly not the sole factor operating in such cases. Both facts are consistent with what has been said herein, and both should be given their proper weight when we consider human evolution.

IN SUMMARY, then, genetically speaking, the individual as such is of no great evolutionary importance unless he be the bearer of a mutant gene, and even then the importance of the mutation may not manifest itself in the population as a whole for generations. In cultural evolution, on the other hand, the individual would seem to hold a position of great importance. The mechanisms in the two instances are so different that they can hardly be discussed in the same terms. In fact, I think some biologists may object strongly to the application of the term evolution in the second case, and if it should lead to confusion of the two mechanisms I would agree, emphatically. I think, however, that there is much to be gained by treating the cultural aspect as a part of human evolution, but above all things we need to understand and to separate clearly in our minds the underlying processes.

IS MATHEMATICS AN EXACT SCIENCE?

N. A. COURT

Born in Warsaw at a time when it was a part of Russia, Professor Court received the degree of Docteur en sciences physiques et mathématiques *at the University of Ghent in 1911. In that year he came to the United States to teach mathematics and since 1916 has been in the Department of Mathematics at the University of Oklahoma. Dr. Court is the author of* College Geometry *and* Modern Pure Solid Geometry.

THE famous eighteenth-century *Encyclopédie Métodique* gave the following definition of mathematics: *C'est la science qui a pour objet les propriétés de la grandeur en tant qu'elle est calculable ou mesurable.* Precise, concise, definite, and simple. This was in 1787. Even though this definition was adequate for the time, it was not destined to remain so very long. Two decades earlier Gaspar Monge had invented descriptive geometry. He did not publish his results until 1795, because for over a quarter of a century the French High Command considered descriptive geometry its own private military secret. Monge's invention led his pupils to the creation of projective geometry, a branch of mathematics that does not deal with magnitude. The quantitative conception of mathematics thus became obsolete. Many efforts have been made since to find a definition that would embrace all of mathematics. The enormous growth of the science during the past century and a half, and the inclusion of such branches as the theory of groups, topology, and symbolic logic, rendered all such attempts unsatisfactory. The hopeless task was finally given up in favor of simply saying that mathematics is what mathematicians are doing.

How do mathematicians acquit themselves of the heavy responsibility that such a definition puts upon their shoulders? They have the advantage that they start out with a great amount of credit. To the layman mathematics is synonymous with exactness, nay, with certainty. Mathematics is precise, mathematics proves all the assertions it makes, all the propositions it advances. And books written by mathematicians seem to bear out the layman's opinion about the authors. These books seem to be written with complete detachment and strict objectivity. There is not a single exclamation point to be found on any of their pages, except when it is used as a symbol for a factorial. But do mathematicians actually do their work with that Olympic impartiality that the final product seems to exhibit?

Ask any mathematician worth his chalk why he spends so much time and effort on his research, and he will almost invariably tell you—and quite truthfully so—that he does it because he finds it very interesting, because he loves to do it, because to him it is a most exciting adventure. Sentimental reasons all. But is sentiment a reliable partner of objectivity?

Every active mathematician will readily agree that he is trying by his efforts to promote and advance the science of his choice. There is no doubt that he is telling the truth and that he is quite sincere about it. But is it "the whole truth and nothing but the truth"? If it were, if the mathematician were interested in the promotion of his science in a purely objective way, it would make no difference to him whether it was A or B that took a given forward step, as long as the advance had been accomplished. But this is not the case, as is abundantly proved by the historically famous, and disgraceful, controversies over priority rights of mathematical inventions. The Newton-Leibnitz quarrel over the invention of the calculus was just as bitter as it was harmful. It actually hindered the progress of the calculus in Britain for over a century.

The dispute between Poncelet and Gergonne as to who was the rightful owner of the title to the invention of the principle of duality may have yielded in scope to the Newton-Leibnitz controversy, but it was fully as acrimonious, if not worse. One could cite the quarrel between Descartes and Fermat, between Legendre and Gauss, and so on and on, *ad nauseam.* Cardan obtained from Tartaglia the solution of the cubic equation under oath of secrecy and then not only published the solution, but claimed it as his own. Our methods may not be as crude, but we are as jealous of our priority rights now as anybody ever was. Editors seem to think that priority claims are established by the date a given article reaches their desk, and publish this date as a part of the article. Perhaps whether it was A or B that made a contribution may not be of so much moment, but whether it was *I* or not *I* is of tremendous importance. The sublime indifference toward public acclaim exhibited by a Fermat does not seem to be of this planet. It may be argued that mathematicians as a rule get little else for their labors; they are therefore at least entitled to the honor and recognition their accomplishments can bring them. This is true enough. But it is a weak argument in favor of the supposed detachment and objectivity with which mathematicians view their work.

There are cases on record when mathematicians were reluctant to publish the results of their findings, their reticence motivated by their solicitude for their science. When the researches of the Pythagoreans brought them face to face with irrational numbers, they were overwhelmed by their discovery. It contradicted the fundamental tenet of their philosophy that everything is (rational) number. The surest way out was to make of this troublesome result a professional secret and to induce the gods to destroy anyone who would dare to divulge to the lay crowd the exclusive wisdom with which only the initiates could be trusted.

We have a similar example in modern times. Gauss was in possession of non-Euclidian geometry ahead of both Lobachevsky and Bolyai, but he was loath to publish his results. He feared that such an unorthodox discovery might undermine the faith of the young in the validity of mathematics in general.

The judgment of both the Pythagoreans and of Gauss as to the effect of their discoveries upon the development of mathematics was totally wrong. But this is here quite beside the point. What is important to note in this connection is that the concealing of the truth is hardly the proper method to inspire confidence in the exactness of the science one is trying to promote.

For men who are supposedly dealing with an exact science, the number of mistakes mathematicians make is both puzzling and disconcerting. A few years ago the Belgian mathematician Lecat published a collection of *Erreurs des Mathématiciens.* The list of names mentioned looks like nothing less than a "Who's Who in Mathematics." Henri Poincaré was awarded the Nobel prize for a paper that had a serious mistake in it. He detected the error himself while his paper was in the process of being published, but it was too late to remedy the situation, and the King of Sweden formally conferred upon the author the Nobel prize for a paper that was wrong.

In an exact science it should be easy to evaluate the merits of a paper, and experts in the profession should be able to decide which of several solutions of the same problem is the correct one. But this is only too often not the case. Here is one example, of many that could be quoted. In a paper *Fourier's series,* recently published by the Mathematical Association of America, R. E. Langer relates a controversy participated in by d'Alembert, Euler, and Daniel Bernoulli. Each of these luminaries wrote a paper on the problem of vibrating strings. The three-cornered polemic lasted more than a decade. The only point of agreement that emerged clearly was that there was always a two-to-one majority that the third party was wrong. Human, all too human. But where does the exact science come in?

In spite of all these foibles, mathematicians mount a vigilant and jealous guard over the exactness of their science and are not a bit sparing of one another when the impeccability of that science comes into question.

The invention of the calculus provoked a flood of criticism as to the mathematical and logical soundness of the new doctrine. Neither Newton nor Leibnitz was quite convinced that the reproaches were groundless, but they found no way of disposing of them. Leonard Euler (1707–83), their most distinguished immediate continuator, paid still less attention to this controversy. He used his great gifts to expand and enrich the work of his illustrious mentors, and his unerring instinct for what was right kept him firmly on the straight path. However, Lagrange (1736–1813), a younger contemporary of Euler, did not share the faith of the courtier of the czars of Russia in the formalism of mathematics. In Lagrange's estimation Euler's calculus "did not make sense."

The mathematical analysis bequeathed by the eighteenth century appeared to the mathematicians of the early nineteenth century to be a structure totally devoid of any foundation. Under the leadership of A. L. Cauchy (1789–1857) they undertook to provide analysis with underpinnings solid enough to render this branch of mathematics impervious to the most exacting criticism and at the same time to safeguard the results of mathematical analysis from all possible errors.

Thus came into being the school of rigor of the first half of the nineteenth century. It accomplished a great deal, but its achievements were anything but final. The second half of the nineteenth century set new goals for rigor. An attempt was made to "arithmetize" mathematical analysis. Dedekind produced his theory of irrational numbers, Georg Cantor the theory of point sets, and so on. And the quest for rigor is still on the march. What satisfies the most rigid requirements of one generation of mathematicians seems totally inadequate for the next. E. H. Moore (1862–1932), for many years professor of analysis at the University of Chicago, expressed this in the apt adaptation of a biblical phrase: "Sufficient unto the day is the rigor thereof." It would seem, however, that mathematical rigor is a very elusive thing. The harder it is pursued, the more adroitly it evades the pursuer. In spite of all the advances that the nineteenth century contributed toward mathematical rigor, the mathematicians of the present generation feel that they are more "up in the air" than any other generation ever was.

As a textbook Euclid's *Elements* has no rival, not only in mathematics, but in any other subject. More people over more centuries have learned their geometry from that book than have learned any other subject from any other single book, with the exception of the Bible. And yet this is not the greatest of the merits of the book. The great role that this book played in the cultural history of mankind is due to the fact that Euclid's *Elements* was the first model of a deductive science. Euclid begins by defining the entities he is going to consider: point, line, angle, etc. Then he lines up his axioms and his postulates, i.e., those propositions that he accepts as valid on account of their plausibility or "obviousness." All the propositions that follow are derived from those assumed by pure reasoning, according to the strict precepts of logic. For some two thousand years there was nothing that approached Euclid's model in perfection.

It is a queer irony of our intellectual history that it is precisely this perfection of Euclid's geometry that inspired the invention of non-Euclidean geometry. All through the ages students of geometry felt that Euclid's parallel postulate was not sufficiently obvious and should be proved. But the many and various attempts to prove it failed. In the first half of the nineteenth century Lobachevsky and Bolyai, following Euclid's model, each independently constructed a non-Euclidean geometry by assuming that Euclid's parallel postulate is not valid. Each of them pushed his geometry far enough ahead to convince the most skeptical that their systems are quite coherent and not likely to run into inconsistencies. All doubt on this score was finally dispelled when it was shown that the Lobachevskian plane non-Euclidean geometry may be interpreted as Euclidean geometry on a pseudo sphere.

The non-Euclidean geometries rendered Euclid's parallel postulate, if anything, even less obvious. Still Euclid succeeded in constructing his elements in spite of this deficiency. From this there was only one step to the conclusion that the logical coherence of Euclid's *Elements* is in no wise dependent upon the obviousness of its postulates, and that it should be possible to build a consistent geometry with a set of postulates that would lay no claim to obviousness whatever.

The basic entities of Euclid's great work fared no better than his axioms. It all started with the "principle of duality," to which allusion has already been made. This principle asserts that if in any valid proposition of plane projective geometry the words "point" and "line" are interchanged, the resulting proposition is also valid. This astounding discovery inevitably led to a strange conclusion, namely, that the nature of the basic entities to which the basic postulates of a deductive science are applied is quite immaterial. In fact, these entities need not have any meaning of their own. Their relation to each other is determined by the postulates that are applied to them, and that relation is all that matters.

On these foundations was built the "formalist school" of mathematics, of which David Hilbert (1862–1943) was the leading exponent, the high priest of the cult. There was, however, a bothersome fly in the ointment. In fact there were two such flies. If postulates for a mathematical science, for example, geometry, are set down arbitrarily, and if the entities to which they are applied are devoid of meaning, what relation does such a geometry bear to the physical world? Richard Courant (1888–), a former colleague of Hilbert, says in the preface to his *"What is Mathematics?"* (1941, written in collaboration with Herbert Robbins) that such a doctrine "is a serious threat to the very life of science," that "such Mathematics could not attract any intelligent person." The formalists, however, made short shrift of objections of this kind as long as they could feel that their science remained logically without a blemish. On that ground they were undeniably right. But it was not so easy to kill the other fly, for nothing less was involved there than the logical foundation of the formalist science.

The "obviousness" of Euclid's basic propositions had reference to the fact that these propositions are extracted from our daily experience and are realized, somewhat crudely, in the world that surrounds us: they are thus consistent with one another. If the postulates are taken arbitrarily, if they have no intuitive connotation, what guaranty is there that they are logically consistent? Without a proof of the consistency of the postulates the whole edifice is worthless. The formalists realized that no less than their bitterest critics. Hilbert made heroic efforts to find such a proof. He failed. And there the matter rests, except that it has been proved to the satisfaction of those most competent to judge that, within the framework of a given formalist science, it is not possible to find a proof that science is consistent. If a proof of consistency for a formalist science is to be produced, it must come from outside that science. This proposition is due to K. Goedel.

The formalist school of thought in mathematics takes logic for granted. To this logic it adds an arbitrary set of entities—"undefined terms"—and an arbitrary set of postulates—"unproved propositions." It is then in possession of all the necessary tools and materials for the building of the proposed branch of mathematics.

Another school of thought came to the conclusion that the formalists are extravagant: they require too much. Logic alone is perfectly sufficient for the erection of the entire edifice of mathematics. Not the old verbal logic, but logic reduced to a set of symbols, after the manner of algebra. By means of this "symbolic logic," to give it its proper name, all mathematical entities, including the integers themselves, can be obtained by purely logical constructions. This philosophy of mathematics culminated in the three-volume work *Principia Mathematica* (1910–13), by A. N. Whitehead and Bertrand Russell. This was an extremely ambitious undertaking, undoubtedly one of the greatest intellectual enterprises of all time. It was hailed with great enthusiasm in many quarters, especially in England and in the United States. Helping hands came forward to render the great work still greater.

But the *Principia* began to suffer from the same malaise as Cantor's theory of point sets, as the infinite processes put to work to provide a logical foundation for the mathematical continuum. Paradoxes and antinomies came to light that were very embarrassing. Some of the fundamental assumptions of the *Principia* introduced for the express purpose of warding off paradoxes were found to be questionable and finally rejected. It was not long before the *Principia Mathematica* was reduced to the status of one more contender for the honor of being the custodian of the foundations of mathematics, under the name of "logicalism."

Among the critics of the *Principia* were the French intuitionists: Borel, Lebesgue, and others. But the greatest challenge of this work came from members of the Dutch school, called by Abraham A. Fraenkel the "Neointuitionists." This school, under the leadership of L. E. J. Brouwer (1882–), put the *Principia* upside down. Not only did they reject the idea that mathematics can be derived from logic, they denied logic any autonomous existence. Logic, according to the intuitionists, is not a science but a technique derived from science to facilitate the study of the science. Furthermore, Brouwer boldly questions the validity of the basic processes of our generally accepted logic. He rejects the law of the excluded middle, i.e., that a proposition is necessarily either true or not true. It may be neither, for there may be no sufficient information to decide the question.

As an illustration of what is meant by Brouwer's negation of the law of the excluded middle, let us consider the example given by Abraham A. Fraenkel (*Scripta Mathematica*, **13**, Nos. 1–2, 1947). The fractional part of the number π has been computed for many hundreds of places, and many more such places could now be computed with much less labor than before, by means of the new electrical calculators. Is there a place in this long row of numbers where the digit 7 occurs seven times in a row? There is no such place in that part of the fraction that is known at present, and we cannot tell whether it will or will not occur if new digits of that fraction are computed.

Now let us consider the real number R which starts out as 0.333333 and every other digit of this decimal fraction is a 3, except that if the nth digit of the fractional part of π is a 7 followed by six more digits 7, we will take for the nth digit of R the digit 2, if n is odd, and the digit 4, if n is even. The digits of R are thus perfectly defined as far as the digits of the fractional part of π are known. But we cannot tell whether R is equal to 1/3, smaller than 1/3, or greater than 1/3.

Is the famous saying "You cannot fool all the people all the time" true or false? Perhaps it is true. But it is conceivable that a man publicly perpetrated a hoax or a lie that remained undetected during his lifetime and that he took his secret with him into his grave. Then the proposition would, of course, be wrong, but we would have no way of proving it. If the man wrote a confession, sealed it, and ordered his heirs to open it on the one-hundredth anniversary of his death, then we shall find out on that day that our proposition is false. But at present the proposition is neither true nor false. Hitler was quite certain that the proposition is false. Witness his principle of "the big lie."

"Francis Bacon is the author of the so-called Shakespearean plays." Is the proposition true or false?

Things did not become any smoother for any of the contending schools of thought when the Polish logician Lukasiewicz raised the question why logic should be limited to only two alternatives, two values: true and false. He proposed a new logic which admits of three alternatives, a three-valued logic. Now *ce n'est que le premier pas qui coute.* If logic can be three-valued, why can it not be four-valued, indeed, why not *n*-valued? There is no reason, however, to stop there. Why must the values of logic be a finite whole number? We might as well have a logic with a continuous number of values—such proposals have been advanced.

That unshakably solid rock of classical logic simply slipped away from under the mathematical edifice, and the whole structure is now "on the rocks." As mathematicians put it, their science is at present passing through a "crisis." It has been in this state, roughly, since the beginning of the present century. What connection, if any, is there between this crisis, and the social and political turmoil in the throes of which suffering mankind has been laboring during the same period of time? This is not the time nor the place to consider this question, but so far as mathematics is concerned, you need not be overly alarmed. Mathematics is not going to the dogs.

Mathematics has two aspects: On the one hand, it is a description of a segment of the world we live in and it furnishes tools for nonmathematicians to describe other segments of that world. This might be called the "functional" part of mathematics. The other part of mathematics deals with its foundations and may be said to be largely philosophical. Of course the two parts are not unrelated. The study of the foundations of mathematics decides how far the mathematical processes may be carried out before they reach the limits of their validity. Fortunately, whatever these limits may be, there is ample room for mathematical activity long before those limits are reached. As a matter of fact, most of the active mathematicians are little concerned about those foundations. At least they do not allow those problems to interfere with their activities as investigators. More than that, even those mathematicians who take a direct part in the debate regarding the logical validity of their science manage to obtain very valuable results in their own special field of investigation that have little relation to those theoretical discussions.

But what about the crisis itself? It would, of course, be foolhardy for anyone to try to predict at present where the crisis leads to and how it will end. What may be said, however, with perfect safety is that mathematics will emerge from it enriched and invigorated, to continue the work it has been so successfully carrying on up to now. Some of the benefits are already becoming apparent.

The crisis is having a sobering effect upon mathematicians. The ancient Greek mathematicians used to say that their gods "geometrized." In recent times the God of the English astronomer Jeans was a "pure mathematician." These views reflected the feeling, never formulated, that some mathematicians had that, through their science, they were creating absolute truths, that their profession is the custodian of some eternal verities. During the latter part of the nineteenth century there was much talk about the possible inhabitants of Mars. The desire arose to find a way of getting in contact with those hypothetical creatures through light signals. Some mathematicians came forward with the suggestion that the signal should be a gigantic right triangle built somewhere in the Sahara Desert, the assumption being that if there are intelligent inhabitants on Mars, such a signal will convey to them the idea that there are intelligent inhabitants on the earth, since the Pythagorean theorem is a universal truth.

Mathematics, in spite of great achievements, in spite of the prestige it enjoys, is at present engaged in a self-imposed soul searching. But this is not a sign of weakness. On the contrary, it is a sign of strength, it is a promise, even a guaranty, of greater successes and of increased usefulness tomorrow. In the attempt to place his science on a firmer foundation, the mathematician may arrive at a better insight, both for his own benefit and for the benefit of others, into the very nature of logic and of logical thinking in general. But he may also learn that the mathematical enterprise in which he is engaged is not independent of the time and the place in which it is being carried out.

SCIENCE ON THE MARCH

BACTERIAL VIRUSES (BACTERIOPHAGES)

WITHIN the past ten years the knowledge that bacterial viruses (bacteriophages) are similar to animal and plant viruses in size, chemical composition, and the necessity of having an intact host cell for multiplication has stimulated much interest in the bacteria-bacteriophage systems.

One of the main reasons why investigators have recently centered their attention on the bacterial viruses has been the realization that these viruses lend themselves to a study of virus formation. The bacterial viruses can be assayed with greater accuracy than most other viruses, and experiments can be carried out quickly. Various analyses can also be carried out with more facility, as the environmental conditions can be readily controlled. Recent experiments indicate that a study of bacteriophages will lead to great advances in the knowledge of the mechanism of virus interference and virus mutation.

Most of the work in this country has been carried out with a nonmotile strain of *Escherichia coli* and seven strains of bacteriophage that can multiply on this bacteria. These viruses have been numbered from T_1 to T_7. The bacteriophages have been classed into groups in respect to serological behavior, physiological characteristics, morphology, and host range. Morphologically, the viruses may show a tailed structure. Such an example is T_1. This virus, under the electron microscope, shows a head of 50 mμ in diameter and a tail 150 × 15 mμ. T_3, on the other hand, has only a head, 45 mμ in diameter. The average number of virus particles liberated per cell is different for each virus, varying from 120 with T_1 to 300 with T_7. The time before the bacteriophages are liberated from the cell also varies for each virus strain. The virus strains also differ by the type of circular holes, or "plaques," they cause in a continuous layer of bacteria grown on solid medium. These plaques are due to the lysis of the bacterial cell by the virus. Some *E. coli* virus strains form small plaques, some medium plaques, and some large plaques, the plaques varying in diameter from 0.2 mm to 3.0 mm.

The over-all formation of bacterial viruses may be looked upon as occurring in three steps. The first step is the adsorption of the virus to the cell. This reaction stops the multiplication of the cell. The time of adsorption is different with the various strains of virus and may take 2–10 minutes. The physiological condition of the cell influences the time of adsorption, old cells taking a longer time to adsorb the virus than young cells. The type of reaction involved in the fixation of the bacteriophage to the cell is unknown. It had been thought that the process was similar to the type of reaction occurring in the union of antigen and antibody with complementary surface groups on the bacteria and bacteriophage causing the fixation of the virus to the cell. A recent discovery by T. F. Anderson has shown that certain of the virus strains need tryptophan in order to become adsorbed to the host. This finding may mean that an enzymatic reaction is involved in the fixation process. The adsorption of the virus to the bacterial cell is important in another connection: bacterial mutants arising by spontaneous mutation may be resistant to the action of bacteriophage. E. H. Anderson has shown that these resistant bacterial mutants need accessory growth factors, but the relation of these factors to infection by the virus is not clear. These resistant cells do not usually adsorb the viruses, so that an understanding of the process of adsorption might lead to a better understanding of this type of resistance to infection.

The second step is the formation of the virus within the cell (to be described in more detail later).

It should be mentioned here that although a bacterial virus becomes adsorbed to a cell, it may not multiply. T_5, for example, although adsorbed, will not reproduce unless calcium is added to the medium. Calcium is not needed for the multiplication of the host. These experiments may therefore indicate that "invasion" by the virus has to be distinguished from adsorption and that calcium is essential for the former process.

At a particular time, the cell bursts and the bacteriophage particles are liberated. This is the third period. Each virus strain lyses the cell at a time characteristic for this strain. This time at which the cell liberates the virus particles is called the minimum latent period. The yield of bacteriophage particles per cell varies tremendously for a given strain of virus. T_1-infected cells may liberate anywhere from 20 to 1,000 virus particles. According to the work of Delbrück, the lysis of infected cells will occur in 13 minutes irrespective of the number of bacteriophage particles formed within the cell. The infection of a cell by a bacteriophage seems to start a reaction which causes the cell to lyse independently of the number of virus particles formed in that cell.

Although it seems clear that in the *E. coli* systems studied, the bacterial virus is liberated by the lysis of the host, there have been other bacteriophage systems reported where virus particles are secreted by the cell without lysis of the latter. This condition in which bacteriophage particles are produced without lysis of the cell is known as lysogenesis. It is not clear how the virus is liberated in such lysogenic strains.

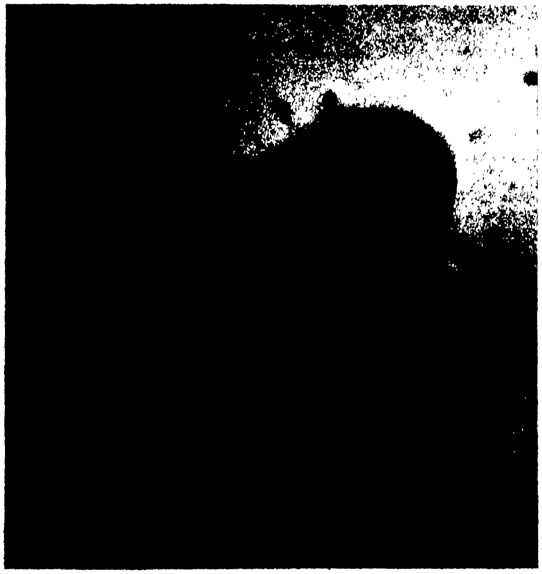
When many bacteria are infected by bacteriophage particles, one obtains what is called a one-step growth curve. This consists, according to the work of Delbrück, of three periods: a multiplication of the virus within the cell; a steady increase, for a time, in virus titer as the cells lyse; and a plateau in the concentration of the virus when all the infected bacteria have been lysed. This type of curve has recently been found by the Henles for the growth of influenza virus in eggs.

Little is known concerning the actual formation of bacterial viruses. This is due mainly to the fact that bacteriophage formation can occur only in the intact cell. Once the virus has entered the cell (and electron microscope studies seem to indicate that the bacteriophage penetrates the cell), there is no known way of studying it until the cell is lysed and the virus particles liberated. All attempts to lyse the cell by grinding and enzymatic methods, before the bacteriophage has lysed the cell, have failed to reveal any virus. This fact is all the more peculiar since recent studies with the electron microscope show the cell filled with virus particles before lysis has occurred.

Another obstacle in the path of understanding virus synthesis is the lack of knowledge concerning the normal synthesis of proteins and nucleic acid. Since all bacteriophages studied so far seem to be composed of at least a protein component and a nucleic acid component, it is essential that the normal synthesis of these substances be understood if one is to understand virus synthesis. Up to the present time, however, we do not know how smaller units of nucleic acid and protein, such as dinucleotides or peptides, are formed, much less a complicated "molecule" like a virus. The foregoing facts, and another great problem in virus formation—the question of self-duplication—show how far we are from unraveling the question of virus formation.

Acknowledging how little is known about virus synthesis, we can say that experiments in the past few years have begun to give us a little insight into the mechanism of virus formation. Reports from this laboratory and the University of California reveal that cells which show no demonstrable multiplication after being inhibited by penicillin will still form bacteriophage; therefore, cell multiplication is not a requirement, per se, of virus formation.

Our experiments and those of Cohen indicate that the virus is synthesized from substances in the medium and not from a precursor in the cell. Furthermore, Cohen's finding that the virus seems to contain predominantly extracellular nitrogen and phosphorus would indicate that the synthesis of the virus probably occurs from small-molecular-weight substances. He also has found that in an *E. coli* organism infected with T_2 (Fig. 1), the ribonucleic acid remains constant and appears to be inert. Protein is synthesized from the beginning of the infection, whereas desoxynucleic acid is synthesized 8–10 minutes after infection. The over-all picture one gathers from these results is that, whereas in the normal cell of *E. coli* synthesis of ribonucleic acid, various proteins, and other substances occur, in an infected cell only virus protein and desoxynucleic acid are synthesized. Moreover, these results indicate that, at least in the infected cell, peptide synthesis occurs before nucleic acid synthesis. Furthermore, ribonucleic acid does not seem to be concerned in protein synthesis as pointed out above. The results



M. Baylor

FIG. 1. T_2 BACTERIOPHAGE
SHOWN INFECTING E. COLI B.

of Brachet and Caspersson seemed to show that nucleic acid was intimately concerned with the formation of protein. Cohen's results indicate, on the other hand, that in an infected cell, the formation of peptides may control the formation of desoxynucleic acid. Further work along these lines would be of great interest.

Our experiments indicate that the synthesis by the cell of the energy-rich donor adenosinetriphosphate is essential for bacteriophage formation. This is of interest in view of the fact that no enzyme systems have been found in bacterial viruses. It is probable that one of the reasons why the bacteriophage needs the cell is to supply the energy for its synthesis.

Experiments in this laboratory disclose that there is a continual competition between the bacterial virus and the host for nutrient materials. Such experiments have been worked out by using high concentrations of bacteria and low concentrations of virus as the inoculum in a synthetic medium. In such systems, the virus will multiply for a time but then stop, although the cells will keep on multiplying. This cessation of virus growth occurs because the bacteria have used up substances in the medium essential for virus formation and these substances cannot be synthesized by the host. Using a *Staphylococcus* bacteriophage, it has been possible to show the need of an unidentified substance in acid hydrolysates of proteins for the synthesis of the bacterial virus.

The study of mutations has led to great advances in the knowledge of bacterial viruses in recent years. Hershey, Delbrück, and Luria have contributed most in this advance. Although such knowledge is likely to change within the next few years, the present investigations are so important as to warrant discussion at this time.

Before discussing the mutation experiments, however, it is necessary to review briefly the effect of multiple infection on bacteriophage formation. It was found by Delbrück that there was no difference in the "burst" size whether a bacteria was infected singly or multiply with the same virus. However, when a bacteria was infected at the same time with unrelated viruses—T_1 and T_2, for example—the bacteria produced only T_2. Even when T_2 was inactivated by ultraviolet light and not able to reproduce itself, it still prevented the reproduction of T_1. These results, whereby one virus excludes the formation of the other virus, have been called the "mutual exclusion effect." A slightly different result was obtained on infecting with T_1 and T_7, which are also unrelated viruses. Here the cells formed either T_1 or T_7. Although in the latter case the infected cell can form only T_1 or T_7, the virus that is not formed is able to depress the yield of bacteriophage from this cell. This is called the "depressor effect."

The mechanism whereby one virus can interfere with the multiplication of another is not known. Delbrück has proposed a "penetration hypothesis." According to this theory, the entrance of a virus into a cell changes the cellular surface in such a manner that the other virus cannot enter. This explanation may hold for the bacteriophage cases studied, but it does not appear to hold for all bacterial viruses. Hershey has recently described a mixed infection involving a wild type strain, T_2, and its *r* mutant. On bursting, the cell liberates both wild type and mutant. Further, mixed infections with related pairs of viruses of T_2, T_4, and T_6 give only partial exclusion. Syverton and Berry have also reported that three different animal viruses—papilloma, myxoma, and B virus—may all infect and multiply in the same cell.

Delbrück has recently published experiments showing that, in a mixed infection, although a virus may not multiply, it may "induce" changes in the virus that does reproduce. Using coli bacteriophages, T_2r+ and T_4r, he found that some cells liberate only T_4r, but that other cells liberate both T_4r and T_4r+. Therefore, while T_2r+ was unable to multiply, it could "induce" T_4r to mutate to T_4r+. In view of the recent finding by Avery and co-workers of an induced change in pneumococci by desoxynucleic acid, it would be of interest to determine whether a nucleic acid from the T_2r+ phage would, when added to a system containing bacteria and T_4r, give rise to T_4r+. This result might be able to account for Hershey's finding that a single cell is capable of liberating both the wild type virus and its *r* mutant. As in Delbrück's experiments, only one virus may actually multiply, but the virus not multiplying may "induce" a mutation in the virus that multiplies. Further work is necessary to explain this phenomenon.

Luria has recently published experiments which may help explain some of the mutations observed in the mixed infections. He showed that two bacteriophages, both inactivated by ultraviolet light, will give rise to an active bacteriophage if made to infect a single bacterium. He has proposed that bacteriophages are made up of 30–50 loci and that each of these loci can be transferred from one bacteriophage particle to another inside the bacterium. Luria suggests that bacteriophage formation occurs by independent reproduction of each of these loci in the bacterium. It is not easy to visualize how such an exchange of loci could occur. As was shown by Schoenheimer some years ago, however, there is a continual changing over of molecules in an organism, with newly formed small molecules re-entering specific portions of large molecules, so that the fact that there appears to be an exchange of "units" between bacterial viruses may not be so unusual a phenomenon as it would at first seem.

WINSTON H. PRICE
The Rockefeller Institute for Medical Research
Princeton, New Jersey

THE PROBLEM OF THE HELICOPTER

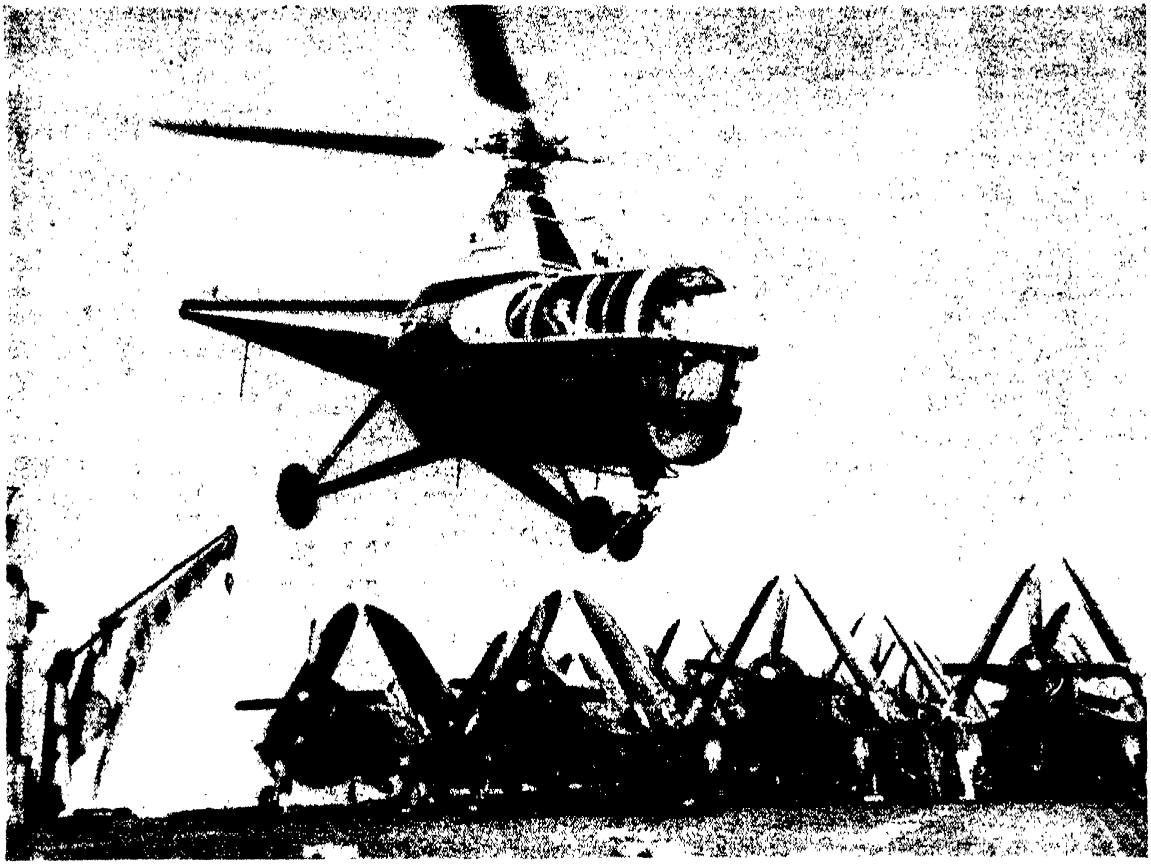
NOTHING could be simpler than the basic principle of the helicopter. A rotor, similar to an airplane propeller, but with a much larger diameter relative to the aircraft, and a smaller pitch or blade inclination, rotates slowly with its axis substantially vertical. The rotor impels the air *downward* and therefore by Newton's law produces a thrust *upward*. The upward thrust gives the necessary lift to the aircraft. If by appropriate means the axis is inclined forward, the rotor thrust provides propulsion as well as lift and the helicopter moves forward. Alternatively, we can say that the helicopter rotor combines the functions of an airplane wing and an airplane propeller, and that seems to be a welcome simplification. Besides, the airplane wing only exercises lift when it is in forward motion by dynamic reaction. The helicopter rotor can produce thrust, or lift, when the aircraft is *not* in motion. Therein is the explanation of the wonderful capacities of the helicopter, which can hover motionless above the ground, climb vertically upwards, descend vertically, and travel in any direction following the inclination of the rotor axis. Thus, the helicopter surpasses the airplane in versatility and does almost as well as a mosquito. The helicopter pays heavily for these added capacities, however, and is inferior in speed and pay load to the airplane. Also, it is a more complex mechanism than the airplane, and its designers have to solve many curious problems.

To be efficient in lift the rotor has to handle large masses of air and impart to them a low downward velocity. Then momentum change and thrust are obtained without too great loss in the kinetic energy of the slip stream. That is why the rotor has to have a large diameter and to revolve slowly. But if the rotor is large and revolves slowly, its torque, or turning moment, is correspondingly large. Somehow this torque has to be counteracted; otherwise fuselage and occupants would revolve dizzily in space. This first problem of counteracting the torque is so important that it dictates the whole configuration of the helicopter. One method is to use a tail rotor that revolves about a horizontal axis and provides a lateral thrust. This is the classical solution which Igor Sikorsky adopted and which is apparent in our photograph of the Sikorsky S-51 helicopter. Unfortunately, the power used up by the tail rotor is pure and continuous loss, the tail rotor is clumsy, and it may be dangerous to careless spectators. Yet the single main rotor-cum-tail rotor configuration is the simplest and the one most popular today. Superimposed coaxial rotors revolving in opposite directions, or rotors placed on either side of the fuselage and also revolving in opposite directions, eliminate the torque but introduce difficulties of their own. However, no one can yet say what will be the final configuration.

Another difficulty of the helicopter lies in the fact that in forward flight the advancing blade of the rotor has a greater velocity relative to the air than the blade retreating from the wind. Accordingly, the advancing blade develops more lift and tends to roll the craft over to one side. Juan de la Cierva, the great pioneer of the Autogiro, solved the difficulty, so it is said, while hearing and paying little attention to an opera. Cierva allowed the blades to hinge freely at their roots, so that they flapped upward and removed the excess lift of the advancing blade. The Autogiro which Cierva invented has disappeared; but the merit of his hinged blade is universally realized, and without the hinged blade or its equivalent helicopter flight would be well-nigh impossible.

It is true that the helicopter can rise and hover, but, people ask, what happens when the power fails? That is a good question. The answer is reassuring. Provided the pilot or an automatic control decreases the pitch of the blades quickly, they continue to rotate. When the rotor is in autorotation, the helicopter becomes an autogiro. It can no longer descend vertically with safety, but it can still make a steep glide and land and come to rest after a very short run, in the smallest of fields.

There is one drawback to the helicopter which remains: lack of inherent stability. The airplane in forward flight can be flown hands off the stick and feet off the rudder bar for several minutes. If the airplane has an automatic, gyroscopic pilot, it can conceivably fly without the pilot's guidance for an hour at a time. In the helicopter no automatic pilot is as yet available, and this type of aircraft has not



SIKORSKY S-51 HELICOPTER

LANDING ON THE AIRCRAFT CARRIER U.S.S. FRANKLIN D. ROOSEVELT DURING ATLANTIC FLEET TEST OPERATIONS.

yet developed inherent stability of its own. As a result, the helicopter pilot has to fly his craft the whole time, and that means fatigue. The problem is important and remains to be conquered.

Accidents due to failure of the rotor are very rare; when they occur they are serious, because the centrifugal force unbalance when a blade lets go is enormous. Failure of the rotor is a matter not of strength but of vibration. In an airplane in steady flight the fixed wing always meets the air in precisely the same way at any instant. In the helicopter rotor the blade meets the air at each point of the cycle in a different way; hence, aerodynamic excitation forces occur. Again, because the blade flaps as it revolves, dynamic forces arise, forces that the engineer terms Coriolis forces. At any rate, there are exciting forces, and vibration follows. The reduction of exciting forces and their isolation from the rest of the aircraft constitute a difficult problem. The physicist will appreciate the situation when he is told that the dynamic equations of motion of the helicopter blade contain variable coefficients and cannot be solved. The vibrations of the rotor also feed back into the controls and produce "stick shake." It is difficult to say which offers the most beautiful field of research for the applied mathematician, helicopter stability or helicopter vibration.

How is control attained? By changing the pitch of the blades at certain points of the cycle, periodically. The mechanism is simple in principle, complex in execution, and involves careful mechanical design. Since the rotor revolves slowly while the internal combustion engine revolves rapidly, the rotor must be geared down about 10 to 1, and the transmission system becomes heavy and complicated. Other mechanical problems lie in the transmission of power to the tail rotor, and in the rapid change of pitch of the rotor for autorotation. The cooling of the engine is also a difficult problem because the engine of the helicopter may be required to deliver its full power while the aircraft is stationary. In brief, it can be said that the mechanical part of the helicopter is much more complex than the mechanical part of the airplane. Moreover, it is impossible to use in the helicopter those standard automotive parts that are available for the motor car. For the same duty, helicopter mechanical units must be much lighter and of more refined construction.

A photograph of the Sikorsky S-51 shows a typical, and perhaps one of the most widely used, helicopters of the day. Modern helicopters are remarkable for the roominess of the cabin and the wide vision that both pilots and passengers enjoy. To be able to take advantage of its hovering and vertical landing capacity, the pilot must have extraordinarily good vision. The photograph indicates the long tail boom, at the end of which the torque rotor is mounted. The landing gear is of the well-known tricycle type with a small nose wheel at the front. A machine such as the S-51, equipped with an engine of approximately 500 h.p., will have a gross weight of 5,100 pounds, top speed at sea level of 103 miles an hour, hovering ceiling of about 5,000 feet, and weight empty of about 3,850 pounds. From these approximate figures it is clear that the helicopter can in no way compare in efficiency, speed, or load-carrying capacity with the airplane. The point is the helicopter should not be so compared. It has spheres of its own in which it reigns supreme.

When the helicopter first became in this country a practical aircraft, thanks to Igor Sikorsky's remarkable development work in 1936–37, there was a burst of enthusiasm as to its possibilities. Predictions were made that helicopters would be the private flying vehicle of the future, and that in a few years private owners would operate hundreds of thousands of helicopters. These optimistic expectations have not been realized, for very good reasons. First of all, although it is very easy to learn how to fly a helicopter, its operation, because of lack of stability, vibration, and some degree of stick shake, is fatiguing. Another reason why we have not had wide adoption of the helicopter for private flying is the very high cost of the helicopter to date. This high cost is due to the fact that few helicopters of any one type have been designed and built, and because of the extra complexities of the mechanism. A two-seater helicopter like that manufactured by Bell Aircraft may have a price of some $25,000; a five-passenger machine with more powerful engines may cost $65,000–$70,000. Such costs are almost prohibitive in private use. Private flying with the use of the helicopter will come certainly when some enterprising manufacturer finds ways of reducing costs to more modest levels.

That does not mean, however, that the helicopter is not already at work. During World War II it found a great many applications in rescue work in the jungles and overseas. The Coast Guard found it an indispensable element of equipment. For military operations of all kinds the helicopter already has remarkable utility. In spite of its cost, it is rendering a variety of services to industry and business, services so useful and so superior to services rendered by other vehicles that cost becomes almost subsidiary. Thus, the helicopter is now being used for crop dusting, for patrol of power lines, for detection and fighting of forest fires, for geophysical and mining exploration, and for many other purposes where its special ability comes into play.

American helicopters lead the world and have

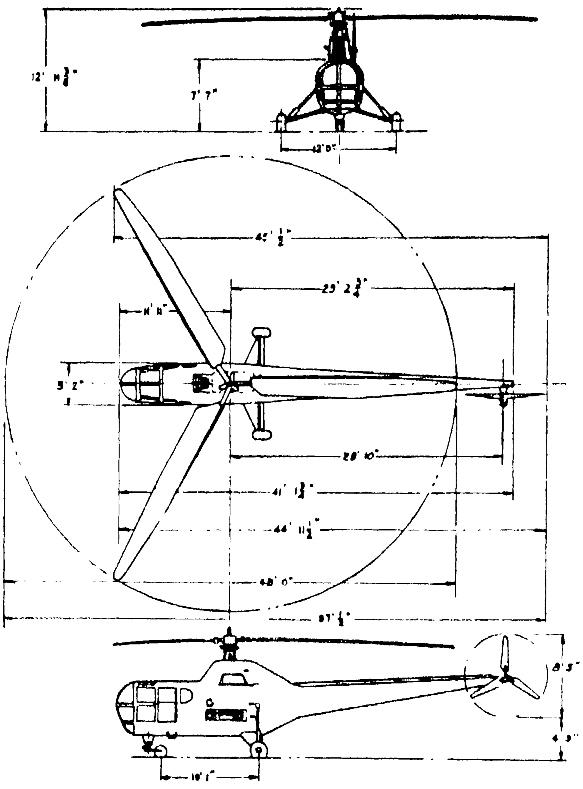
ARMY-SIKORSKY R-5F HELICOPTER

been sold to Great Britain, Sweden, Holland, Argentina, Brazil, and many other countries. Young men have come from the four quarters of the world to learn how to fly the American helicopter. It is also gratifying to think that the helicopter has saved far more lives than it has destroyed.

The Post Office Department, after a series of experiments, has contracted with Los Angeles Airways for a system of carrying mail by helicopter, connecting the center of Los Angeles with many far-flung local post offices. The experiment has proved most satisfactory, and there is no doubt that similar services will be extended to other metropolitan areas such as New York, Chicago, Philadelphia, and probably Boston. Such services would make a tremendous difference in the time of delivery of air mail, and supply the missing link in the air mail service of the United States.

No attempts have yet been made to carry passengers from the airports to the hearts of cities, but there is very little doubt that the ferrying of passengers will soon come. The Piasecki helicopter is already available for the carrying of ten passengers, and estimates of expenditures indicate that ferrying of passengers from the heart of cities to airports is economically feasible. The Civil Aeronautics Board hesitates, on the grounds of safety, to approve the carrying of passengers. The helicopter can land on roof tops or on any odd city lot 100 feet by 100 feet, but there is some hesitancy in putting it to work in thickly populated districts, with skyscrapers inducing unexpected and violent air currents.

In conclusion, it may be said that, although the helicopter is never likely to equal the airplane in long-range transportation, it has already demonstrated its utility in special applications; nor is there doubt that such utility will grow. Also, it seems particularly well suited for private flying. Nothing could be more practical than the helicopter that could land, so to speak, in a man's own back yard. One can be sure that American ingenuity will not be long in conquering the difficulties that now delay the use of the helicopter in private flying.



PRINCIPAL DIMENSIONS OF SIKORSKY S-51

FRONT, TOP, AND SIDE OF HELICOPTER (NAVY DESIGNATION, HO32-AIR FORCE R-5F). OVER-ALL MEASUREMENTS ARE HEIGHT, 12′ 11⅜″; LENGTH (BLADES EXTENDED), 57′ ½″; MAIN ROTOR TIP CIRCLE DIAMETER, 48′; TAIL ROTOR TIP CIRCLE DIAMETER, 8′ 5″; LANDING GEAR TREAD, 12′. FOR EASE IN HANDLING ON SHIPBOARD, MAIN ROTOR BLADES MAY BE FOLDED BACK ALONG TAIL CONE TO REDUCE THE GREATEST WIDTH TO THAT OF THE LANDING GEAR AND THE GREATEST LENGTH TO 44′ 11½″. CABIN WIDTH IS 5′ 2″.

ALEXANDER KLEMIN

Helicopter Editor
Aero Digest

BRITISH INSTITUTIONS FOR RESEARCH IN SOILS AND CROP PRODUCTION*

WHATEVER kind of farming a man undertakes, he is ultimately dependent on the soil and the crops, and so it was natural that British agricultural education and research should begin with these two very important subjects. There had long been spasmodic efforts to study them, but the modern period began rather more than one hundred years ago when three organizations were started that have since played a great part in our rural life: the Royal Agricultural Society, the Royal Agricultural College, and the Rothamsted Experimental Station. They began quietly and in a small way, in a period not unlike the present, when England was greatly impoverished by years of war at sea and on the Continent, when there were great social and political upheavals, and many people thought the country was going to the dogs. But under the troubled surface many movements were developing which later emerged and greatly helped the people of those days, and are proving even more useful now. The Royal Agricultural College did great service in training men; the Royal Agricultural Society started a far-reaching research program that has not yet been exhausted; and the Rothamsted Experimental Station has carried out agricultural experiments from its beginning.

---

* From a speech given over the British Broadcasting Corporation network.

The Station began in typically British fashion. Rothamsted is not a village or a town, but an ancient manor which remained during the first six hundred or seven hundred years of its existence quite unknown to history; and it might have remained unknown but for the fact that the young squire, John Bennet Lawes, coming into his inheritance in 1836 and anxious to increase the output from his land, was asked by a neighbor why ground bones were so good as manure on some soils and so poor on others. Lawes knew a little chemistry: he realized that bones contained a phosphate which was insoluble in water but would become soluble on treatment with sulphuric acid; he thought it should then be effective on his land. He tried the experiment, and it was so. More important still, geologists were at that time discovering large deposits of mineral phosphate for which there was no obvious use. Lawes thought that this would serve instead of bones, and experiment showed that he was right. He took out a patent and set up a factory for making this soluble phosphate on a large scale. Thus he was able to supply farmers with a new fertilizer—superphosphate—now famous throughout the world. That was the beginning of the commercial fertilizer industry. Lawes made a fortune out of it. But he did not stop there; in association with Joseph Henry Gilbert he carried out from 1843 onward a series of scientific experiments on soils and crop production on his farm at Rothamsted that are now known wherever agricultural science is studied.

It was relatively easy in those days for these two very intelligent and industrious men to make scientific discoveries. They were pioneers, and the nuggets were, so to speak, lying on the surface. They had all the sturdy independence of the Victorians. Lawes paid the whole cost of the work and would have scorned any idea of Government help. The work was practically all chemical, for at the outset agricultural science was just a branch of chemistry. In these days, it is of course very much wider and more complex: biochemistry, physics, mathematics, pedology, and some half a dozen branches of biology all now come into agricultural science. The modern Rothamsted research station that has grown up during the past thirty years has well-staffed and well-equipped departments dealing with all these subjects. Nevertheless, the Chemical Department under Dr. Crowther, is still one of the most important. The director is now Dr. Ogg.

Rothamsted remains primarily a research institution, but, like most other British research institutes, it can take in a limited number of students who have already graduated and who therefore have a good knowledge of their science. Men and women come from all over the civilized world to work there, many bringing new ideas—and new ideas are the lifeblood of a research institution. It is a postgraduate school of the London University, and students having a Bachelor's or Master's degree acceptable to the Senate may become candidates for the Ph.D. or D.Sc. degrees without further examination. These degrees rank very high, especially the D.Sc., and their proud possessors are to be found in important posts in many countries.

There is another, smaller Soil Institute at Oxford University under the direction of Mr. Morison. He has specialized in the soils of the Southern Sudan and has collected material of great value for students of tropical soils. Graduates of recognized universities can obtain a degree after two or three years' study, according to their qualifications.

Scotland also has a soil research institute, the Macaulay Institute at Aberdeen. Unlike Rothamsted, it has local duties and affiliations, and is responsible for the Soil Survey for Scotland. A student who wishes to gain experience in that type of work could usefully spend some time there. The Institute possesses equipment for studies in soil mineralogy, an important new branch of soil science.

Wales has an important center of soil studies at Bangor, under Professor G. W. Robinson, who was until recently in charge of the Soil Survey of England and Wales, and who also can receive postgraduate students.

An important group of agricultural research institutes has developed at the Cambridge University School of Agriculture started by T. B. Wood, one of the greatest of the pioneers. With him was associated Sir Rowland Biffen, whose inspiration came from two great figures in English biological science, Marshall Ward and William Bateson. Marshall Ward was the founder of modern plant pathology in Britain, and Bateson of modern genetics. Biffen, a student of both, combined their subjects and bred wheats to resist rust, giving them also other desirable qualities. Thus, Cambridge became a great center for the breeding of wheats for English conditions, and the Cambridge wheats are widely and successfully used. Hunter bred barleys at Cambridge. The work is now carried on under Professor Engledow, and a well-known postgraduate school has developed where prospective plant breeders from overseas can profitably study.

Fodder crops, such as oats, grasses, and clover, however, are not dealt with at Cambridge but at the University College of Wales, at Aberystwyth, where the climatic conditions are more favorable for the breeding and selection of that group of plants. This work was started by Sir George Stapledon and has attracted widespread attention in many parts of the world, particularly since one of the most urgent needs of today is the improvement of pastures and grazing lands. Sir George has now retired, but the work continues under the competent direction of Professor T. Jenkin. Apart from the valuable help they will receive at the Plant Breeding Station, postgraduate students will find much to interest them in other departments of the College and its beautiful surroundings.

A grassland improvement station has also been set up at Drayton, near Stratford-on-Avon. Although it is not equipped for sustained postgraduate studies, it can furnish material for a student centered at a University. Useful work on the production of varieties for northern conditions has been done at the Scottish plant-breeding station at Corstorphine, near Edinburgh. Potato breeding is undertaken there, but it must be admitted that the greatest success with this crop has been attained by the commercial breeders, artists in their line, who have constantly before them the ideal of the perfect potato, and spend long years of effort in trying to attain it.

The other great branch of agricultural science inspired by Cambridge—plant pathology, the child of Marshall Wood—has spread rather widely, but is still largely being developed by Cambridge men. Dr. Kenneth Smith at Cambridge deals especially with virus diseases of plants, and the same subject is being investigated by a vigorous group of workers under F. C. Bawden at Rothamsted—without overlapping the Cambridge work. These diseases are spreading in all parts of the world and are doing tremendous harm, causing great losses to important crops. Much has been learned about the nature and properties of the different viruses and the way they are transmitted—a fascinating story. When you realize that some of these diseases are transmitted by the aphis, the little fly of the same family as the notorious green fly of roses; that the aphis sucks up juice from a diseased plant and so imbibes some of the virus, then carries it to a healthy plant and infects that—when you think what minute quantities are involved, you will marvel at the skill of the experimenters who discovered the details of the process, dissecting the aphis to find out how the transmission is brought about, what changes the virus undergoes during transit, what the virus does when it gets into the plant. It is a good subject for a keen young biologist.

One might not at first think of higher mathematics as having anything to do with agricultural science, and indeed it is only in the past twenty-five years that it has come in. The beginning was made at Rothamsted by R. A. Fisher, who is now at Cambridge, dealing with genetics from this standpoint. The work was ably carried on by F. Yates, and a strong department under him provides ample scope for young mathematicians with a leaning toward biological science.

A young but promising department has been set up at Oxford dealing with the design of agricultural and general biological experiments. It is not everyone that combines the necessary mathematical ability with a sound biological sense, and fewer still have as yet equipped themselves for the posts of statistical advisers to the experiment stations. But the need is there, and the demand must grow.

Overseas students with the necessary scientific training will find no special difficulties in entering a British research station for study, provided always there is room. The stations are not, however, equipped for training, and they provide no systematic courses, though they are nearly all closely associated with universities, where of course teaching is available—but always on the same condition: provided there is room. For our colleges and universities in Britain are fuller than ever: there is a great demand among our young people for more and better education. In all material things life here is austere, though no overseas student need fear that he will go hungry. The intellectual and cultural life of the nation still goes on as before. Science and the arts, music, the drama, painting, still flourish vigorously, and the student will find ample provision for all his intellectual needs.

Sir E. John Russell, F.R.S.

Campsfield Wood, Woodstock, Oxon
England

DESCRIPTION OF ROCK COLORS

MINERALS display a variety of colors. Indeed, minerals and mineral products compose many of the pigments used in paint manufacture. In descriptive mineralogy one of the distinctive properties of each mineral is color; another is streak, the color of the powdered mineral.

Rock colors are less vivid yet diversified. Weathering changes the composition and the color. Books about sedimentary rocks cite color as evidence of the origin and past history of the rock and its constituent minerals. It is controversial whether red beds signify aridity or tropical weathering. "The environmental conditions determining the colors of sediments are not fully understood, but are of great importance."[1]

One may be surprised, therefore, to discover that the color names used are referred to no standard. The specific gravity, hardness, crystal form, index of refraction, and composition of a mineral are all determinable. Not so, the color. The mineral determines the color rather than the color the mineral. "Ruby-red" is the color of a red ruby.

The description of rock colors is in worse state. With notable exceptions, most geologists name rock colors offhand. Their choice is affected by the immediate surroundings and by the weather, is inconsistent from day to day, and is very individual, having little regard for the usage of other geologists. I asked thirty-four petroleum geologists, who describe rock cuttings daily, to specify the colors of 14 different rock specimens. One specimen received 28 different specifications.

About 150 years ago Werner, father of mineralogy, worked out an arrangement of colors and color names, illustrated by a set of mineral specimens. He may be regarded as having founded the scientific standardization of color names for all purposes. "The following eight colors were selected by Werner as fundamental, to facilitate the employment of this character in the description of minerals: *white, gray, black, blue, green, yellow, red,* and *brown.*"[2] Under the guidance of Professor Robert Jameson, another mineralogist, Werner's system was published in book form in 1814.

Robert Ridgway, an ornithologist, published his book on *Color Standards and Color Nomenclature* in 1912. It was referred to as a standard for both rocks and maps by the United States Geological Survey. In the descriptions of rocks it yields such terms as "cinnamon-rufus" and "deep Corinthian."

In 1928 Goldman and Merwin abridged Ridgway's 1,115 colors to a set of 114 designed for use by field geologists. The chart[3] did not attain general use, partly because it employed rather forbidding symbols and gave no color names.

From 1915 on the Textile Color Card Association of the United States issued its "Standard Color Cards." These have been used as standards by some geologists.

The Maerz and Paul *Dictionary of Color,* published in 1930, is an excellent reference work but not a field manual. It illustrates 7,056 different colors and, undertaking by means of these to define all usable color names in the English language, it offers a wide choice. Geologists using it have startled their colleagues by describing the color of a limestone as "French nude."

Since Newton and Lambert, it has been recognized that a particular color is the summation of three effects. One of these, called lightness, or value, is illustrated by the range of the neutral colors from black through the grays to white. Value can be measured directly by physical means as the reflectance.

Another of these effects is hue. Newton arranged all the gradational hues of the spectrum from red through yellow, green, blue, to violet on a circular arc. He completed the circle by means of the purples, which are mixtures of blues and reds.

The third effect is chroma, which is the inverse of grayness. Complementary hues may be placed at opposite poles of a diameter of the hue circle. Complementary hues mixed in the right proportion make a neutral gray, which can also be approached by adding gray to either one of the hues and thus reducing its chroma. The chroma of any hue may be said to range from vivid through bright and moderate and weak (or grayish) down to the very absence of the hue represented by a neutral gray as the limit of the sequence.

Since each color is a function of the three variables, value, hue, and chroma, it is possible to represent the whole array of known colors by a three-dimensional diagram or color solid. For practical geological purposes consider the color solid to be a sphere with a diameter representing the neutral axis from black at one end to white at the other. A radius perpendicular to the middle point of this axis revolves through the whole hue circle. Each radius, then, is a representation of the chroma of a particular hue from gray at the axis to vivid at the surface of the sphere. Given a particular hue and chroma represented by the end point of some radius vector, there is still a range of lightness from darker to lighter represented by a line through the end point parallel to the neutral axis.

The Munsell color notation devised by A. H. Munsell (1858–1918) divides the value into 10 major units from 0 (black) to 10 (white). His 10 hue units are *R, YR, Y, GY, G, BG, B, PB, P,* and *RP,* the letters referring to red, yellow, green, blue, and purple. Each hue unit has 10 subdivisions of equal arc on the hue circle; for example, 1*YR,* 2 *YR,* . . ., 10 *YR.* Chroma in the color sphere ranges from 1 to 10 (and also above 10, but few rock chromas exceed 6).

Munsell chose these units to represent equal psychological steps in color perception. Their real value in scientific measurement is that each color is carefully reproduced by the Munsell Color Company and is physically specified by means of photometric measurements. A Munsell designation such as 5*YR* 6/2 (where 6 is the value and 2 the chroma) is physically fixed and internationally communicable without recourse to native languages.

Soil scientists and others who study drugs, chemicals, and agricultural products have already adopted the Munsell system as a basis for color description. Color experts are determining the Munsell designations of the 7,056 colors in the Maerz and Paul *Dictionary.* When this work is completed it will provide an immediate correlation between standards.

In the past two years a Rock-Color Chart Committee, composed of representatives of leading geological organizations, has determined the range of rock colors by comparing a wide selection of specimens with Munsell standards. Rock colors are concentrated in the *YR*s and the *Y*s. The green, blue, and purple rocks are relatively scarce and very grayish; that is, most of them have chromas less than /2.

Following the lead of the Inter-Society Color Council and the National Bureau of Standards,[4] the Rock-Color Committee has tried to select a system of rock-color names sufficiently standardized to be acceptable to color scientists, sufficiently useful to be satisfactory to field geologists, and sufficiently commonplace to be understood, at least in a general way, by anyone concerned with rocks. A new color chart for field geologists illustrates these color names by means of chips supplied by the Munsell Company. The first copies will soon be available.

Each color chip on the chart is marked by a name and a Munsell designation. A color name, such as "pale red," occupies a portion of space in the color solid, whereas a Munsell designation is represented by a point. Munsell designations are therefore more accurately descriptive than names, and numerical interpolation is easy between colors described by numbers. The Munsell system gives a definitive answer to the old question, What happens to the color when a rock is wet? The hue and chroma remain unchanged, but the value is diminished. In other words, the color is darkened only.

Although the Committee has established the range of rock colors and designed a field chart, the mechanical problems of arranging Munsell standards for microscopic and other laboratory use remain to be solved.

RONALD K. DEFORD

Argo Oil Corporation
Midland, Texas

REFERENCES

1. TWENHOFEL, W. H. *Treatise on Sedimentation.* (2nd ed.) Baltimore: Williams & Wilkins, 1932, 775.
2. DANA, E. D., and FORD, W. E. *A Text-Book of Mineralogy.* (3rd ed.) New York: John Wiley, 1926, 248.
3. GOLDMAN, M. I., and MERWIN, H. E. *Color Chart for the Description of Sedimentary Rocks.* Washington, D. C.: National Research Council, 1928.
4. JUDD, D. B., and KELLY, K. L. Method of Designating Colors. *U. S. Dept. Commerce, Nat. Bur. Stand. Res. Paper 1239.* Part of *Nat. Bur. Stand. J. Res.,* September 1939, **23**, 355–85.

◆

BOOK REVIEWS

UNDERGROUND WEALTH

Mineral Resources in the United States. Bureau of Mines and Geological Survey. viii + 212 pp. Illus. $5.00. Public Affairs Press. Washington.

NEARLY sixty members of the staffs of the Bureau of Mines and the Geological Survey have pooled their knowledge to compile and synthesize the information contained in this volume. Like any compilation, it is not designed to be read at a sitting, but it should be required reading for those to whom it is addressed. According to Secretary Krug, these include "legislators, Government officials, producers and consumers of minerals, and others concerned with problems of mineral supply." Obviously, it is little more than a pious hope that many legislators and government officials will absorb the basic facts which the book contains, even if copies come into their possession, for it takes a different psychological approach than is embodied in a compendium to reach the average congressman or government administrator.

An introductory section sets forth the objectives, methods, and results of the survey, providing an honest appraisal of its limitations. To anyone familiar with the mining industry, the imperfections of knowledge regarding any ore body being developed are well known. There are cases in which engineering and geological skills have been applied to the delineation of the deposits with such painstaking care that reliable estimates of tonnages are available. As the authors make clear, these are the exceptions; hence, the estimates of reserves for any metal or mineral can be no more accurate than the data from the individual deposits which, in sum, comprise the nation's reserve.

A second section discusses trends in the several phases of the mineral industry, including exploration. In the limited space devoted to this subject it is obviously impossible to do justice to its many ramifications, but the private operator is bound to feel that something less than justice has been done to individual and corporate initiative in developing technological processes and devices which have expanded reserves and increased recoveries, or created new uses for old and new mineral products. Even so, it is a useful summary with a cautiously prophetic viewpoint.

Approximately three quarters of the book are devoted to brief but careful surveys of the known and prospective reserves of thirty-nine individual metals, minerals, and fuels which are of critical importance in our industrial economy. Quite literally, the subject is covered from A (*Arsenic*) to Z (*Zinc*), drawing upon the vast fund of private and public information placed at the government's disposal during the war. Such a survey could have been prepared only as a consequence of a national emergency that prompted or forced individuals and companies to divulge information privately acquired and privately held. Its publication is timely, for it provides an inventory of resources at a moment when we need to take stock of our raw materials for the immediate future. The composite picture is more encouraging than our professional pessimists would have us believe, yet, in a list of thirty-nine minerals, this nation falls short of self-sufficiency in twenty-seven, for ten of which we produce less than 10 percent of our requirements.

It is probable that the book will be criticized for its self-imposed limitations, and for the scrupulous preservation of anonymity for the industrial and mining organizations that comprise the mineral industry. Criticism of this kind is gratuitous, however, for this is not a treatise on the mining industry but on our national mineral reserves. The authors have achieved their goal with commendable success, though one may reasonably lament the mediocre proofreading and the poor job of printing from which the volume suffers. Scientists will perform a patriotic duty if they will make a sincere effort to see that the book comes to the attention of the legislators and officials for whom it was written. It is reasonably certain that the producers and consumers of minerals will not miss it.

HOWARD A. MEYERHOFF
Department of Geology and Geography
Smith College

FACTORS IN BIOLOGICAL BALANCE

Birds of Prey of Northeastern North America. Leon August Hausman. xxv + 164 pp. Illus. $3.75. Rutgers Univ. Press. New Brunswick, N. J.

IF FOR no other reason than the fact that it forges another link in the desperately needed chain of favorable publicity for our birds of prey, this book would be useful, and it should go a long way toward breaking down the almost impregnable wall of blind ignorance and prejudice against these truly wonderful and necessary creatures. Dr. Hausman has cleverly woven a strong thread of constructive conservation through every page of this interesting work. He has done this in a very clear-cut manner and has carefully avoided classifying himself as a long-nosed "dickey-birder," an attitude that has alienated many of the very persons that well-meaning conservationists try to win.

Birds of Prey, although conveying the impression that it is a technical work, definitely is not. It deals in generalities and should have appeal to the student not too far advanced in bird study. The competent observer of raptores, however, will be disappointed not to find the exacting data he would expect in a book of this type.

If, as I believe, this book is intended, not for students but for ordinary readers, such readers will find themselves much more familiar with the personalities and the intricate relationship of our birds with the rest of the living world than before.

There is a line illustration covering each species, and these are quite good considering the limitations of simple line technique. Naturally, full-color paintings lend warmth and are of much greater value for purposes of identification.

T. H. CUNNINGHAM

Rockville, Maryland

EVERYBODY'S AGRICULTURE

The Business of Farming. Herrell DeGraff and Ladd Haystead. xviii + 244 pp. Illus. $3.00. Univ. of Oklahoma Press. Norman.

THE title of this book implies that a reader could expect to find most of the problems discussed that affect profits and losses in farming. This book does admirably on production phases. It briefs one almost equally as well on organization and reorganization of the income enterprises on a specific farm or area of land. It does not emphasize nearly so well the problems of credit, value of lands, first costs, hazards in owning property, debt repayment, and loss of property owing, not to inefficient management, but to error at time of purchase.

One finds more than the usual proportion devoted to productive phases of the farm and less to farm management or ranch organization. Soil- and water-management problems hold the prima donna position whether it be on a 10-acre tract, 160-acre standard, or a 3-section wheat farm in the Plains.

The authors should not lose sight, however, of the almost equally important phase—economic disasters caused by purchase of either good or poor land at too high a value at the beginning of one's "business of farming."

In American agriculture nowadays a farmer, to be successful, must produce yields above average for his community. To do this he requires technical information on the variety of seed to plant, kind of fertilizer, method of applying fertilizers, control of plant diseases by new kinds of chemical compounds, and newest labor-saving equipment, as well as the cheapest and the easiest way of preserving and storing the harvested product. Recognizing this fact, the authors have mentioned in a general way some of the new techniques in production such as DDT, soil fumigants, hormone feeding, weed killers, vitamin retention, etc.

A young, nontechnically trained reader or a businessman putting his funds in land for investment might think that current know-how in production is more important than future know-how or the problems due to price changes. All except the young and inexperienced know these techniques will be obsolete a few years from now.

The Index could have been two- or threefold longer in order to give more topical descriptions. The inquiring young businessman without a formal training in agronomy or animal science, for example, could, after consulting the Index, then write for any of the 134 references listed or see his county agent. The county-agent approach is more effective than references for new techniques in production. References are more effective with economic problems. Commercial money or farm-management services are more applicable to these problems than general information from rural economic departments of land-grant colleges.

For some types of readers—perhaps the busy man financially successful in some other business or the nonacademic, white-collar, salaried man—references to technical information will not be of interest. The college graduate, fresh out of college three to five years, itching to leave his job and get into the beef cattle finishing business, dairying, or raising potatoes, celery, or cabbage, will probably obtain and read all the references on his problem.

The second problem, that of organization, is not as well treated by the authors as production efficiency. Naturally, only general terms and policies can be mentioned. Full credit should be given the authors for attempting to discourage anyone trying to get rich quick in agriculture. In several places favorable reference is made to major income being supplemented by minor-enterprise income, even at an accounting loss. In this day of high-cost specialization in production, minor enterprises often take justifiable profit from major enterprises. Today, efficiency goes hand in hand with specialization, not with several enterprises on the same farm. A credit structure to withstand for two or three production periods low income caused by low yields or low prices is the suggested farm organization of some economists. This permits efficiency of production in the major enterprise. The authors might well discuss this in more detail in a revised edition two or three years hence. Labor and equipment management might perhaps be treated as one. They are well analyzed. If any constructive criticism is offered at all on this phase, it is that the approach to these topics is too academic. Labor problems are continually shifting with equipment problems. Obsolescence costs in both machinery and buildings are likely to be many times greater than the reader will expect to experience during the next ten to twenty years if he sets his credit structure as indicated in relation to other costs for the farm.

The treatment of the third and last phase of business in farming—mainly how to keep sales above expenses —is not too explanatory. The topic is treated more or less as if the operator would have a static or fixed level of prices and costs with which to pay interest, taxes, and other costs. Perhaps that is being too critical. On page 176 we find: " . . . . an individual farmer can do little or nothing about price on a bulk market." The reader who is worrying about purchasing at 1947 levels or waiting until later can find little comfort in the book except that if he buys good land, not too high, and applies good management he will average out over twenty to thirty years. There is little to tell him how to tide over that one or two years out of the thirty that may wipe him out entirely. It takes only one such decline in a farmer's lifetime to destroy his twenty-year accumulation of credit.

Price declines and area crop disasters don't disrupt the farm owner for his whole lifetime. It is the mismanagement of credit that permits these two to cause a shift in the farmer's occupation. Some of the most efficient farmers on the best land on the fringes of the corn belt in the early thirties were forced off the land with only the family and the car. Many of them could write a chapter on how to be successful if credit is supervised. Their first item would be not soil management but credit safety. A poor, run-down, badly eroded, once-cultivated farm purchased at less than its potential productive value has made many a mediocre manager appear successful on the books of the farm-management association. In the same associations, the most efficient managers (some ex-county agents), who purchased the best land above its potential productivity value, were not so successful and returned sooner or later to some other occupation.

Good and poor management and good and poor farms make up the four general categories, all equal in opportunity for success if the purchase price is in line with potential productivity value. The amateur reader may get the impression that only good land should be purchased. We often hear the slogan "You can't pay too much for good land." The truth is, it has been done for all types of land.

HOMER J. HENNEY
Colorado Agricultural Experiment Station
Fort Collins

OURSELS AS OTHERS SEE US

The American People: A Study in National Character. Geoffrey Gorer. 246 pp. $3.00. Norton. New York.

MOST books on the American people by visitors from other countries have been written by travelers, journalists, and writers and have been admittedly impressionistic. As such they have generally been refreshing and useful. What distinguishes this book, written by an Englishman, is that it is presented to the reader as an anthropological and psychoanalytical study and thereby lays claim to consideration as a scientific treatise. It appears to this reviewer, however, that most of Gorer's generalizations lack documentation and frequently tell but half the truth. Except for a few references to other societies, the fact that the author is an anthropologist has had no apparent effect on his methodology. Much of the material of the book is based upon discussions with university colleagues and their wives (he lists the names of about fifty such informants), who are hardly representative of a cross section of the American people. As a result, most of the author's observations, at best, apply to the middle class. Furthermore, Gorer's glib explanations of American behavior in terms of psychoanalytic concepts applied en masse (the Freudian analysis was developed as an instrument for the treatment of individuals) leave much to be desired and detract from many of his otherwise keen and stimulating observations.

The study of national character, at best a difficult task and one that lends itself more readily to literary than to scientific treatment, is especially difficult in the case of the American people, because of the heterogeneity of the population and the marked regional, rural-urban, and class differences. Mr. Gorer avoids this complexity by limiting his study to a restricted area of the country and to certain groups that he quite arbitrarily identifies with "true Americanism." Thus, he rules out the Southern states, rural New England, and California and ". . . the more obvious minorities, whether ethnic, religious or social . . ." (p. 15). By eliminating the "obvious minorities" and other important Americans who constitute more than a third of the nation's population, Mr. Gorer is no longer studying "The American People." A more accurate title for the book might well have been "Less Than Two Thirds of the American People." In line with this approach, Mr. Gorer distinguishes between "complete" and "incomplete" Americans and lists the Negroes, Jews, Mexicans, American Indians, Asiatics, and foreign-born as among those ". . . whose Americanism is for one reason or another considered incomplete . . ." (p. 211). The author does not raise the question "By what groups are these minorities considered 'incomplete Americans'?" Mr. Gorer, perhaps inadvertently, has adopted the criteria of "Americanism" of some of our most dubious flag-wavers and has mistakenly identified their propaganda with the thought of most Americans. This is further evidenced in his statement that "Intolerance, racial discrimination, terrorism, are perfectly compatible with Americanism; . ." (p. 196).

The psychoanalytic tone of the book is set in the first chapter, entitled Europe and the Rejected Father. For the author the key to the understanding of the American character is the rejection of the immigrant father by his children, a process which he assumes has been going on since the arrival of the Mayflower. He derives many of the fundamental themes in American life, such as the idea of equality before the law, equal opportunity, and the dislike of authority and consequently of government, to the early rejection of the immigrant father who was the symbol of authority and all that was foreign. In similar fashion he explains the high status of American women and the active social role of the middle-aged women. This type of oversimplified psychological approach is also used in his treatment of complicated political phenomena. Thus, he explains ". . . the pathological hatred felt for the late President Roosevelt and his family by so many of the most respected and respectable Americans . . ." (p. 35) in terms of the rejection of the father and hence of authority. But then he writes that

". . . Roosevelt was loved by the less assimilated and less assured groups . . ." (p. 35); i.e., by Mr. Gorer's "incomplete Americans" who apparently had not yet learned to hate authority despite the fact that according to the author's scheme they should have been actively rejecting their fathers. Mr. Gorer's confusion arises from his failure to realize that many of the Roosevelt-haters did not fear authority as such, but feared that Roosevelt would not use his authority in behalf of their vested interests.

Another example of Gorer's psychological explanation of American behavior is his suggestion that because American babies are bottle-fed by the clock they suffer hunger and frustration which in adulthood causes them to fetishistically admire women's breasts and drives them to drink—milk! (pp. 77–78). For all Gorer's sophistication, he seems to this reviewer to be a babe in the American woods.

OSCAR LEWIS

Department of Sociology
University of Illinois
Urbana

CATALYSIS, ESPECIALLY IN RELATION TO LIFE

Life, Its Nature and Origin. Jerome Alexander. vii + 291 pp. Illus. $5.00. Reinhold. New York.

SCIENTIFIC books range between two extremes. There are some that quickly lead the reader into the very heart of their subject, by skillfully displaying its most important features, in logical sequence. Their success is in being orderly. At the other extreme are books that carry such a burden of detail that they are of profit only to those who are already familiar with the subject. To such readers a book of this class may be especially rich in suggestions, from the very fact that the author industriously collected ten thousand more or less pertinent facts, then could not bear to discard a single one of them in order to clarify his main story.

This book is of the second type. Its title should have been "Catalysis, Especially in Relation to Life." The author's thesis may be summarized by brief quotations, beginning near the middle of the book:

> Life is fundamentally a product of catalytic laws acting in colloidal systems of matter throughout long periods of geologic time [quoted from Leonard Thompson Troland].
>
> The simplest living units of which we have indisputable evidence are the genes.
>
> It is possible that in some cases the gene itself is an aggregate of simpler units. . . . The smaller and simpler the living unit considered the greater the probability that the atomic and molecular units composing it might have come together by mere chance.
>
> The original living catalyst was able to initiate its own duplication.
>
> [To explain the development of complex organisms from simple ones, and the possibility of organic evolution]: Biocatalysts are subject to modification which involves the fixation, at an active catalyst area of a gene or other catalyst unit, of some particle which changes the nature and/or rate of the catalytic change occurring there.

In brief, life is viewed *as the result of,* and as acting *by means of,* self-duplicating but alterable catalyst surfaces.

The final chapter is a philosophic glance at the creature whom, in our more optimistic moments, we hail as evolution's greatest masterpiece. Robert Louis Stevenson sixty years ago described this masterpiece more dramatically:

> What a monstrous specter is this man, the disease of agglutinated dust, lifting alternate feet or lying drugged with slumber; killing, feeding, growing, bringing forth small copies of himself; grown upon with hair like grass, fitted with eyes that move and glitter in his face; a thing to set children screaming; . . .

It seems a pity that the author's views, so often reasonable—or at least thought-provoking—are obscured by needless details, or details presented in the wrong places. How life first appeared in what was then a sterile culture medium, the entire earth; how life spread and developed; how catalysis has shaped its destiny—that would have made a dramatic story! It would have been easy to leave the reader with the impression that God, who is worshiped by astronomers as a great mathematician, must really be a master biochemist, putting catalysis to a multitude of ingenious uses, in every living cell of all the vast creation.

HORACE G. DEMING

Department of Chemistry
University of Hawaii

AFRICAN PRIMITIVES

Tribes of the Liberian Hinterland. George Schwab. xix + 526 pp. Illus. $7.50, paper; $10.00, cloth. Peabody Museum. Cambridge, Mass.

THIS is the report of the Peabody Museum Expedition to Liberia, conducted by Mr. and Mrs. George Schwab in 1928. It covers two trips to Liberia's inner border, during which ninety-two days were spent in the interior: first, from Monrovia on both sides of the St. Paul River; and, second, from Cape Palmas along the Cavally River in the southeast. Nine of Liberia's twenty-three tribes were studied, namely, the Gbunde, Loma, Mano, Ge (Gbe), Gio (Ngere), Tie, Sapa (Sapo), Grebo, and the "Half Grebo." Two maps of tribal distribution are included, one after that of Dr. Struck, and a larger one (1:534,000) prepared by Dr. G. W. Harley, whose contribution in "editing, revising, and supplementing of the data of the Schwab manuscript makes him in effect virtually a collaborator if not a co-author."

The basic similarities between these peoples have made possible a composite topical treatment of their cultures, rather than separate discussions of the individual tribes. Variations are mentioned where these have been noted, and care has been taken to indicate to which tribes specific statements pertain. The peoples of this area are basically agricultural and subsist mainly on rice, supplemented by domestic animals. They also do some hunting, trapping, and fishing. Villages, varying in size from about ten to three thousand individuals, are inhabited by several exogamous, patrilineal lineages, which the author calls "families." Neighboring villages whose lineages claim descent from a common ancestor form the "clan," which was the largest political entity before "paramount chiefs" were established by the Liberian government. Even today, chiefs with authority over an entire tribe are lacking.

One may question the author's terminology at several points, as in using "Palaver House" for council house, "devils," "country devils," and "Bush devils" for officials of the Poro and Sande cults, and "Big Devil" for their principal leader. To be sure, these terms are employed by Africans themselves in speaking the pidgin English current in Liberia. Yet their adoption in place of standard English phrases and their use without quotation marks imply a certain condescension toward African institutions that is out of place in a scientific report.

In its content, this monograph resembles the earlier ethnographies more than the detailed modern studies of African peoples; yet one is impressed by the interesting side lights and details which the author was able to record during his brief field work. The publication of this body of useful information, regarding an area in which the United States has long shown a special interest, fills a large gap in West African ethnography.

The reviewer cannot conclude without quoting the dedication: "To the memory of Harvey S. Firestone, to whom the gratitude of the Liberian people stands as an enduring monument in the Hinterland."

William R. Bascom

Department of Anthropology
Northwestern University

BIRDS OF THE ANTIPODES

Australian Bird Life. Charles Barrett. 239 pp. Illus. $3.50. Oxford Univ. Press. New York.

FIRST printed in 1945, then reprinted in 1947, Barrett's *Australian Bird Life* is designed as a handy volume, pleasant to read, yet scientific and containing an adequate account of Australian birds. Unquestionably interesting and scientific, since it is based on the author's wide and accurate knowledge of Australian birds and upon the published works of other authors, the volume falls considerably short of being adequate except, perhaps, for those already thoroughly familiar with Australian birds. With only *Australian Bird Life* in hand, identification of more than a very few species would be very difficult.

Barrett treats the birds of Australia by broad groups such as jungle-dwellers, water-lovers, the big land birds, birds of the sea, etc. Each chapter is a narrative description of a single group, with emphasis on the most unusual or outstanding species. Scientific names are included for all species mentioned, in addition to the vernacular, and often numerous popular names. Much factual information based on interesting and authoritative observations of habits, nests, songs, displays, etc., is recorded in each section. The volume is well illustrated with half tones and color plates; one of the latter occupies two pages and illustrates the eggs of thirty-two different species.

The most interesting section is entitled Australia's Wonder Birds. In this chapter Barrett vividly describes the appearance of the lyre and the bowerbirds, as well as their habits and their remarkable ability to build dancing mounds and playhalls and to decorate the latter. The "painting" habit of the satin bird is most remarkable.

The distribution of most species is described, and ranges for many are included, but because the volume does not carry a map of Australia this information is not as useful as it could be. *Australian Bird Life* is a companion volume to the author's *Australian Wild Flower Book*.

E. J. Woolfolk

U. S. Forest Service
Washington, D. C.

SOCIAL PATHOLOGY

Psychosocial Medicine. James L. Halliday. 278 pp. $3.50. Norton. New York.

THE author of this book is a prominent British psychiatrist. He has undertaken to study the psychological needs of human groups. If these fundamentally social needs are not satisfied, the result is a "sick society." The medical approach to the study of the sick society is called "psychosocial medicine" by the writer. Basically, the book is an application of the concepts of psychosomatic medicine to the illnesses of communities and social groups.

Part One deals with Medical Logic, stating that illness must be regarded as a reaction to material as well as psychological and environmental factors; thus "mechanismic" (mechanistic) and "biological" viewpoints are complementary in considering the cause and prevention of disease.

Part Two describes the concept and the incidence of psychosomatic affections. The author defines them as "bodily disorders whose nature can be appreciated only when emotional disturbances in addition to physical ones are investigated." At this point the psychosomatic aspects of various chronic diseases and their connection with personality types are aptly delineated.

Part Three treats the problem of the sick society, describing the environmental changes in the world of the child and the adult during the past hundred years with their devasting influence on the emotional security of modern man. As a result we find a declining birth rate, decreasing psychological health, and a deterioration of moral and social values, all signs of a threatening disintegration of our Western civilization. In order to survive we shall have to rediscover in our lives a sense of social purpose and spiritual values—notwithstanding the fact that we have made grandiose technical advancements and have improved our physical health conditions to an undreamed-of degree. The social health of Britain and the United States is the subject of special chapters.

Altogether, we are indebted to the author's farsightedness in dealing with some of the most pressing problems of our time.

PAUL WENGER

Bedford, Massachusetts

SCIENCE IS AMAZING

A Treasury of Science Fiction. Groff Conklin, Ed. ix + 577 pp. $3.00. Crown Pub. New York.

RECENTLY there has been a move to broaden the audience for that type of fantastic story called science fiction by its followers by republishing in book form examples of the genre that first appeared in pulp magazines. The present book is the newest addition to this field. A good idea of the drive for respectability in this field of writing may be had by recalling the evolution of the display title for *Astounding Science-Fiction,* the magazine from which substantially all the recent stories used for reprint have been derived. (In the present case all but four out of thirty stories are from this source.) Back in the thirties this magazine was known as *Astounding Stories.* Later it changed its name to *Astounding Science-Fiction,* giving equal prominence to both parts of the name. At present it retains the latter name, but the *Astounding* has been sharply reduced in size on the cover, and the full emphasis is placed on *Science-Fiction.*

In some circles, this type of fiction has been considered as a possible competitor for the detective story as light reading. Though this seems hardly probable in the near future, it does provide a fairly accurate idea of the quality of these stories. The subjects are, indeed, often much more interesting, but the style and presentation is usually inferior to that of good detective stories. It does not appear, moreover, that the emergence of these stories from the pulps promises any immediate improvement in quality. The present collection, for example, contains a story written for the *Saturday Evening Post* by Robert Heinlein that is decidedly below the standard of the stories he has written for *Astounding Science-Fiction.*

What, then, can be said of the present book? For the fan it will probably supply a number of his favorite stories in a convenient permanent form. For those new to the field it is perhaps not quite so likely to make converts as the previous anthologies, since the vein being worked is not a broad one and much of the richest ore has already been removed. It will, however, give the new reader a pretty fair picture of both the strengths and the weaknesses of this type of hus, in "No Woman Born," by C. L. Moore, he will find a very excellent concept, and he will also find how it can be diluted by verbosity and repetition. "With Folded Hands," by Jack Williamson, on the other hand, is much more satisfactory in its presentation of a serious idea in fictional form. In it, as in most of the other successful stories, the emphasis on science is small (this despite the title evolution mentioned above), little scientific explanation is attempted, and the fanciful inventions and far-off times and places are used primarily to widen the scope of human situations which the author can examine.

R. M. SCHRAMM

California Research Corporation
Richmond, California

HEALTH FOR THE MASSES

You and Your Doctor. Benjamin F. Miller. x + 183 pp. $2.75. Whittlesey House. New York.

THE author, a clinical professor of medicine with experience in medical research and the public-health field, has given us an unusually readable book on a subject of vital concern to all. Attention is focused on the current major questions of medical care; for example: the general practitioner, "a casualty of modern science;" overspecialization; group medical practice; preventive medicine; emotional illness; medical check-ups; medical education and research; prepayment of medical care; and administration of national medical care. The *status quo* in medical care is repeatedly and forthrightly challenged, although generally in a temperate vein and with stimulating discussions of the changes deemed imperatively necessary. As regards the argument that today American health is excellent by comparison with previous years or other countries, the author retorts that "We should use only one yardstick to measure health. It must be based on the scale of what can be achieved at the present time" (p. 175), and he demonstrates that very much more indeed could be attained even now.

There is some confusion in the book about the relationship between group practice and prepayment, apparently an assumption of identity between the two (pp. 45, 123, 179). Actually, most groups are fee-for-service (about 80 percent of groups with five or more physicians, as of 1946). It is well to remember also that medical groups, prepayment or otherwise, merely approximate the ideal dealt with in the book—some more closely, others not so closely. Be this as it may, there can be no doubt that, in this simply told yet informative and provocative book, the author has made a notable attempt to acquaint the layman with the point of view of one important school of medical thought on ways and means of attaining optimum national health.

M. S. GOLDSTEIN

U. S. Public Health Service
Washington, D. C.

AAAS SUMMARIZED PROCEEDINGS AND DIRECTORY

It is a pleasure to report that the *Summarized Proceedings and Directory of Members* of the Association is now nearing completion and is scheduled to be ready for distribution in August. The long delay in its appearance has been due entirely to the difficulties of getting such books printed.

The forthcoming volume is more than a report on a century of activities of the Association. It includes corresponding information about the Pacific and Southwestern Divisions of the Association. Still further, it contains important statistical data about each of its 203 affiliated and associated societies: names and addresses of their secretaries, dates of organization, objects, membership dues, total memberships, divisions and branches, meetings, journals and subscription prices, prizes and medals, and libraries. Together these data present a summary of the present status of most of American organized science.

As a background for the activities of the Association and its affiliated and associated societies from the respective dates of their organization to the present time, the dates of the founding of the principal academies of science and universities throughout the world are recorded from the year A.D. 983 to about 1600. In order to illustrate how far the organization of scientific societies has advanced in the past hundred years, a review is presented of the attempts to establish them in this country in the decades immediately preceding the founding of the Association a hundred years ago.

Scientific progress has been so rapid and revolutionary during the life of the Association that it is difficult now to understand and appreciate the great adventures and achievements of our predecessors a few decades ago. To assist in the orientation of readers, the historical sketch of the Association is divided into five 20-year periods, and a few of the greatest scientific discoveries in each of them are enumerated. In these reminders of the great achievements of the past such names as Lyell, Darwin, Kirchhoff, Mendel, Mendelyeev, Hertz, Maxwell, Planck, DeVries, and Bateson appear.

The Directory of Members portion of the book (about 1,500 pages) contains approximately 40,000 names, alphabetically arranged. The name of each member is followed by the year of his birth, his address, the university from which he received his highest degree, his field of specialization or chief scientific interest, his professional position, the year he became a member of the Association, the year he became a Fellow, and the section or sections of the Association with which he is affiliated.

The volume closes with an unusual and valuable section, a Geographical Index of the names and sectional affiliations of all members of the Association at December 31, 1947. That is, the names of all members who are residents of the United States are grouped, first by states in alphabetical order, then by cities and towns within the respective states (also in alphabetical order), and, finally, the names of members in each city or town are arranged in alphabetical order. The names of members who are residents of foreign countries are similarly arranged alphabetically in sequence by continents, countries, cities, and individuals. The Association has members in 76 foreign countries.

It frequently happens that a librarian or scientist wishes to get the names of chemists, zoologists, or specialists in some other field of science who are residents of a particular city, such, for example, as Urbana, Illinois. With the new Directory before him he will turn to Illinois in the Geographical Index, and then to Urbana. Probably to his surprise, he will find that 77 members of the Association are residents of Urbana. To the right of each name is a letter indicating the sectional affiliation of the member—*C* for chemistry and *F* for zoology. By counting the *C*s and *F*s he will learn that, of the 77 members of the Association who are residents of Urbana, 33 are chemists and 29 are zoologists. If he should desire information about any chemist or zoologist in the list he would then turn back to the General Directory.

F. R. Moulton

SYMPOSIUM ON NATURAL RESOURCES

The Symposium on Natural Resources will deal, in a broad and diversified manner, with man's relationship to the useful materials in his environment. The aim of the contributions will be to clarify thinking by the scientific and general public on the principles involved in a wise utilization of such resources. At the present time the difference between the renewable, or cyclical, and the nonrenewable, or noncyclical, resources of the earth is inadequately appreciated. The basis of any sound permanent economy must lie in ensuring that the renewable resources are really used in ways that permit full cyclic restoration, and that, in so far as nonrenewable resources are vital to such an economy, adequate provisions for continual exploitation of progressively poorer sources, and of substitute materials, be made continually in advance of exhaustion. Such substitutions involve social and economic as well as purely technological problems. The whole material basis of human culture is involved in problems of this kind. Such problems can be discussed only in terms of a wide range of disciplines, ranging from geochemistry and plant physiology to economics and social anthropology. Moreover, any practical consideration of the question as to whether we are making the best use of our resources inevitably involves problems of value in contemporary cultures. A particularly urgent case is involved in the extreme strain placed by modern warfare on the riches of the earth.

The contributors to the symposium will be T. S. Lovering, of the U. S. Geological Survey; Stanley A. Cain, of the Cranbrook Institute of Science; and G. E. Hutchinson, of the Osborn Zoological Laboratory, Yale University. Dr. Lovering has had extensive experience as an investigator and a teacher and has given much attention to the general aspects of mineral resources. He is a graduate of the Minnesota School of Mines and has a doctorate from the University of Minnesota. He is the author of *Minerals in World Affairs*. Dr. Cain is one of the ablest of the younger botanists of the United States. He is a graduate of Butler and has his doctorate from the University of Chicago. Before being called to the Cranbrook Institute he taught at Butler, Indiana, and the University of Tennessee. He has been particularly interested in the broader aspects of plant ecology and their relationship to evolutionary and general biological problems. Professor Hutchinson, who has given attention to these problems in connection with his work as consultant in biogeochemistry at the American Museum of Natural History, will discuss "Man in the Biosphere."

SYMPOSIUM ON SOURCES OF ENERGY

THE material progress of civilization depends largely on the energy that man can make available to do his physical work. The energy of the sun over past ages has been abundantly stored and conveniently packaged for our use, but we are using up these resources at an alarming rate. We know that the sun's energy comes from reactions of atomic nuclei and we have lately learned how to produce atomic energy under controlled conditions. We are not sure that mankind can be trusted with this newly discovered energy, but we must carry our research proward vigorously to determine how our resources of uranium and thorium can best be utilized for peaceful purposes. Finally, we have the energy of the sun with us always, but we are now utilizing only a small fraction of it. Sunlight falling on the earth's surface does not give temperatures high enough to be attractive for the direct production of useful power. Our most common method of utilizing this energy is by means of a photochemical reaction in which carbohydrates are produced that can then be burnt with the oxygen of the air to release this stored energy at higher temperatures.

What are the facts regarding a greater utilization of the sun's energy, and what are the chances that an increasing world population in its insatiable desire for more energy can, in the future, obtain still more from the sun?

Most of the energy running man's machines comes from oil or coal or water power. In "Energy from Fossil Fuels," M. King Hubbert, associate director of Exploration and Production Research, Shell Oil Company, Houston, Texas, will discuss these three sources; he will review briefly the evolution of fossil fuels and show that many geological ages are necessary for nature to accumulate the fuel that man uses up in a few generations. He will emphasize the fact that the exploitation of fossil energy is irreversible and that the historical events associated with it are without precedent and incapable of repetition.

Mr. Hubbert will give a summary of the approximate magnitudes and distributions of the various classes of fossil fuels over the surface of the earth. He will try to project the time sequences into the future, and will show that the rates of consumption of these fuels after passing a maximum, or several maxima, must ultimately decline asymptotically to zero.

Speaking on "Atomic Energy," Eugene P. Wigner, of the Physics Department, Princeton University, will discuss the possibilities for the utilization of the uranium and thorium resources found in the earth. Professor Wigner played an important part in the successful design of the nuclear chain-reacting piles at the University of Chicago, and in 1946–47 he was research director of the Clinton Laboratory.

Farrington Daniels, of the Physics Department, University of Wisconsin, will speak on "Solar Energy." He will emphasize the very efficient way in which nature makes use of the enormous amounts of energy that fall annually on every acre of the earth's surface, for the production of carbohydrates and other organic material in the growth of plants. He will summarize recent developments and new laboratory experiments designed to understand better the mechanism by which carbon dioxide and water are brought together by means of sunlight working through the agency of chlorophyll; he will also summarize what is now known concerning the possible efficiency in the number of tons of carbohydrates that can be produced per acre per year with different kinds of plants. He will discuss some of the speculations that have been directed toward a greater utilization of sunlight for the production of fuel and food, particularly in areas where the soil and water conditions are not conducive to agriculture. Finally, he will outline briefly other proposals that have been made for utilizing sunlight to supply the energy requirements of future civilizations after the fossil fuels have been exhausted. Professor Daniels was director of the Metallurgical Laboratory at the University of Chicago during 1945–46.

SYMPOSIUM ON WAVES AND RHYTHMS

One of fifteen symposia planned for the Centennial Celebration in Washington of the American Association for the Advancement of Science will deal with the subject of waves and rhythms. If nature ever abhorred a vacuum, she seems to love waves and rhythms. Rhythmic events are characteristic of many phenomena, ranging from social, political, and economic cycles through a variety of biological rhythms to radiation phenomena in chemistry and physics.

Three papers will be presented on September 14 dealing with selected aspects of this subject. James B. Macelwane, S.J., will consider rhythmic events in the earth's crust from the point of view of a seismologist. Dr. Macelwane is professor of geophysics, dean of the graduate school, and dean of the Institute of Geophysical Technology at St. Louis University. He is a member of the National Academy of Sciences and author of several books and many papers on seismology and related subjects.

Hudson Hoagland will consider aspects of biological rhythms with special reference to determinants of rhythmic activity in the nervous system. Dr. Hoagland is executive director of the Worcester Foundation for Experimental Biology, neurophysiologist at the Worcester State Hospital, and research professor of physiology at Tufts Medical School. He is the author of numerous publications dealing with aspects of neurophysiology and the physiology of time.

A third aspect of waves and rhythms will be discussed by Vern O. Knudsen, a student of the physics of sound and of supersonics, who has made outstanding contributions to the solution of problems in audition. Dr. Knudsen is professor of physics and dean of the graduate division of the University of California at Los Angeles. He has served as president of the Acoustical Society of America and in 1934 was awarded the thousand-dollar prize of the AAAS. In addition to many scientific papers, he is the author of *Architectural Acoustics* and *Audiometry*.

SYMPOSIUM ON PROBLEMS OF THE OCEAN

Among the dozen or more symposia to be held during the Centennial Celebration of the AAAS at Washington, September 13–17, is one on Problems of the Ocean. Oceanography, which includes the study of the physical, chemical, and biological conditions of the ocean and which overlaps allied fields such as meteorology, geology, etc., has made rapid progress in the past decade. These advances range from those that have immediate and important practical application to those that aim primarily at adding to the sum total of human knowledge. There is need, however, for a much better understanding of the phenomena of the oceans. This is emphasized when, in the present state of the world, these great bodies of water must be considered as connecting links rather than separating barriers, and when it is also clear that the rapid rate of increase in human populations requires an expanding dependency on marine resources, i.e., the resources of perhaps the only major area not yet fully exploited.

Early oceanographic expeditions obtained empirical information on currents, temperature, waves, and other phenomena. These followed notable advances in devising physical and chemical theories to explain the distribution of properties found in the ocean, but at the present time these fall far short of actually accounting for existing conditions. Theory and experiment will undoubtedly provide explanations and methods for predicting time changes and for extrapolating knowledge into the depths and other areas thus far essentially unexplored. This goal is still far in the future, however. Because of the complicated nature of the interrelationships that exist in the ocean, it is at present necessary in many cases to turn to empirical correlations to meet immediate demands.

Although fundamentally unchanging, the oceans are never at rest, nor is the distribution of properties ever twice exactly the same. Within limited areas and over short intervals of time, conditions change to a degree that exerts great influences on the organisms living in the sea, on the atmosphere, and on the coasts and sea bottom. Because of these facts the close interrelationship between the various phases of oceanographic research, and their mutual dependence on related fields of endeavor, have long been recognized.

The themes of the three speakers at the symposium on Problems of the Ocean represent the major bases on which the science of oceanography rests. Carl Eckart, director of the Scripps Institution of Oceanography at La Jolla, California, will discuss the theory of stirring and mixing; it is ordinarilly thought that stirring and mixing can occur only in turbulent motion, but closer examination reveals that relatively simple motions also result in stirring. Richard H. Fleming, Chief, Division of Oceanography, Hydrographic Office, U.S. Navy, will describe the requirements for surveys and the various applications of oceanographic information. Daniel Merriman, director, Bingham Oceanographic Laboratory, Yale University, will talk on the biological problems of the ocean. The symposium will conclude with comments from the floor by several principal discussants and members of the audience.

SYMPOSIUM ON GENES AND CYTOPLASM

ANOTHER of the fifteen symposia planned for the Centennial Celebration of the American Association for the Advancement of Science is to deal with genes and cytoplasm. The fundamental importance of the gene in all kinds of creatures from microorganisms to man is widely recognized; but *how* genes accomplish their great effects, control of the biochemical, physiological, structural, and behavioral properties of the organism, has only within the past decade been subjected to concentrated exploration on a scale and in a manner that may lead to a general solution. This broad question involves two distinct problems that have been attacked by different experimental methods. On the one hand is the problem of the nature of the primary activity of genes, their first, and possibly their only, direct activity. On the other hand is the problem of how these basic gene activities can result in the development of an organism with its cells and tissues alike in genes but different in hormone and enzyme production, in structure, and in behavior. The problem of abnormalities in development, such as cancer, is at least in part a special case of the more general problem of developmental differentiation. Not only are the diversities among cells of one body cytoplasmic, but the products of gene activity operate *in* the cytoplasm, and there appear to be, moreover, cytoplasmic materials (plasmagenes) comparable to nuclear genes. The cytoplasm must therefore be investigated along with the genes in attempts to discover what genes do and how cells with the same genes become diverse. These two major problems are dealt with from biochemical, genetic, and embryological points of view by the three papers to be presented in the Symposium on Genes and Cytoplasm.

One of the most fruitful trends in modern work has been to shift attention from morphological traits, which are remote from the primary action of genes, to biochemical properties of the organism, which must underlie the morphological traits and be more directly related to primary gene activity. Among biochemical traits, two classes have held out the greatest promise of being close to primary gene activity: enzymes and antigens. Indeed, it has even been suggested that the primary products of gene activity may be enzymes, antigens, or their specific reactive molecular groupings. In this field, no modern investigation has been more successful or attracted more attention than the biochemical genetics of the bread mold, Neurospora, a program initiated by the geneticist Beadle in collaboration with a group of biochemists led by Tatum. One of the initial members of this group, the biochemist David Bonner, of Yale University, will present a paper on the status of genic control of biochemical reactions and the possibility of arriving by such studies at a knowledge of the gene and its primary activity.

Whatever conclusions are reached as to the pri-[illegible]n of a gene, they will have to be reconciled with many facts discovered by the methods of genetics. Among these are the facts on which are based the concepts of position effect, interaction of alleles, and genic balance. The activity of a gene may be modified either in dependence upon its position in the chromosome, or by the kind of allele present in the same nucleus, or by the ratio between the number of genes of one kind and the number of genes of other kinds present in the nucleus. These phenomena, puzzling in themselves quite aside from the difficulty of reconciling them with the rapidly growing knowledge of biochemical genetics, form the subject of a paper to be presented by Curt Stern, of the University of California at Berkeley. Professor Stern has discovered new facts in these fields and interpreted them by theoretical concepts to account for these difficult matters. These concepts lend themselves readily to a reconciliation between them and the new biochemical genetics.

The biochemical approach represented by Bonner and the genetic approach of Stern go far toward providing us with insight into the primary activity of genes. There remains, however, the essentially embryological problem of accounting for the origin of differences among cells with the same genes. Here a decisive role of cytoplasmic factors has long been suspected. In agreement with this, German plant geneticists have for over 20 years been accumulating evidence for the existence in the cytoplasm of genelike determiners. Similar agents have more recently been discovered in a few animals by French, American, and English workers. Curiously enough, though the problem is essentially an embryological one, the main clues to its solution have come from studies on unicellular organisms, in which all the cells produced by one ancestor are usually alike in traits as well as in genes; yeast, bacteria, and the protozoan *Paramecium*. It is no accident that the traits investigated in these organisms are mainly enzymes and antigens, the two classes of substances held to be most closely related to gene activity. An account of this work will be presented by T. M. Sonneborn, of Indiana University. The work to be reviewed by Professor Sonneborn leads to the view that the products of gene activity are themselves endowed with the genic property of self-duplication and that competition among these plasmagenes gives rise to persistently diverse cell types.

With this full turn of the wheel, there emerges the beginning of a synthesis. Each gene performs a definite biochemical activity, probably conferring specificity upon the more complex materials of the cell, such as enzymes (Bonner); in this activity, the genes vary in their affinity for the substrates on which they operate, and then vary in their efficiency in operating upon them (Stern); the enzymes and antigens specified by gene activity may themselves be genelike (plasmagenes), and the diversities arising among cells with the same genes may be due to the different results of competition among them under different conditions (Sonneborn).

THE
SCIENTIFIC MONTHLY

SEPTEMBER 1948

THIRTY YEARS OF MASS SPECTROSCOPY

ARTHUR J. DEMPSTER

Dr. Dempster, Argonne National Laboratory, Chicago, received the Annual Thousand Dollar AAAS Prize in 1929.

IN THIS anniversary year of the AAAS it is fitting that we should review the development of mass spectroscopy. It is now nearly three decades since many of the ordinary chemical elements were found to be made up of isotopes, that is, of atomic forms that are chemically identical, but differ in mass and other nuclear properties. At the close of the first world war the theory that the atom had a central nucleus was well established. It made the charge on the nucleus the basis for the chemical properties of an element, and thus left open the possibility of several nuclei of different mass having the same charge. The naturally radioactive substances had just been satisfactorily arranged as groups of isotopes. Thus the discovery of isotopes in the ordinary elements fitted readily into the newly developed nuclear theory of atomic structure.

Mass spectroscopy is the study of these isotopes and their combinations and is based primarily on the identification of the mass of a charged atom or molecule by means of its deflection in a magnetic field. The subject grew out of the earlier study of "positive," or "canal," rays. These rays appear when an electrical discharge passes through a gas at a low pressure, and were found to consist of positive ions which were attracted to the negative electrode. If there is an opening in the electrode, the rays pass through with a high velocity and extend as a luminous bundle into the space behind. These positive, or canal, rays affect a photographic plate, cause fluorescence on a screen, or charge up an electrode in their path. Such a beam of positive atoms is deflected when it passes through a magnetic field; the amount of the deflection varies according to the mass, or atomic weight, of the moving particle, and thus serves to analyze the atoms present in the beam. Several methods of using this principle of magnetic analysis will be outlined in this article.

Mass spectroscopy is primarily the scientific study of isotopes and the ions made from them, but there has been an ever-widening application of its results and methods to chemistry, biology, and industry. The scientific side includes three basic problems: what stable and radioactive isotopes exist in each element; what is the relative abundance of the stable isotopes; and what are the exact masses of the isotopes.

Naturally occurring isotopes. In a series of investigations between 1907 and 1913, Sir J. J. Thomson had succeeded in distinguishing atoms and molecules differing in mass by 1 part in 15, and in showing that neon was probably made up of two kinds of atoms with different masses. Improved methods of analysis were devised in 1918–20 that were characterized by the use of focusing—the naturally divergent beam of charged atoms was made to recombine after the magnetic analysis. The advantage thus gained was analogous to that obtained by using a lens—a great increase in intensity and definition in the images—with the result that the isotope masses of even the heavy elements could be separated and measured. In the succeeding decades these improved methods were extended to all the chemical elements and showed that in most cases they had a complex isotopic structure.

The exploration of the isotopic constitution of the naturally occurring elements was the main activity in this field during the first half of our thirty-year period. By 1933, 183 isotopes had been identified in 66 elements. In the second fifteen years this number was increased to 283 in the 83 naturally occurring elements. Of these, no less than 202 in 71 elements were first found by F. W. Aston. The experimental arrangement used by Aston illustrates the method used in this analysis and is shown in Figure 1. The source of ions was the gas discharge at the left in which positively charged atoms and molecules were formed and accelerated toward the cathode, thus acquiring various energies—corresponding to 20,000–50,000 volts. A narrow ribbon of moving particles was deflected by an electric field, the slow ions being deflected more than the fast. In the magnetic field which followed, light masses M_1 were more deflected and struck the near end of the photographic plate, whereas the heavy masses M_2 gave an image at the far end. The particle paths shown resulted in the fast and slow rays of each kind being focused on the photographic plate. An example of such a photographic mass spectrum showing seven isotopes is reproduced in Figure 2.

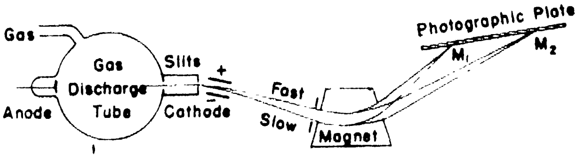


Fig. 1. Focusing of fast and slow ions from gas discharge (Aston, 1920).

Fig. 2. Mass spectrum of normal samarium, showing seven isotopes.

Another focusing arrangement, illustrated in Figure 3, was used by Dempster in 1918 to analyze ions liberated from heated salts, and in 1921 to analyze ions of gases and vapors produced by electron impact. This focusing arrangement made use of the fact that semicircles of the same radius passing through a point come together after 180°, as shown in Figure 3. This arrangement later proved to be especially useful for measuring the abundance of isotopes and analyzing the products produced by electron ionization. Owing to the difficulty in devising suitable sources of ions for some elements, many of the isotopes were not discovered till late in the thirty-year period. The spectrograph illustrated in Figure 4 was constructed by Dempster in 1935 and, using a spark to produce ions, made possible the identification of 33 new isotopes in 17 elements.

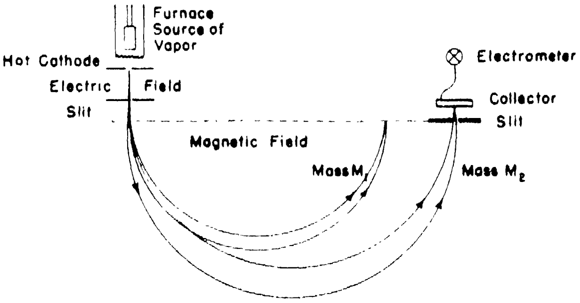


Fig. 3. Focusing of a divergent ion beam with same energy (Dempster, 1918).

Seven of the 283 naturally occurring isotopes were first detected by the analysis of optical spectra (C^{13}, N^{15}, O^{17}, O^{18}, Si^{30}, In^{113}, and Pb^{204}). Evaporation of liquid hydrogen at a low temperature served to concentrate the heavy hydrogen isotope of mass 2 to a point where it could be detected spectroscopically. The helium isotope of mass 3 was detected by the cyclotron and is at present the least-abundant stable isotope known to exist in nature, since it is approximately a million times less abundant than ordinary helium. Several physicists, particularly Bainbridge and Nier, using mass spectroscopic methods, added various isotopes to complete the present list.

The discovery that the chemical elements were made up of isotopes with nearly integral mass values led to the revival of Prout's theory of 1815, according to which the atoms of all elements are different aggregations of hydrogen atoms. The fundamental units in the nuclei were at first considered to be protons and electrons, but after the discovery of the neutron in 1932 and the development of the theory of nuclear "exchange forces," the building blocks were supposed to be protons and neutrons, the number of protons being equal to the nuclear charge and the total number of protons and neutrons being equal to the isotopic mass.

The binding forces holding these particles together are still a subject of investigation. Recently the discovery of mesons has introduced new elements into the theory of nuclear forces. To quote one theorist, "These forces, in which the neutral or electrically charged mesons play the role of photons, and the nuclear particles the role of sources of these heavy quanta, are still not known in detail, much less understood."

Among the 283 naturally occurring isotopes, several are of special interest. Five radioactive ones occur among the stable isotopes, K^{40}, Rb^{87}, Sm^{152}, Lu^{176}, and RE^{187}, and the fact that they still exist bears on the question of the age of the earth. Recently small amounts of the relatively short-lived radioactive carbon isotope C^{14} have been found in organic matter. Four stable isotopes of elements useful in biological studies have now been separated by the methods of physical chemistry, so that the hydrogen isotope of mass 2 (deuterium), carbon of mass 13, nitrogen of mass 15, and oxygen of mass 18 can be obtained by investigators. These may be built into organic molecules, or even into known parts of complex molecules, and be made to show the details of physiological or other processes, since they can be identified later by the mass spectrometer. Although the isotopes of an element, as the name signifies, are almost identical in chemical behavior, they may differ enormously in nuclear reactions; for example, some isotopes have a very much greater power of absorbing slow neutrons than their neighbors, the outstanding ones in this respect being B^{10}, Cd^{113}, Sm^{149}, Gd^{153}, Gd^{155}, Hg^{196}, and Hg^{199} (Figs. 2, 5). The mercury isotope of mass 196 has a very small abundance, but its absorbing power is so large that 5 percent of the neutrons absorbed form radioactive mercury atoms of mass 197, which after a few hours become ordinary gold. In a nuclear reactor we can make gold in appreciable quantities and thus finally realize the dreams of the alchemists.

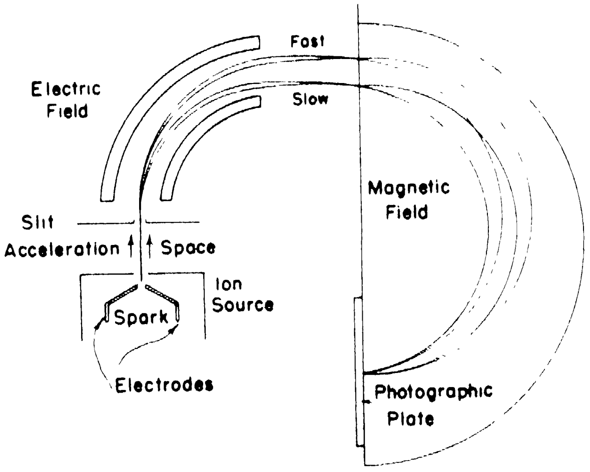


Fig. 4. Path of rays from spark source in a double-focusing mass spectrograph (1935).

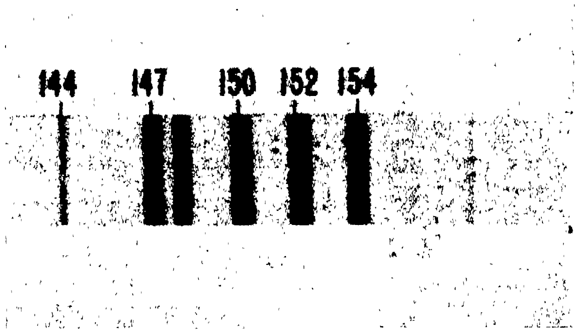


Fig. 5. Abnormal abundance of samarium isotopes produced by neutron absorption in isotope at 149.

The rare isotope of uranium, U^{235}, found in 1935, is also of special interest. It is worth noting as characteristic of the essentially glacierlike advance of basic science (popular opinion notwithstanding) that this very significant entity was not noticed until after fifteen years of active exploration in this field, and only after 200 other isotopes had been discovered. A photograph showing the doubly charged uranium isotopes is reproduced in Figure 6.

At first this isotope was of interest to the radiochemists because it satisfied all the requirements for the hypothetical "actino-uranium," the origin of the actinium series of radioactive substances. After the discovery of the fission phenomena in uranium, the fundamental scientific question arose as to whether the faint isotope was responsible for the effects observed. A few micrograms of uranium were separated in 1940, and the isotope at mass 235 was found to be especially susceptible to fission. This discovery led to projects for separating large amounts of the isotope.

Radioactive isotopes. Radioactive isotopes were identified in the naturally radioactive elements as early as 1906, and the chemical identity of several pairs and groups of radioactive substances was

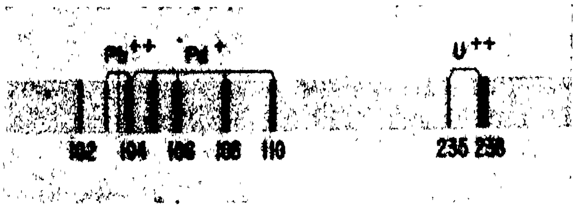


Fig. 6 Mass spectrum of uranium showing the faint isotope of mass 235. The palladium is from one electrode and served for calibration (Dempster, 1935).

well established in 1913 when the term isotopes was first introduced. Between lead and uranium 40 different kinds of radioactive atoms were known, and these fell naturally into 10 groups with 2–8 isotopes in each. The isotopes in each group were identical chemically, but presumably differed in mass.

The observation in 1934 by I. Curie and F. Joliot that alpha-ray bombardment of boron and aluminium gave rise to radioactive nuclei with an appreciable lifetime was the first item in an unexpected extension of our knowledge of isotopes. This was brought about by the discovery of many new methods of causing transformations. The cyclotron came into operation at this time and produced protons and deuterons of high energies; other methods of accelerating these particles were developed; the neutron was discovered and found to produce transformations in nearly all nuclei. The number of radioactive isotopes grew rapidly, and by 1943 more than 400 so-called active isotopes had been observed. Their masses not only occupy the gaps between the stable isotopes, but extend out to lighter and heavier masses. Chlorine, for example, has 2 stable isotopes at masses 35 and 37, and 4 radioactive isotopes at masses 33, 34, 36, and 38. These unstable atomic structures, usually called "radioisotopes," resemble each other and the stable isotopes of the same element in chemical properties only. In nuclear properties the radioactive isotopes differ radically not only in the radiations they emit but in the characteristic rates at which they change into stable nuclei. In some cases two radioactive isotopes called "isomers" may have the same mass, or one may have the same mass as a stable nucleus. After the discovery of the fission of uranium, the study of the radioactive fission products led to a new increase in the number, so that at the present time we know approximately 540 different radioactive isotopes that may be produced by various means.

The masses of about half of these can be deduced from the methods used to produce them, and among the remainder the mass spectrograph has been used to determine their mass in more than forty cases. For this purpose the radioactive atoms were analyzed in the same manner as stable isotopes, being collected on a gelatin film and detected by means of their radioactivity, since they are usually far too few for the ordinary detecting methods (Fig. 7). However, with the formerly "missing" element of nuclear charge 43, technetium, the long-lived radioactive isotope at mass 99 could be observed directly by means of the charge it carried.

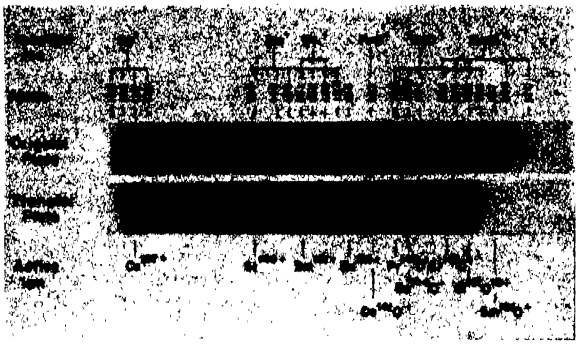


Fig. 7. *Above:* Mass spectrum of mixture of rare earth elements exposed to neutrons; *below:* transfer made by radiation from the radioactive isotopes (Lewis and Hayden, 1946).

Radioactive forms of carbon, sodium, phosphorus, calcium, iron, copper, and iodine are especially useful for biological studies, but unfortunately none of the radioactive isotopes of nitrogen or oxygen have a half-life longer than two minutes. Consequently, for many investigations in physiology and organic chemistry, it is necessary to use the stable isotopes of these elements at masses 15 and 18 and to depend on a mass spectrometer for their detection.

The plutonium isotope of mass 249, made by neutron absorption in uranium, was discovered in 1941 and, like U^{235}, was found to be highly fissionable. This discovery transformed the leisurely investigation of the possibility of a nuclear chain reaction as a source of power into the wartime development of nuclear reactors as sources of neutrons for the production of this isotope.

Relative abundance of the stable isotopes. Twenty-two of the naturally occurring elements have only 1 isotope, 20 have 2 isotopes, and the remaining 39, all even-numbered elements, share the remaining 221. It is remarkable that no two patterns of abundance are alike, and that no general theory exists at present to explain how the different elements should have acquired the particular isotope abundances they now have. For most elements, the isotopic abundance was deduced from the density of images on photographic plates. For some elements, a more accurate measurement has been made by comparing the total charges carried by the different isotopes. The term "mass spectrometer" is applied to an instrument in which the abundance of the various ions is measured electrically; instruments that record the separated ions photographically are usually referred to as "mass spectrographs."

Slight differences have been found to occur in the relative abundances of the isotopes of carbon, oxygen, boron, and lead. The carbon isotope at mass 13 is more abundant in organic material (1–88.3) than in limestones (1–92.3). The heavy oxygen isotopes have a different abundance in different limestones, depending on the temperature at which the calcium carbonate was laid down (Urey, 1948). Water from different sources varies slightly in density, owing to variations in the relative abundance of the oxygen isotopes (Dole, 1936): in fact, it is difficult to define a standard sample of oxygen for atomic weight comparisons beyond a certain accuracy. Lead from different regions varies in isotope abundance, probably owing to the admixture of lead of radioactive origin (Nier, 1938).

Radioactive transformations and neutron absorption can give rise to very abnormal isotope abundances. Thus, strontium is formed in certain micas from the radioactive decay of rubidium of mass 87, and consists almost entirely of 1 isotope. Very abnormal abundances have been produced in cadmium, samarium, gadolinium, and mercury, which have isotopes with very large absorption for neutrons. These isotopes capture neutrons and increase their mass by unity. The abnormal abundance produced in samarium is illustrated in Figure 5, which should be compared with the normal distribution of Figure 2.

Masses of the isotopes. The discovery by Aston in 1920 that the mass of the hydrogen atom was 1.008, slightly greater than unity, when the light oxygen isotope is taken as exactly 16, was of fundamental significance for theories of nuclear structure. The existence of isotopes with nearly integral mass values leads us to think of the constituents of the nucleus, and if it is built up out of hydrogen nuclei—that is, protons—or out of protons and neutrons, the mass of a nucleus is not the same as the sum of the masses of its constituents. The doctrine of the conservation of mass in all combinations and reactions, though still fundamental in chemistry, had, however, been modified by physicists long before 1920. In 1881 J. J. Thomson showed that a body should acquire added inertia when it is charged, and this "electromagnetic" theory of mass was used in 1902 to account for the increase in the mass of beta particles with increasing energy. This extra mass for transverse acceleration, according to Lorentz' theory, was the kinetic energy divided by the square of the velocity of light. Since neutral atoms could be ionized to form charged particles, there were speculations that all mass might be electromagnetic in origin. That light radiation should be accompanied by a transfer of mass was shown by Einstein in 1905 to be required by the relativity principle when applied to the electromagnetic theory of light. The same formula $E = Mc^2$ for the mass transferred M by an amount E of light radiation also followed, however, from simply considering the pressure exerted by light on an absorbing surface and applying the laws of conservation of energy and momentum.

Popular writers have sometimes assumed that the relativity theory predicted that the formula $E = Mc^2$ would hold for any new forms of energy or processes that might be discovered. Commenting in 1907 on the generality of this formula, Einstein wrote:

> The circumstance that the general law is deduced from a special case makes it necessary to examine the necessity of the deduction. We ask: "Might not other special cases lead to other conclusions?" A general answer to this question cannot be given at the present time inasmuch as we do not have a complete theory of the physical world corresponding to the relativity principle. We must limit ourselves to special cases which we can handle at the present time by the relativity theory of electrodynamics.

Mechanics was later incorporated in the relativity theory. One simple way of doing this made inertia appear as an effect of energy, just as electromagnetic momentum and light pressure had been an effect of electromagnetic energy, and in this formulation the relation $E = Mc^2$ also held.

At present, however, theoretical physicists recognize great difficulties in reconciling quantum phenomena with classical electrodynamics. They are now speculating on radically new types of electron theory, in one of which, for example, the electron has an infinite electromagnetic mass, but includes a term which gives the correct change when the electron is bound in the hydrogen atom. Coming to the nucleus and its constituents, we are still further from a satisfactory theory of the meson forces, which presumably hold together the many particles in a nucleus and thus determine its binding energy and its mass. That the differences between the atomic weights of the elements and exactly integral values might be attributed to different binding energies was first suggested by Langevin in 1913, and, in several cases of nuclear transformations among the light elements, experimental observations have in fact shown that changes in nuclear energy E are reflected quite accurately in the changes in mass M as given by the formula $E = Mc^2$.

Although the artificial disintegration of several nuclei by alpha rays was first observed by Rutherford in 1919, this discovery led to no immediate comparison of the energy set free in these processes with the change of mass—partly because protons of different energies are formed and the nuclei left in various excited states, but also because the masses of the isotopes involved were not accurately known at that time.

The discovery by Cockcroft and Walton in 1932 of the artificial disintegration of lithium by protons was a great stimulus to mass spectroscopy. For the first time physicists had a nuclear transformation for which they could measure the energy set free in a nuclear process and compare it with a change of mass. It was at once apparent, however, that, although the directly observed energy change could be measured with comparatively great accuracy, the uncertainties in the masses involved (hydrogen, helium, and lithium) amounted at that time to 40 percent of the change in mass resulting from the transformation. To have the mass differences match the energy measurements in accuracy, it would be necessary to determine atomic masses to four or five decimal places, a precision approaching that of optical spectroscopy, and a hundred times greater than that previously attained. Under this stimulus the following decade, 1932–42, brought great progress in the precise measurement of atomic masses. Bainbridge in 1933 redetermined the masses of 11 light isotopes, including those of lithium, by using a "velocity filter." This selected a group of ions in a narrow velocity range and gave sharper images on the photographic plate; but still the uncertainty in the masses involved in the transformation of lithium amounted to 30 percent of that predicted by the mass-energy formula.

In the years 1935–40 several spectrographs of a new type were developed—based on the principle of so-called double focusing. The paths of the ions in a spectrograph of this type are illustrated in Figure 4. The ions were formed by a vacuum spark and were accelerated and deviated first by an electric and then by a magnetic field. The differences in the deflections produced in the fast and slow rays by the electric and magnetic fields were arranged to cancel, and divergent rays also recombined at the photographic plate; hence, the term double focusing. The advantage in this arrangement is analogous to that obtained in optics by the use of an achromatic lens, which recombines light rays differing in color as well as in direction. This instrument was initially used to analyze the ions produced by a spark in order to complete the preliminary study of the isotopic constitution of the elements. Incidentally, the spark source was also found to give a great number of multiply charged ions of the solid electrodes for which no other source of multiply charged ions was available.

Some of the most accurate measurements of the masses of the light elements were made by Bainbridge and Jordan in 1936 by means of the mass spectrograph shown in Figure 8; masses differing by as little as 1 part in 10,000 were completely separated. The lithium mass was remeasured, and the decrease in mass in the transformation by protons was found to agree quite accurately with the value computed from the mass-energy formula. In the same year Mattauch and Herzog in Austria and Aston in England also completed instruments that gave accurate mass measurements of the light isotopes. Similar observations were carried out in Japan in 1939, and at the University of Illinois by Jordan in 1940. In these latter measurements, masses differing by only 1 part in 30,000 were completely separated. This marked an increase in accuracy of two hundred times that attained with Aston's first instrument, with which in 1920–22 he demonstrated the nonintegral nature of the hydrogen-oxygen ratio and the general shape of the packing fraction curve.

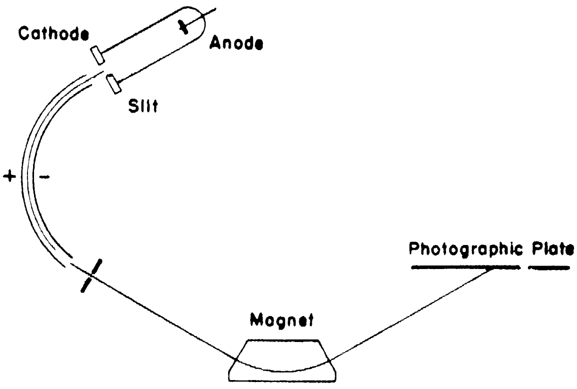


Fig. 8 Precision mass spectrograph for gas ions (Bainbridge and Jordan, 1936).

WITH the greater accuracy made possible with these instruments, the masses of 17 light isotopes were measured, and in the meantime many nuclear transformations had been found among these isotopes. Many of these transformations involved the emission of beta rays with energies that varied over a wide range, thus making it impossible to apply the formula $E = Mc^2$ to calculate an expected mass difference between the isotopes. It was, however, possible to maintain the formula by postulating a new entity, the neutrino, of very small mass, which carries off the energy difference between that actually in the beta ray and a constant maximum amount.

Apart from the 17 mass measurements just

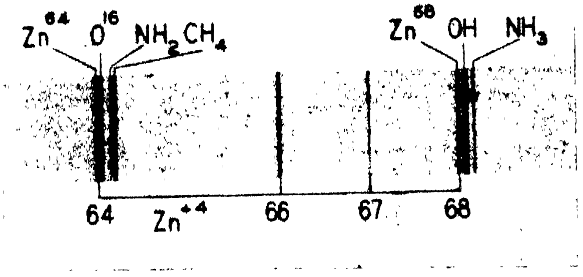


Fig. 9. Zinc ions with four charges compared with other ions at masses 16 and 17.

referred to, we have also at present approximately sixty comparisons between the heavier stable isotopes. These show several points of interest even though the accuracy is less than that attained with the light elements. Taking the lightest oxygen isotope as exactly 16, the masses of the isotopes depart from exactly integral values by various amounts. The fraction given by the difference divided by the integral value, amounting to a few parts in 10,000, is called the "packing fraction," and values have been measured for many of the isotopes. Figure 9 illustrates the mass spectrum of zinc with 4 charges superimposed on several ions whose masses are very accurately known. Unfortunately, such direct deductions of packing fractions have been made for only a few isotopes. Figure 10 illustrates a comparison between doubly charged isotopes of uranium and thorium with singly charged tin isotopes. From sixty such comparisons it is possible to draw a packing fraction curve as shown in Figure 11, which, although not unique, does pass through values that are in accord with the measurements. If protons and neutrons are taken as the nuclear constituents, the binding energies are very large; per particle the energy amounts to several million electron volts as shown by curve *B* in Figure 11. These very large binding energies indicate that the constituents of nuclei are held together by very much stronger forces than those that hold the atoms in molecules.

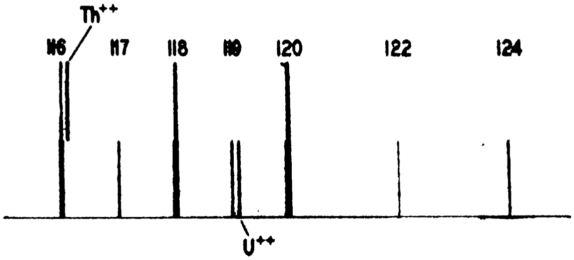


Fig. 10. Mass spectra of uranium and thorium with two charges superimposed on singly charged tin isotopes (Dempster, 1936).

The mass changes in two nuclear phenomena, natural radioactivity and nuclear fission, are of special interest. Uranium of mass 238.13, after a series of radioactive transformations, finally becomes lead of mass 206.05. Subtracting only the masses of particles emitted, we would expect the mass of the lead to be 206.10. However, extra mass is lost and the energy equivalent of this extra mass using $E = Mc^2$ agrees very roughly with the total energy of the particles emitted in the series of transformations.

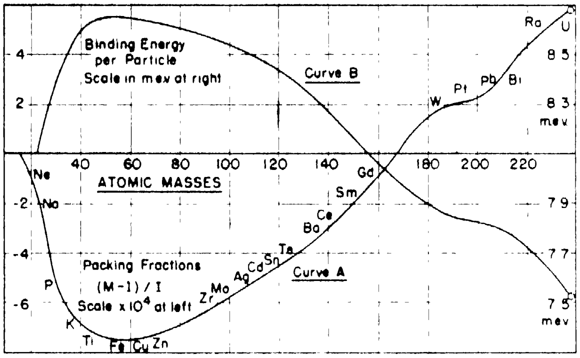


Fig. 11. Curve *A*, packing fraction curve; curve *B*, binding energy per particle in nucleus.

The mass differences shown in Figure 10 suggest a comparison with the energy released in the fission of a uranium or a thorium nucleus. In a more common type of fission, for which the masses may be taken from Figure 10, the uranium nucleus of mass 235.12 absorbs a slow neutron of mass 1.008 and produces as fission products columbium (92.944) and praseodymium (140.958). Two neutrons may also be formed (2.016) and, necessarily, 8 electrons (0.004). The total mass decrease is thus 0.211, and we would expect, if the mass-energy relation $E = Mc^2$ holds, that an amount of energy given by $0.211 \times 9 \times 10^{20}$ ergs should be released in the fission of 235 grams of the uranium isotope. In other units this is 20,000 kilowatt hours per gram, or 196 million electron volts (Mev) per atom for this particular method of fission. About 22 Mev remain for a time in the excited nuclear energy states of the fission products, and the remainder is released as kinetic energy of the nuclear fragments. That these nuclear fragments do have large kinetic energies was readily demonstrated by many experimenters in the spring of 1939, but the ranges of the fragments or the sizes of their ionization pulses do not supply us with accurate values of the energies of the particles. Direct calorimetric observations of the heat developed gave a rough agreement with the value computed above, but at the present time we do not have accurate measurements of this interesting quantity and do not know how accurately the formula $E = Mc^2$ applies to this phenomenon.

Applications to chemistry, biology, and industry. Over a period of several decades the methods devised in one field of science have usually found applications in others, and this has been the case for mass spectroscopy. The method of analysis illustrated in Figure 3 was adapted very early in our period to the study of the kind of ions produced by the impact of electrons of various energies. In 1922–30 H. D. Smyth and others, by gradually reducing the electron velocity, found the minimum potential at which particular ions are first produced by electrons, and secured important data for the development of the quantum theory of molecular structure. In 1933–38 Professor Tate and others observed the patterns of singly and multiply charged ions that are produced in various gases by electron impact. This application of mass spectroscopic techniques served as a basis for a new method of chemical analysis, whose importance in special fields is just beginning to be realized. The impact of 72-volt electrons on the benzene molecule, C_6H_6, for example, gives rise to 45 different positive ions with definite relative abundances. Conversely, the ion pattern can be used to identify the molecule, and the pattern observed with a mixture of a dozen or more hydrocarbons can be analyzed and ascribed to the superposition of the standard patterns from particular hydrocarbons. Instruments for carrying out these analyses in the oil and rubber industries were developed in 1939, and at the present time more than 30 such instruments are in continuous use in this country.

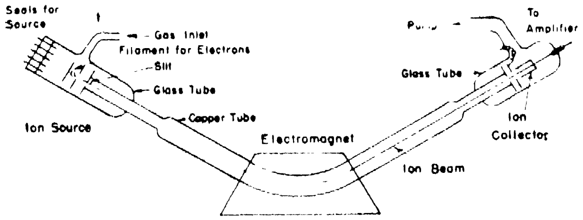


Fig. 12. Mass spectrometer for analysis of ions from gases (A. O. Nier, 1940).

The mass spectrometer illustrated in Figure 12 was developed by Nier in 1940; it has recently been adapted for biological investigations, in which carbon of mass 13, nitrogen of mass 15, or heavy oxygen isotopes are used as "tracers." In such studies carbon dioxide, for example, may be made with the heavy carbon isotope and built up by enzymic conversion in tissues into complex organic molecules. The carbon is examined in different organic compounds separated from the tissue, and the molecule into which the carbon dioxide had been built is identified by its extra content of carbon of mass 13. Two curves showing the ions from carbon dioxide are given in Figure 13. One of these was enriched with the radioactive carbon of mass 14.

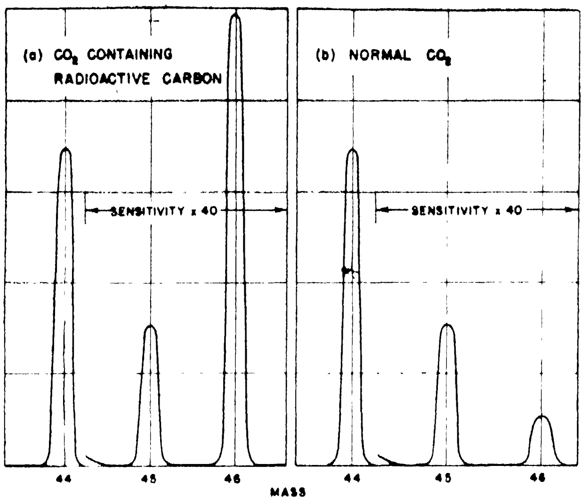


Fig. 13. Mass spectrometer record of ions from carbon dioxide. Mass 44 is due to $C^{12}O^{16}_2$, mass 45 is due to $C^{13}O^{16}_2$, and mass 46 is due to $C^{12}O^{16}O^{18}$ in (*b*) and to this ion plus $C^{14}O^{16}_2$ in (*a*) (Ingraham and Hess, 1947).

Mass spectrometers of this type can detect the presence of a very small amount of one gas even when it is mixed with large amounts of other gases. Recently it has been found, for example, that much besides carbon dioxide is extracted by the lungs from the blood and exhaled in the breath. The mass spectrometer illustrated in Figure 14 was built by A. O. Nier and A. Hustrulid in 1943 for detecting leaks in vacuum equipment. Permanent magnets are mounted inside the glass tube, and the instrument is adjusted to indicate the presence of

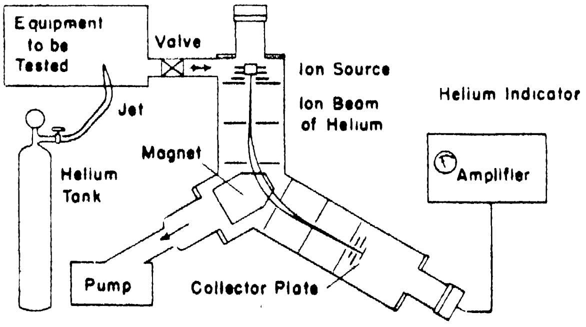


Fig. 14. Mass spectrometer for detecting leaks (Nier and Hustrulid, 1943).

helium. Helium gas from a small jet is allowed to flow over suspected points in the equipment, and as soon as a hole is reached helium enters and immediately indicates the location of the leak.

Separation of isotopes. Every mass spectrograph separates isotopes, but usually only in the amount necessary to record their presence. In many cases, however, larger amounts have been collected. About one fifth of a milligram of the 2 lithium isotopes was collected by Oliphant, Shire, and Crowther in 1934 for studying their transformation under proton and deuteron bombardment. To determine which isotope of potassium emitted beta rays, 20 milligrams were separated in 1937 by W. Smythe and A. Hemmendinger and collected in hollow containers. Similarly, to show that it was the rubidium isotope at mass 87 that emitted beta rays, the same experimenters collected 2 milligrams of the separated isotopes. In 1944 separated isotopes of thallium and silver were collected by W. Walcher for optical studies at the rate of 1.5 micrograms per hour. A. E. Shaw has collected the separated isotopes of samarium at the rate of 2 micrograms per hour.

The most sensational of these small-scale separations, however, was the separation in 1940 of 4 micrograms of uranium by Nier and 2 micrograms by K. H. Kingdon and H. C. Pollock. With these samples the fundamental scientific discovery was made and confirmed that the faint isotope of mass 235 was extremely susceptible to fission, and that a reasonable amount of this isotope, if separated, would constitute an explosive of unprecedented violence. One method that was developed to separate this isotope in large amounts used the magnetic deflection method of mass spectroscopy; however, the most important element in its success was the skill, based on cyclotron experience, with which the many engineering problems were met. The instrument called the "calutron" was adapted in 1946 to separating milligram amounts of the isotopes of many chemical elements. These are now available for experimental use in physics, biology, and metallurgy.

Other methods for separating the uranium isotopes were based on thermal diffusion or diffusion through a barrier. In the development of all these separation methods, a mass spectrometer was essential, in order to observe the enrichment actually attained. It was the compass that brought these great ventures safely to port. That these instruments could be rapidly provided when the need arose was possible only because of the experience gained in this country during the previous decade in the scientific investigations already referred to. No more striking demonstration of the importance of scientific "capital" for the national welfare could be imagined.

At the present time the subject of mass spectroscopy has two distinct aspects; on the one hand, it is still a fertile field of scientific investigation, and, on the other, it provides instruments, methods, and materials for investigations in chemistry, biology, and geology, and for the control of industrial processes. The study of the isotopic constitution of the elements, although almost completed for the stable isotopes, is still very active in the analysis of many of the radioactive isotopes; the natural abundances have still to be measured with the precision needed to detect variations brought about by many enrichment processes; and the measurement of masses is far from adequate, both in the accuracy attained and in the small fraction of the isotopes for which measurements of any kind exist. Because of the position of the subject in relation to theory and applications, we may confidently expect significant advances in the decades ahead.

◆

GENES, CYTOPLASM, AND ENVIRONMENT IN PARAMECIUM*

TRACY M. SONNEBORN

In 1946 Professor Sonneborn, of the Department of Zoology, Indiana University, shared half the Annual Thousand Dollar AAAS Prize with Miss Ruth Dippell and Miss Winifred Jacobson, also of Indiana University.

AMONG organisms of all sorts, differences between individuals in hereditary traits are due to differences in nuclear genes. The remainder of the cell, the cytoplasm, is usually conceived as playing in most cases a passive role, of little or no significance in heredity. So far as environment is concerned, it may determine to what extent a hereditary trait will manifest itself, or indeed whether it will manifest itself at all, in a particular individual; but these environmental effects are not lasting: they do not extend to subsequent generations.

These generalizations must seemingly be reversed when one turns from the individual to the parts of an individual. The cells of the body of a higher organism are of many different kinds—muscle, nerve, connective tissue, and so on. Yet there is reason to believe that all these kinds of cells in one body have exactly the same set of genes. They arise by repeated divisions from a single cell, the egg, and at each cell division the nucleus with its genes seems to be partitioned with precise equality. How then can one explain the diversity among the body cells if the genes control their traits and if all have the same genes? The problem is even more puzzling. Different cell types from one body may be grown as long as desired in tissue cultures or in grafts, and many of the cell differences persist. They are, in effect, hereditary differences among cells apparently identical in genes. Although little is really known as to the explanation of these facts, it is suspected that cytoplasm and environment may be important in this sort of heredity, "heredity on the cellular level," which must play an essential part in the development of every individual from the egg.

One of the great bars blocking progress in exploration of these problems is the nasty fact that diverse body cells cannot be bred to each other the way Mendel bred peas and Morgan bred fruit flies. Yet such crossbreeding was the essential prerequisite for discovery of the mechanism of inheritance of the individual. As there seems at present no likelihood, at least in the foreseeable future, that body cells can be mated to each other, the outlook for analysis of the nature of the hereditary differences among body cells is not promising.

There is available, however, an indirect and second-best method of tackling these problems. Among organisms that consist of but a single cell, the group of individuals produced from a single ancestral cell by repeated divisions is fundamentally comparable to the group of body cells produced from an egg. There is no doubt but that the cells of such a group of unicellular individuals all have the same genes; but unfortunately they also usually have exactly the same hereditary traits. In some cases, however, they may develop different traits. And, what is more important, *any* of these cells in certain species can be mated, like peas or fruit flies. Here then is a great opportunity to try to find out how it happens that cells with the same genes can develop hereditarily different traits, and to discover what parts are played in this by genes, by cytoplasm, and by environment.

In the hope that such an investigation might throw light on these fundamental problems, which are pregnant with implications for an understanding of normal and pathological development of higher organisms, there has been in progress for eighteen years in my laboratory an intensive study of the common and familiar unicellular animal *Paramecium aurelia*. After twelve years of work, which must be considered as having only prepared the way for the main job, the past six years of work have begun to lead to solutions of the problems at the center of interest. The following account is an attempt to present briefly the chief results.*

Studies on heredity in paramecium go back forty years to the pioneer researches of Jennings in the period just before the fruit fly *Drosophila* became the password to epoch-making discoveries about the mechanism of heredity. For thirty years, however, the study of heredity in paramecium remained to a large extent outside the main currents of genetic advance, because of an important limitation in technical possibilities. Investigations by Maupas, the French librarian at Algiers, had shown by 1888 that, when paramecia come together in pairs and adhere, they fertilize each other and undergo essentially the same nuclear processes that take place in the mating of higher organisms. Yet this mating, or conjugation, could not very well be used in studies of heredity, because paramecia with different hereditary traits could not be made to conjugate with each other. As the crossbreeding of diverse hereditary types is the first requirement for the sort of study we wished to make, this type of work was not possible. In spite of this technical limitation, much of interest and significance was discovered, as a perusal of Jennings' review on *Genetics of the Protozoa* will show.

---

* In this recent work, there have been numerous able collaborators: my research associates Ruth V. Dippell and Arlene LeSuer; research assistants Winifred Jacobson, Betty Bartel, Jeanne Whallon, and Brunhilde Memmert; technicians Marcella Chandler and Jewell Stogsdill; graduate student John R. Preer, more recently an independent investigator making important contributions in his own laboratory at the University of Pennsylvania;

During the 1930s, determined efforts were made to overcome this technical limitation. These finally led to Sonneborn's discovery of sexes, or mating types, in paramecium. As the mating types themselves proved to be hereditary, it was easy to start with one animal and raise large cultures in which all the animals were of one mating type. When such a culture was added to a culture of the opposite mating type, the animals at once began to stick together and form large clumps. In an hour or so these clumps broke up into pairs of conjugating animals, each pair consisting of one animal of each mating type. With this knowledge, crossbreeding animals with different hereditary traits became as simple as in higher organisms; it was only necessary to mix together two cultures differing in mating type as well as in the other trait one wished to study, and the necessary mating would occur immediately.

When individuals of the single species *Paramecium aurelia* were collected from various parts of the United States, grown into stock cultures, and studied with respect to their mating types, it was discovered that this species is really a *group* of eight "physiological species," or "varieties," as they are called. Each variety has two mating types that interbreed freely, but interbreeding between different varieties is practically impossible.

After this foundation had been laid, progress in the study of heredity proceeded apace. The Mendelian laws were quickly found to operate in paramecium just as in the heredity of higher organisms; genes and chromosomes behaved at conjugation just as in the maturation and fertilization processes of higher organisms.

Two kinds of traits have been examined in a study of genes, cytoplasm, and environment in the control of heredity in paramecium. Surprisingly, two different systems of control are involved for these two kinds of traits. Yet the two systems are similar in some important respects, and each seems to have implications of broad and general significance. (A full account of the background material, together with much of what follows, will be found in a review by Sonneborn [1947].)

GENES, CYTOPLASM, AND ENVIRONMENT IN THE CONTROL OF THE "KILLER" TRAIT

In varieties 2 and 4 occur several stocks known as "killers" because fluid in which they have lived kills paramecia of other stocks. The latter are therefore called "sensitives." The killing substance in the fluid has been identified by van Wagtendonk as belonging to the class of chemicals known as desoxyribonucleoproteins. Such chemicals are of special interest, because they occur in chromosomes and viruses and are believed to be the stuff that genes are made of. This killing substance, which is known as "paramecin," also occurs inside the body of killer paramecia, but in them it does no harm. Paramecin is among the most active of biological substances: a sensitive paramecium can be killed by a single particle of it. Precisely what a single particle may be, however, is not entirely clear. Quite possibly a single particle is a single molecule, but it may be a highly polymerized molecule or even an aggregate of molecules. Each killer animal carries only a small number of particles of paramecin in its body. Samples of fluid containing many paramecin particles have little or no killing action on sensitive paramecia if they are conjugating, are at low temperatures, or have an abundance of food available.

There are a number of different kinds of paramecin, each produced by a different strain of killer paramecia. Two kinds have been studied very fully. One kind, produced by stock 51 of variety 4, acts so as to produce clear blisters on the surface of sensitive animals within a few hours. Later, the body form alters, a hump developing near the hind end of the body on the side away from the mouth.

---

visiting investigators in my laboratory, Professor Mary L. Austin, of Wellesley College, and Dr. G. H. Beale, of the Institute of Animal Genetics, Edinburgh; my colleague, the biochemist W. J. van Wagtendonk, and his research assistants, Patricia Hackett, Leonard Zill, and Donald Simonsen. For the privilege and means of carrying on cooperative investigations of this magnitude, we are deeply indebted to Indiana University for encouragement, financial support, working facilities, space, and that indispensable item—time for research; to the Rockefeller Foundation, the National Institute of Health, and the Jane Coffin Childs Memorial Fund for Medical Research, for liberal financial assistance.

The animals stop feeding, move slowly, grow smaller, round up, and finally die within 24–48 hours. Another kind of paramecin, produced by stock G of variety 2, acts more slowly. Early in the reaction, sensitive animals move aberrantly, often reversing the direction of their normal spiral movements and undergoing violent spinning on their longitudinal axis. They stop feeding, move more slowly, and die after several days. Still other kinds of paramecin act with great speed, killing their victims within a few hours.

The capacity to produce a particular kind of paramecin is inherited. This part of the work was carried out most fully on the killer stock 51 of variety 4 and on the sensitive stocks, 29, 32, and 47, of the same variety. Each of these stocks reproduces absolutely true to type under standard conditions. Within stock 51, all animals are killers; the trait is inherited through years of reproduction: thousands of cell divisions and many conjugations. With equal stability, all cultures of stocks 29, 32, and 47 are pure sensitives through fission and conjugation.

When the killer stock is mated to the sensitive stock 47, the breeding analysis shows that the two stocks do not differ in any gene affecting the killer or sensitive traits, but that this difference in traits is due to a difference in cytoplasm. The production of paramecin depends on a cytoplasmic factor, called "kappa;" when this gets into sensitive animals from stock 47, it converts them into killers. All the progeny of the converted animals are also killers and contain kappa, so it must multiply. It thus has the property of self-multiplication, the basic property of genes. Miss Dippell has shown that kappa also mutates like a gene, mutant kappas controlling the production of different kinds of paramecin which kill sensitive animals in different ways, produce paramecin more slowly, or differ in still other ways. Nevertheless, kappa is not in the nucleus and is therefore not a gene in the usual sense.

On the other hand, neither is kappa entirely independent of the genes. This was discovered by breeding analyses following the mating of the killer stock 51 to either of the sensitive stocks 29 or 32. The results showed that a dominant gene *K* must be present if kappa is to persist and multiply; when an alternative recessive gene *k* is present in pure (homozygous) form, kappa cannot survive. Stock 51 is pure for gene *K*, stocks 29 and 32 are pure for gene *k*. When stock 29 or 32 is bred so as to replace their *k* gene by *K*, they remain sensitive and no kappa arises; hence the gene *K* cannot make kappa arise. But if some kappa is also added when *K* has been introduced, then the kappa multiplies and transforms the animals into killers. Conversely, when animals of the killer stock 51 have their *K* gene replaced by *k*, kappa disappears and they transform into hereditary sensitives. As expected, restoration of the *K* gene cannot restore the killer trait: kappa is irretrievably gone.

The gene *K* thus controls something in the organism that is essential for the maintenance of kappa, but it cannot initiate the production of kappa if none is present to start with. Breeding analysis showed that stock 47 contains the gene *K* but lacks kappa. That is why, in the crosses to this stock, inheritance seemed purely cytoplasmic with no gene control at all. The gene control was there, but it could not be discovered because the two stocks (51 and 47) were alike in having the required gene.

Much more insight into the nature of kappa was obtained by a study of the effect of environmental conditions on it. The kappa in stock G is a slowly multiplying kappa. When the animals are given an excess of food, they multiply more rapidly than kappa; consequently, the amount of kappa per cell gradually declines. By varying the growth rate of the paramecia, Preer found that they could maintain kappa only if grown no more rapidly than two divisions per day, which is therefore the maximum rate at which kappa can multiply in this stock. When the animals were grown more rapidly and the average concentration of kappa per cell steadily declined, the animals transformed, first to resistant nonkillers, then to sensitives, which could be converted back to killers by growing them slowly, and finally to irreversible sensitives that had lost kappa. Using data obtained from ingenious experiments, including some in which kappa was destroyed by X-rays, Preer calculated that there were approximately a few hundred kappa particles in the original killer animals. Mathematical methods, essential for this sort of analysis, were developed for our use by Richard Otter, of Princeton University. Similar estimates of particle number for the stock 51 kappa were obtained by others, using high temperatures to stop kappa multiplication or destroy it.

As the preceding results prove, kappa, though genelike, is both gene-dependent and environment-dependent. A hereditary killer can be transformed into a hereditary sensitive either by substituting the gene *k* for its *K* gene or by growing the animals rapidly, subjecting them to X-rays, or exposing them to high temperatures. Moreover, these environmental effects—if carried to the point where *all* kappa is gone—are irreversible; they are permanently inherited. Preer showed that if a single particle of kappa remains, it will, under conditions of slow growth of the animals, restore the full original amount and result in reversion to the killer trait.

In view of some of these results, the question has been raised by Altenburg, Lindegren, and Spiegelman as to whether kappa might be a symbiotic organism. Two recent discoveries are of interest in that connection. First, Sonneborn discovered that kappa is, under extreme conditions, "infectious." When sensitive animals with the *K* gene are exposed to immense concentrations of kappa in their culture fluid, a certain fraction of them acquire kappa and transform into permanent hereditary killers. This apparently never happens except when kappa is concentrated enormously beyond the limits ever likely to occur in nature. Second, Preer found that kappa is much larger than a gene; it is in fact larger than most viruses, as large as rickettsias or small bacteria. This remarkable discovery was made by following two clues. The X-ray inactivation studies indicated a particle of about this size. The discovery of the desoxyribonucleoprotein nature of paramecin suggested that kappa also might be of similar nature. As there is a specific staining method (the Fuelgen method) for such substances, Preer used this stain and found particles of the expected size in killer, but none in sensitive, paramecia.

This brings the status of investigation of kappa up to date. There is still no decisive evidence on which to base a secure judgment as to whether kappa is a symbiotic organism. Essentially, the same uncertainty exists for a comparable cytoplasmic factor in the fruit fly *Drosophila* investigated by L'Heritier and his collaborators. So far as paramecium is concerned, however, it may be said that the staining methods that reveal kappa do not reveal other particles of similar size and chemical nature. Kappa may therefore be an example of a rare and exceptional group of phenomena. Its study, however, emphasized the difficulty of distinguishing between a normal cytoplasmic genetic factor and a symbiotic or parasitic microorganism. Any pathogenic or symbiotic organism that became established in the cytoplasm and passed from generation to generation through the cytoplasm of the egg could simulate the behavior of kappa. There is, in fact, reason to suspect that comparable agents are involved in the causation of certain types of cancer. It has further been suggested by Darlington that these agents may not be organisms, but self-multiplying particles or plasmagenes native and normal to the cytoplasm of other creatures than those in which they cause disease. Whatever may be the correct interpretation of kappa, it is remarkable that its pathogenic action is confined, not to those animals in which it occurs, but to those that lack it. It controls the formation of paramecin, which is harmless to animals containing a full allotment of kappa, but lethal to those paramecia that have no kappa or too little of it. Perhaps further work will throw light on the knotty problem of the origin and nature of kappa and the comparable events involved in mammary cancer of mice, sarcoma of fowl, and resistance to CO_2 in the fruit fly.

GENES, CYTOPLASM, AND ENVIRONMENT IN THE CONTROL OF ANTIGENIC TRAITS

Within the past year, attention has been directed to other traits in variety 4 of *P. aurelia* which show an even more interesting and probably more general system of interrelations between genes, cytoplasm, and environment. When paramecia are injected into a rabbit several times, blood serum obtained from the rabbit, even when greatly diluted with water, will paralyze the normally rapid-swimming paramecia. This paralytic action of the serum is due to union of specific chemicals in the paramecia, called antigens, with corresponding specific chemicals in the serum, called antibodies. The antibodies, are formed in the rabbit's blood in response to antigens of the injected paramecia. This is a highly specific reaction. If two different strains of paramecia, *A* and *B*, are injected into different rabbits, the serum obtained from the rabbit injected with *A* paramecia will paralyze paramecia of strain *A* but may have no effect on the *B* paramecia, and vice versa.

Six such diverse strains (*A, B, C, D, E,* and *G*) arose within the one stock 51 of variety 4, and six specific antisera were obtained against them. The six diluted antisera each paralyze specifically only animals of the corresponding strain or antigenic type. By serological methods, it was shown that the paralysis reaction on each type is due to interaction between *one* kind of antigen and its corresponding antibodies. There are thus six different antigens in the six types of strains. Each of these six types is strictly inherited under standard conditions of culture (27° C with enough food to permit one fission per day).

When these six types are mated to one another and the hybrids are analyzed by further breeding, the results show that the differences in antigenic type depend not on differences in genes, but on cytoplasmic differences. The cytoplasmic factors involved here, however, cannot be seen by the staining method used for demonstrating kappa.

As with kappa, the cytoplasmic factors, or plasmagenes, involved in the control of these antigens are subject to environmental influence. When animals of any one of these six types are placed in a few drops of the corresponding paralyzing antiserum, all the animals are paralyzed; but if the serum is dilute enough, all will recover when removed from the serum. The recovered animals may be isolated and each will multiply to form as large a culture as desired. Surprisingly, when such cultures are now subjected to the same antiserum again, they are no longer paralyzed by it. Instead, they are now paralyzed by a different one of the six antisera. In other words, they have been transformed to a different one of the six antigenic types. Moreover, this transformation is inherited thereafter as long as the cultures are grown under standard conditions. Large proportions of the exposed animals are hereditarily transformed, up to 100 percent in some cases. In an analysis of the details of one of these transformations, Beale has found that the original antigen ceases to multiply after exposure to antiserum and another antigen begins to appear after several hours. The latter gradually increases until transformation is complete. This suggests that the antigen is itself a plasmagene which cannot multiply when combined with antibody. This gives an opportunity for another plasmagene to multiply and replace it.

These facts about transformation of antigenic type imply that each animal actually has several possibilities, most of which are not or cannot be realized without sacrificing other possibilities. There is clearly something mutually exclusive about these possibilities, for an animal can manifest only *one* of the six antigens at a time. By the method of exposure to paralyzing antiserum, it can be transformed (directly or indirectly) so as to manifest *any* one of the six antigens. Moreover, the transformations with antiserum involve no irreversible loss of capacities, because further exposure of transformed animals to antisera which can paralyze *them* can bring the organisms back to their previous antigenic type. Hence, the capacity to produce all six antigens is inherent in all animals of stock 51, but only one—any one—of these six capacities can be realized at any one time. Since the antigenic type has been shown to be determined by cytoplasmic factors, or plasmagenes, and since the mechanism of transformation of type with antiserum indicates that the antigens are themselves the plasmagenes, there must be a different kind of plasmagene for each of the six kinds of antigens. The existence of different plasmagenes for different antigens is further neatly shown by the discriminating action of each of the six antisera: each suppresses one, only one, and always a different one of the six antigen plasmagenes.

As already indicated, transformation of antigenic type involves great increase of an antigen of which little or none could previously be detected. It might be supposed that change of antigenic type could involve only a shift of position, without change in quantity, of the antigens. Antibodies are very large molecules, and it seems improbable that they penetrate the cell; more likely, they act only on the surface of the cells. Hence, they could unite only with antigens on the cell surface. If antigens inside the cell wandered to the surface, this could conceivably be a mechanism of transformation of antigenic type without resorting to an increase of the new antigen at all. This possibility seems unlikely, however, in view of the serological evidence. The paramecia are thoroughly broken up before they are injected into the rabbits to induce antibody formation. Antibodies against more than one of the six antigens are sometimes found in the antiserum, but the great bulk of the antibodies present are specific for the one antigen that characterizes the type of the injected paramecia. The same conclusion is indicated by the reverse experiment, for it is possible to discover what antigens are present in animals by their capacity to inactivate a serum: the antigens present combine with their corresponding antibodies in the serum and remove them from solution. In this way it can be shown, using either whole or broken-up paramecia, that each antigenic type contains mainly or exclusively one type of antigen, only traces of the other antigens being present if they are present at all. Hence, change of antigenic type must involve an enormous increase in the antigen characteristic of the type to which an organism transforms.

This leaves an interesting problem. An animal ready to transform as a result of exposure to paralyzing antiserum has five possibilities open to it. What determines which one of these five other antigens will multiply and replace the antigen inactivated by antibody? On the hypothesis that a competition is involved here, with the best-adapted antigen taking over, attempts were made to discover how to control which antigen would take over. In view of the previously proved importance of temperature and food supply on the growth of kappa, and a priori probability that these conditions, if any, might be important in influencing the outcome of competition among plasmagenes, experiments to test their action were carried out. They proved to be decisive.

Animals exposed to paralyzing antiserum transform to one type at high temperature, to a different type at low temperature; to one type when fed abundantly, to another type when fed little. With this clue, the same conditions of temperature and food supply were employed *without exposing the animals to paralyzing antibodies.* These alone also brought about transformations. But now the transformations occurred much more slowly: with antiserum, transformation is complete within 2 or 3 fissions; without antiserum, 20, 30, or more fissions are required to transform. This difference in rate of transformation was actually predicted because antiserum had been interpreted as "knocking out" the main competitor antigen plasmagene, a process that should greatly accelerate transformation. Regardless of how the transformations are brought about, they are permanently inherited, even through sexual reproduction, under standard conditions of culture.

These results seem to throw light on some remarkable, long-known, but puzzling facts in parasitology. Another kind of unicellular animal, the trypanosome, may undergo similar transformations in the course of its infection of the mammalian host. Some trypanosomes cause sleeping sickness and other diseases in man. In laboratory animals, such as the dog, after the trypanosomes have greatly multiplied in the blood and the dog is very sick, there comes a crisis and the dog seems to recover. Trypanosomes practically disappear from the blood, because of the action of induced antibodies formed against the trypanosomes. But a relapse occurs and trypanosomes again teem in the blood; they had not all been killed off. The new trypanosome population is unaffected by the antibodies that had been built up in the dog, for the parasites are of a different antigenic type! Now comes a new crisis, another depletion of the trypanosome population, due to the action of new antibodies formed against the new type of trypanosome. But this is far from the end. Relapse and crisis may alternate a dozen times or more before the dog or the trypanosome finally gives in and dies. With each relapse, the trypanosomes have a new antigenic type; with each crisis, the dog has built up a new kind of antibody.

As with the transformations of antigenic type in paramecia, the diverse types of trypanosomes reproduce true to type. This is shown by cultivating them in an organism like the mouse that fails to form antibodies against them. In the case of the trypanosomes (as in other, similar cases in other organisms), it has long been supposed that the antiserum acted as a selective agent, killing off all the trypanosomes except a few that had spontaneously (and independently of exposure to antibodies) mutated to a new antigenic type. The results with paramecium show that the same kind of result can occur when selection of spontaneous mutations is excluded; that the "mutants" are indeed stimulated to arise by the action of the antibodies; and that the "mutants" are not mutants in the sense of having changed genes or nuclei, but differ only in their cytoplasm, one antigen plasmagene rising in concentration while another declines.

Thus far only the roles of cytoplasm and environment in the control of antigenic type in the paramecia have been set forth. All the types are alike in genes, and so the genes seem to have no determining effect at all. Further analysis shows, on the contrary, that this is an incomplete and false picture. The role of genes was discovered by extending the studies to stock 29, another stock of variety 4. In this stock, six antigenic types have also been found. The results obtained with the six antigenic types of stock 51 have all been confirmed with the six types in stock 29. But the antigenic types occurring in these two stocks are not identical. In stock 29, four of the six types are similar to the types *A, B, C,* and *D* in stock 51, though they are not exactly the same; the other two types, *F* and *H*, do not occur in stock 51; and the two types *E* and *G* of stock 51 do not occur in stock 29.

When one performs breeding experiments to determine the basis of the difference between the two stocks in the capacity to form antigen *F*, it turns out that this is due to a difference in a single pair of genes. The capacity to make the *F* antigen plasmagene depends on a gene which is present in stock 29, absent in stock 51. Note that the possession of this gene does not assure that the possessor *will* be type *F;* it merely means the possessor *can* be type *F*. As the methods of transforming any type to type *F* (if the required gene is present) have been discovered, it is always possible to determine when the gene is present.

From these results, it looks as if each of the antigen plasmagenes can be formed when the proper genes are present, not otherwise. In this respect, the results are very different from the results with kappa and the killer trait, for kappa could not be formed under the action of the required genes unless some kappa was already present. As the overwhelming importance of genes in the control of hereditary traits is abundantly demonstrated by investigations on all sorts of organisms during the present century, the system of determination and inheritance manifested by the antigenic traits seems likely to be more general and significant than the system earlier discovered for the killer trait.

WE RETURN now to the problem with which we started. What may our discoveries signify with reference to the dark problem of how the cells of complex higher organisms come to be diverse? Like the different antigenic types within one stock of paramecia, the different cell types in the body of a higher organism probably have the same genes. The results on paramecium show how cells with the same genes can differ persistently in their traits as a result of competition between alternative and mutually exclusive plasmagenes, in spite of the fact that the plasmagenes themselves are dependent upon genes for their origin and maintenance. There is at least a possibility that similar mechanisms operate in the differentiation of different cell types in the body of a higher organism. If so, there emerges the hope of similar control of cell type, of control of the process of transformation from one cell type to another, including even the transformation from normal to cancer cells and, what is of greater practical importance, the reverse transformation.

This account of the status of the work on antigenic traits in paramecium gives it perhaps too great an aspect of finality. It has now been in progress only a year and is therefore properly to be considered as in its early stages. What will emerge as the study progresses cannot of course be predicted, but there is reason to hope that whatever emerges must be of interest in relation to problems of cellular heredity, differentiation, and transformation, wherever they are met in both unicellular and higher organisms.

◆

SYMPHONY AT SEA

From where I sit and see—
The sheets with reef points more like notes,
A vane which marks the mood
Of a maestro's wand—
I hear a symphony.
The gusty thumb and digits strum
The strings, the halyards and the stays.
The wand waves to and fro, fortissimo;
The mood becomes allegro
And crashing tympanists are wet
With their percussion.
On high a throated chorus
Dances in the wind
Like smoke caught in a draft.
I think the music will not end
When someone stands and shouts,
(It might be "bravo")
As clouds uncurtain jewelings on the land.
The notes die with the fluttering sheets
And lapping waves applaud politely
On the hull in port.

JOSEPH HIRSH

NUCLEAR PHYSICS AND HIGH-VOLTAGE ACCELERATORS

MERLE A. TUVE, LAWRENCE R. HAFSTAD, and ODD DAHL

Drs. Tuve, Hafstad, and Dahl, all of the Carnegie Institution of Washington, collaborated on the paper that was awarded the Annual Thousand Dollar AAAS Prize in 1930.

IN OUR paper at Cleveland (Experiments with High-Voltage Tubes, *Phys. Rev.*, 1931, **37**, 469) in December 1930, which received the AAAS prize for that year, we reported on the first production of artificially accelerated particles (electrons in the beta-ray region) and radiations having energies above one million volts (electrons in the beta-ray region and X-rays in the gamma-ray region). Lacking a better voltage source at that time, we had used a Tesla coil for the development and testing of cascade-type (multiple electrode) tubes for voltages above one million, and to carry out these measurements on high-energy radiations and particles. We also reported then the first observations on animals of whole-body lethal and sublethal exposures to gamma rays, which have been of increasing interest in recent years. We had studied the effect of large dosages of highly filtered gamma rays on rats (using 6 grams of radium at the Bureau of Standards).

Viewing the enormous progress in nuclear physics using high-energy accelerators since 1931, we feel that it is highly immodest to list even the high lights of "what has happened in the field of our paper" since we gave it. We were early workers, beginning in 1927, in a field that was enormously rich in immediate potentialities, even beyond our own recognition and great expectations. The cyclotron (1932); the first artificial transmutations (1932); the discovery of the neutron and positron and their artificial production (1932–34); the host of nuclear transmutations produced by high-energy particle accelerators (1932–39 and to date); the measurement of the primary nuclear forces (1935–37); the discovery of resonance reactions and studies of nuclear energy levels (1935–37 and to date); the work on nuclear fission, after its first glimpse using radium sources; even the atomic pile and its consequences —all were more or less direct consequences of the burgeoning enthusiasm of the early workers with high-voltage sources (1926–32), who had the clear-cut aim of producing artificial beams of high-energy particles for the direct study of the atomic nucleus. We were privileged to be among this group, but our early efforts, reported in the 1930 paper, contributed only in a very limited and modest way to the flowering of knowledge and activity in this area which began about 1932.

Our own activities during the decade of the thirties were largely focused on our original 1926 goal of studying "the simplest interactions between the primary particles at very close distances." This resulted in our measurements of the proton-proton interaction, which turned out to be very nearly the same as the proton-neutron interaction, not even visualized when we began. These interactions are the binding forces of atomic nuclei, and may ultimately be further explained in terms of mesons. We also made early studies of nuclear resonance reactions (Hafstad, L. R., and Tuve, M. A. Resonance Transmutations by Protons. *Phys. Rev.*, 1935, **47**, 506–7) and other transmutations of the light elements, and participated with our colleagues in early studies of nuclear fission and the emission of delayed neutrons (Roberts, R. B., Hafstad, L. R., Meyer, R. C., and Wang, P. *Phys. Rev.*, 1939, **55**, 664).

We like to think that our enthusiasm and that of our colleagues, especially Gregory Breit, for the possibilities of discovery and the basic value of new ideas and facts of philosophical importance in this area may have contributed in a characteristically infectious way during the early period to the initiative and vigor of some of the other workers who chose to enter the field of nuclear physics using artificially accelerated particles, but the development of this entire field since our paper of 1930 has been so vast that we cannot meet the request of the editors of this journal for a résumé of later events in the field of our paper, and must disclaim any detectable vein of influence on later developments which can be attributed to our 1930 report. We greatly enjoyed participating, however, in the rapid development of modern nuclear physics, indubitably based on the leadership of that great and humble man from New Zealand, Lord Rutherford.

THE TISSUES IN INFECTION AND IMMUNITY

REUBEN L. KAHN

Professor Kahn, University Hospital, University of Michigan, received the Annual Thousand Dollar AAAS Prize in 1933.

IF YOU have had a sore throat lately, you were probably greatly annoyed by the discomfort the infection caused you. Actually, what you experienced was an outstanding defensive maneuver of the body against infectious microorganisms. Virulent streptococci caught you off guard—you exposed yourself to them in a state of low resistance, thus enabling them to gain a foothold in your throat. Even with their initial advantage over you, your body was able to imprison them in the very area of their attack and hold them there until reinforcements, especially fluid, phagocytes, and antibodies were brought by the circulation in sufficient quantity to destroy them.

Had these microorganisms, localized in the circumscribed area in the throat, had the opportunity of breaking through the prison bars and entering the blood stream, they would have gained a vital advantage in their warfare with you. Like an enemy gaining access to the transportation facilities of a country, the microorganisms could then have established numerous foci of infection in different parts of the body. But the chances are against such an occurrence. Once in a long while large numbers of microorganisms of exceptionally marked virulence attack a host of unusually low resistance. Then the capacity of the body to keep the microorganisms confined to the area of attack may be reduced, giving the enemy the upper hand. Under ordinary conditions, however, the localizing capacity of the tissues is sufficiently powerful to keep the microorganisms confined to the area of attack.

Our ability to keep the streptococci localized in the case of a sore throat is intimately bound up with our natural immunity to these microorganisms—the result of our close contact with streptococci through the ages, in which we have been subjected to innumerable subclinical attacks by them, similar to vaccinations with minute doses of these microorganisms. These attacks have in turn called forth defensive responses, which gradually built up in us a considerable degree of natural immunity, unquestionably saving us from streptococcic infections on many occasions when our surface tissues, particularly the skin and exposed mucous membranes, come in contact with these microorganisms. When, however, owing to our lowered resistance or to the high virulence of the microorganisms or to both, they succeed in gaining a foothold in some exposed tissue of the body, the localizing phenomenon immediately manifests itself.

The localization of attacking microorganisms by our tissues in the immune state is apparently a universal law in parasitism, for we see this localization not only in human beings and in animals, but also in plants. This localization, also referred to as "retention" or "fixation," gives the microorganisms a chance to establish themselves in a tissue of a host, and it aids the host by keeping the microorganisms restricted to a local area. Whether the parasite remains chronically localized or is rapidly destroyed in the area depends on many factors, such as the virulence of the parasite, the number of parasites taking part in the attack, the resistance of the host, and the tissue involved.

The localizing phenomenon is not limited to living microorganisms. The tissues of an immune host manifest this phenomenon also against nonliving microorganisms. That dead microorganisms can replace living microorganisms in calling forth immunity was a revolutionary observation made in this country by Salmon and Smith in 1886. Pasteur had just shown that attenuated microorganisms, instead of virulent ones, could be used in immunizing chickens against chicken cholera. But the assumption was still prevalent that microorganisms must be alive in order to produce immunity. The observation of Salmon and Smith placed immunity reactions in the realm of chemical or colloid-chemical reactions. If dead microorganisms can serve as antigens in immunizing and in testing the extent of immunity in animals, it means that chemical constituents of the microorganisms, such as protein and other fractions, play a major role in immunity.

The localizing phenomenon is also manifested against antigenic substances unrelated to microorganisms, such as proteins from any source. As a matter of fact, egg and serum albumin, or protein mixtures, such as horse serum, are common antigens employed in immunity studies, both in the production of antibodies in animals and in calling forth tissue reactions. It would appear that an animal reacts to proteins and other antigens by the same defensive measures it utilizes in reacting to microorganisms.

Returning to infection in man, it is easy to understand why the capacity of the tissues in the immune host to localize attacking microorganisms is a vital defensive reaction. For, if our tissues lacked this capacity, every attacking microorganism would enter the blood stream and become widespread throughout the body. This localizing tendency persists even until death. For example, a person with tuberculosis may have but a single tubercle in the lung; or perhaps an entire lobe of a lung may be involved; or, indeed, in rare instances, an entire lung. Yet the localization of the tubercle bacilli is so powerful that the remaining tissues of the infected person might be free from tubercles. Even in miliary tuberculosis, when the tubercle bacilli have become widespread throughout the body and there is little hope for recovery, we see innumerable small, circumscribed tubercles in different tissues of the host. The tubercles are ill-formed, to be sure. They are not like the thick-walled tubercles one sees under ordinary conditions in tuberculosis, but they represent a final attempt, so to speak, on the part of the tissues to localize and wall off the invaders.

Some eighteen years ago, I became interested in this localizing, or retention, capacity of the tissues for infectious microorganisms, proteins, and other antigens. In due time I devised an experiment to determine quantitatively the localizing capacities for protein antigen of different tissues in animals in the living state. It was my hope that the experimental findings of the tissue response to protein as an antigen would enlarge our knowledge of the tissue response to microorganisms in infection and immunity. Before considering that experiment, let us briefly examine the accepted views of the role of antibodies, phagocytes, and of the tissues in immunity.

LITTLE controversy exists now with regard to the functions of antibodies and phagocytes in immunity. Sixty years ago, in 1888, Nuttall, for many years professor of biology at Cambridge University, observed that fresh blood serum possessed some germicidal powers. This discovery may be looked upon as the forerunner of our knowledge of antibodies in immunity. One of the most helpful antibodies in immunity is diptheria antitoxin. This antibody was discovered by Roux and Yersin, also in 1888, at the Pasteur Institute, by growing diphtheria bacilli in broth and injecting animals with the broth filtrates. These filtrates contained diphtheria toxin and when injected in animals called forth the production of diphtheria antitoxin. Soon Behring and Kitasato showed that diphtheria antitoxin can be employed in human beings with diphtheria; that the antitoxin neutralizes the toxin in vivo.

Most microorganisms, however, exert their harmful effects not by the production of soluble toxin, like diphtheria or tetanus toxin, but by growing in the tissues and thereby interfering with normal cell life. Many types of streptococci and staphylococci, the typhoid bacilli, and the tubercle bacilli might be mentioned as illustrations of microorganisms in this category. These and many other microorganisms in this class are known to also call forth the production of antibodies. These antibodies, furthermore, are believed to be defensive in nature, although their mode of action is not as clear-cut as that of antitoxic antibodies. The same holds true of antibodies produced in animals by the injection of protein antigens.

Turning from antibodies to phagocytes, or white corpuscles, it was Metchnikoff, the Russian scientist, who first expounded their role in immunity. Metchnikoff was a zoologist, and his interest was not limited to the white corpuscles of mammals. He was interested also in the ways a unicellular animal like the *Amoeba* handles bacteria and foreign matter. Actually, the *Amoeba* digests bacteria by intracellular digestive enzymes, and foreign particles which it cannot digest, it extrudes from its monocellular body. In mammals, Metchnikoff observed that white corpuscles have the property of drawing bacteria into their bodies and destroying them, presumably also by digestive processes.

Metchnikoff's view that the white corpuscles are the soldier cells of the body was at first severely criticized by his colleagues, but ultimately it became the forerunner of the theory of phagocytosis. This phenomenon has come to embrace not only the role of white corpuscles, which are mobile and can rapidly accumulate in any area in the body where microorganisms open attack, but also the role of fixed phagocytes known as the reticulo-endothelial system, which includes numerous specialized cells, especially in the spleen and liver, which exert phagocytic powers not only against microorganisms but also against foreign particles. Phagocytosis is referred to as the "cellular" concept of immunity in contradistinction to the role of antibodies in immunity, which is referred to as the "humoral" concept.

Half a century ago, there was high controversy between these two concepts of immunity. This controversy was not free from international implications. The French school of workers in immunity, under the leadership of Metchnikoff at the Pasteur Institute, promulgated the view that phagocytes carry the burden of defense. The German school, under the leadership of Ehrlich, of Salvarsan fame, who fathered the side-chain theory of antigen-antibody reactions, insisted that antibodies carry the burden of defense. It was during this controversy that Metchnikoff made the famous remark, "Any German who does not accept Ehrlich's side-chain theory is damned."

Little is heard of that controversy among present-day immunologists. It is now generally accepted that both the antibodies and the phagocytic cells play important parts in protecting us against bacterial invasion and in defending us when microorganisms succeed in gaining a foothold in the body. When it comes to the function of the tissues in immunity, controversy again runs high. Textbooks of bacteriology and immunology do not generally even discuss tissue immunity; they present chapters only on tissue hypersensitiveness. As is well known, the defensive role of the fluids of the body is covered in these textbooks by chapters on "Antibodies" or on "Humoral Immunity;" the defensive role of the phagocytes, by chapters on "Phagocytosis;" and the role of the tissues in immunity, by chapters on "Tissue Hypersensitiveness."

The concept of tissue hypersensitiveness had its origin in "anaphylaxis," a term coined by Charles Richet, who was professor of physiology in the Medical Faculty of Paris, in 1902. The term anaphylaxis, "against protection," as distinguished from prophylaxis, or "for protection," was first employed in an article by Richet and Portier entitled "The Anaphylactic Action of Certain Poisons," which appeared on February 15, 1902, in the *Bulletin of the French Biological Society.* Richet and Portier reported that repeated injections in dogs of a glycerin extract of the tentacles of Actiniaria, which is poisonous, causes the animals to become sensitized instead of immunized to the poison. The accepted view had been that repeated injections of an injurious substance tended to produce tolerance or immunity to the substance. But here was evidence that repeated injections of a poison produced a condition of hypersensitivity.

Anaphylaxis has been investigated especially in guinea pigs. Theobald Smith in 1904 reported that guinea pigs often died suddenly after a second injection of horse serum given directly into the blood stream, and this experimental occurrence was for a number of years referred to as the "Theobald Smith Phenomenon." The death of the guinea pigs was due to asphyxia resulting from the acute spasm of the muscular coat, and the consequent valvelike closure, of the smaller bronchi. The important fact, however, was that the animal died with great suddenness following a second injection of the antigen in the blood stream.

One year after the report on anaphylaxis, M. Maurice Arthus, in an article entitled "Repeated Injections of Horse Serum in the Rabbit" (*Compt. Rendus Soc. de Biol.*, 1903, **55**, 817), claimed to have corroborated the findings of Richet and Portier. This author observed that a first injection of horse serum in the rabbit, given by any route, has no harmful effect on the animal. But repeated injections, if given under the skin, produce local inflammatory reactions, with tissue necrosis. Arthus interpreted these reactions to be manifestations of local anaphylaxis.

These experiments by Richet and Arthus attracted a great deal of attention, and gradually the belief became prevalent that the tissues in infection and immunity apparently do not act in harmony with antibodies and phagocytes in defense of the host but that they become hypersensitive. To correlate tissue hypersensitiveness with humoral and cellular immunity in an infection, such as a sore throat, is to assume that antibodies and phagocytes are lined up against the streptococci in behalf of the host, whereas the tissue cells under attack in the throat are in a state of "against defense" and are lined up against the host in behalf of the streptococci.

Because it did not seem possible that the body under attack by microorganisms is divided against itself, I was led to undertake studies of the meaning of tissue hypersensitiveness. My first series of articles was published under the general heading of "Studies on Sensitization." In due time, these studies began to indicate that the tissues react in harmony with antibodies and phagocytes in defense of the host, and I changed the general heading of the articles to "Tissue Reactions in Immunity." Finally, I felt so confident of my premise of the defensive nature of the tissues in infection and immunity that I entitled my summation of these studies "Tissue Immunity" and not "Tissue Hypersensitiveness."

Our findings indicate that the sudden death of the guinea pig following a second injection of antigen in the blood stream, as well as the local inflammatory reaction in the skin of the rabbit to repeated injections, does not justify an assumption that the tissues in infection and immunity under natural conditions are in a state of hypersensitivity. They indeed indicate that the tissues, the antibodies, and the phagocytes work in harmony in defense of the host. This view at once leads us into depths of controversy. Some authors accept the classical interpretation of tissue hypersensitiveness according to Richet and Arthus. Some maintain that hypersensitiveness is a phase of immunity and that it makes little difference what terminology one uses. Others look upon hypersensitiveness and immunity as two distinct coexisting phases in infections, such as in tuberculosis. Then, too, allergy in man, so intimately tied in with tissue hypersensitiveness, needs to be considered. It was in 1906 that the term "allergy" was created by von Pirquet in Vienna. Originally, it was intended to mean altered tissue reactivity, but the term has become a synonym for tissue hypersensitiveness.

With regard to the experiments to be described, no claim is made that they will give all the answers to the question of the role of the tissues in infection and immunity. Available knowledge of immunity is altogether too limited to give adequate answers to any comprehensive questions in this field. The claim is made merely that any concept that attempts to explain anaphylactic shock obtained under special conditions of injection actually can be said to apply only to parallel experiments carried out under the same conditions of injection. Since there is no record of any guinea pig or any other animal dying from anaphylaxis under natural conditions, any assumption that the tissues under natural conditions of infection and immunity are in an anaphylactic or hypersensitive state is beyond experimental indication.

Little need be said about the explanation of Arthus that the local inflammatory reaction in the skin of the rabbit to repeated injections of antigen is a local anaphylactic reaction. Many workers in immunity look upon this skin reaction as a defensive reaction, although some hold onto Arthus' concept that the rabbit, like the guinea pig, is in a state of tissue hypersensitivity as a result of the repeated injections.

Let us now turn to the experiments carried out in my laboratory on the localizing capacity of the tissues in immunity.

As ALREADY emphasized, when an animal possesses immunity to microorganisms, the tissues possess the capability of localizing them in the area in which they gained entry. Similarly, when an animal is immunized to a protein antigen, it will localize this antigen in the area of injection. The localizing mechanism developed against living microorganisms is apparently applied to protein and other antigens.

An experiment in which the localizing capacity of the skin of immunized animals is determined quantitatively employs a method, developed in my laboratory, which enables us to measure this capacity in terms of localizing, or retention, units. The localizing capacity of the skin of nonimmunized controls is then determined for comparison.

In this experiment, rabbits are immunized with horse serum. However, the use of horse serum in the quantitative determination of the localizing capacity of the skin of these animals would prove inadequate. Instead, therefore, a type of horse serum is employed which is quantitatively standardized on a unit basis. Actually, horse serum diphtheria antitoxin, standardized on the basis of neutralizing units of diphtheria toxin, is employed in testing the skin's localizing capacity.

The injection of antitoxin in the skin of these animals and the determination of the extent to which it is localized in the area of injection necessitate that they be injected also with a given dose of diphtheria toxin. Let us suppose that the antitoxin is injected in a given area in the skin of a rabbit that had been previously immunized with horse serum, and that diphtheria toxin is injected in another area. The skin will localize the injected antitoxin and prevent it from reaching the blood stream. That will mean that the diphtheria toxin will reach the blood stream and exert its toxic action unhampered. Suppose, however, the antitoxin is injected into the skin of a nonimmunized rabbit under the same experimental conditions. The antitoxin will reach the blood stream rapidly, neutralize the injected toxin, and thus prevent toxic action.

In an actual experiment, rabbits *A* and *B* are immunized to horse serum. Rabbits *C* and *D* are nonimmunized controls. All four rabbits are injected with horse serum antitoxin in the skin and, simultaneously, with a lethal dose of diphtheria toxin. The skin of the nonimmunized rabbits, having little capacity for localizing the injected antitoxin, will permit it to reach the blood stream without delay and thereby neutralize the diphtheria toxin. The skin of the horse serum-immunized rabbits, having a marked capacity for localizing the horse serum antitoxin, will prevent it from entering the blood stream and neutralizing the toxin. The nonimmunized control rabbits will thus survive, and the rabbits immunized to horse serum will succumb to the diphtheria toxin.

The following results were obtained under experimental conditions in which rabbits *A* and *B* were given two immunizing injections of horse serum, *C* and *D* no injections of horse serum, and all rabbits were given the same dose of diphtheria toxin (50 minimal lethal doses, 250-gr guinea pig).

Rabbit *A* was not protected from toxin death by 1,000 units of antitoxin injected into the skin, indicating that this amount of antitoxin was localized in the skin and was thereby prevented from reaching the blood stream and neutralizing the toxin.

Rabbit *B* was protected from toxin death by 1,500 units of antitoxin injected into the skin, indicating that this amount of antitoxin exceeded the localizing capacity of the skin, spilled over into the blood stream, and neutralized the toxin.

Rabbit *C* (control) was not protected from toxin death by 15 units of antitoxin injected into the skin, indicating that this relatively small amount of antitoxin was localized in the skin of the nonimmunized animal, and was thereby prevented from reaching the blood stream and neutralizing the toxin.

Rabbit *D* (control) was protected from toxin death by 20 units of antitoxin injected into the skin, indicating that this amount of antitoxin exceeded the localizing capacity of the normal skin, spilled over into the blood stream, and neutralized the toxin.

Here we have an illustration of the marked localizing capacity which the skin acquires as a result of a relatively limited degree of immunization. By arbitrarily considering the antitoxin units localized as retention, or localizing, units then:

1) The skin of a nonimmunized rabbit possesses a localizing capacity of 15–20 units.
2) The skin of a rabbit previously immunized by two injections of the antigen possesses a localizing capacity of 1,000 to 1,500 units.

TABLE 1

Localizing Capacity, Expressed in Units, of 13 Different Tissues in Immunized and Nonimmunized Animals*

Tissue	Immunized Animals	Nonimmunized Animals (Controls)
	Localizing Units	
Skin	1,000–1,500	15.0–20.0
Under skin	750–1,000	15.0–20.0
Peritoneal tissue	750–1,000	5.0– 7.5
Lung	400– 500	2.5– 5.0
Spleen	300– 400	2.5– 5.0
Uterine wall	200– 300	2.5– 5.0
Popliteal gland	200– 300	5.0–10.0
Testicle	200– 300	5.0–10.0
Liver	150– 200	2.5– 5.0
Articular space	150– 200	5.0–10.0
Skeletal muscle	75– 100	5.0–10.0
Brain	75– 100	5.0–10.0
Blood stream	50– 75	2.5– 5.0

* The rabbits were immunized by two injections of horse serum about one week apart.

Our experimental records show that if rabbits are immunized by 4 or 5 injections of horse serum instead of 2 injections, the skin of these animals will localize as many as 3,500 units of horse serum antitoxin, possessing what might be considered 3,500 localizing units.

The same experimental approach made possible the quantitative determination of the localizing capacities of 13 different tissues of nonimmunized and immunized animals (Table 1 and Charts 1 and 2). For reasons of simplicity, in order to reduce the localizing capacities of different tissues to single figures, the retention units for the tissues in the charts are based on the number of antitoxin units injected in each tissue that protected the animal against the lethal dose of toxin. The number of antitoxin units that did not protect the animals are the lower figures given in the table. It is evident from the table and charts that in nonimmunized animals the units of retention range from 5 to 20; in the immunized animals, the range is from 75 to 1,500.

The skin shows the greatest localizing capacity, probably because through evolutionary ages this tissue has been exposed to microorganisms. As a result, it has built up a marked capacity to localize them and thereby prevent them from entering the blood stream. This same localizing capacity is evidently manifested against protein antigen. The exposed mucous membranes apparently also show a marked localizing capacity. Studies of the localizing capacity of the conjunctiva indicate that this tissue shows a localizing capacity similar to that of the skin. The intraperitoneal tissues show an extensive localizing capacity, possibly because of the relatively large surface exposure to which the injected antigen is subjected.

It is of interest that of the internal organs studied, the lung shows the next marked localizing capacity, with 400–500 retention units, possibly because it is, in part at least, an exposed tissue. Skeletal muscle shows a relatively low retention capacity, with 75–100 retention units, and the same applies to brain tissue. The retention units are lowest, only 50–75, when the antigen is injected in the blood stream. This finding is also not without interest, since, when the antigen is injected in the blood stream of a nonimmunized animal, the retention units are only 2.5–5.

This tissue immunity experiment is presented to emphasize the importance of localization in immunity and to stimulate an interest in the study of individual tissues in immunity. The method employed is limited in its application. Yet it has brought to light, in addition to the data presented in

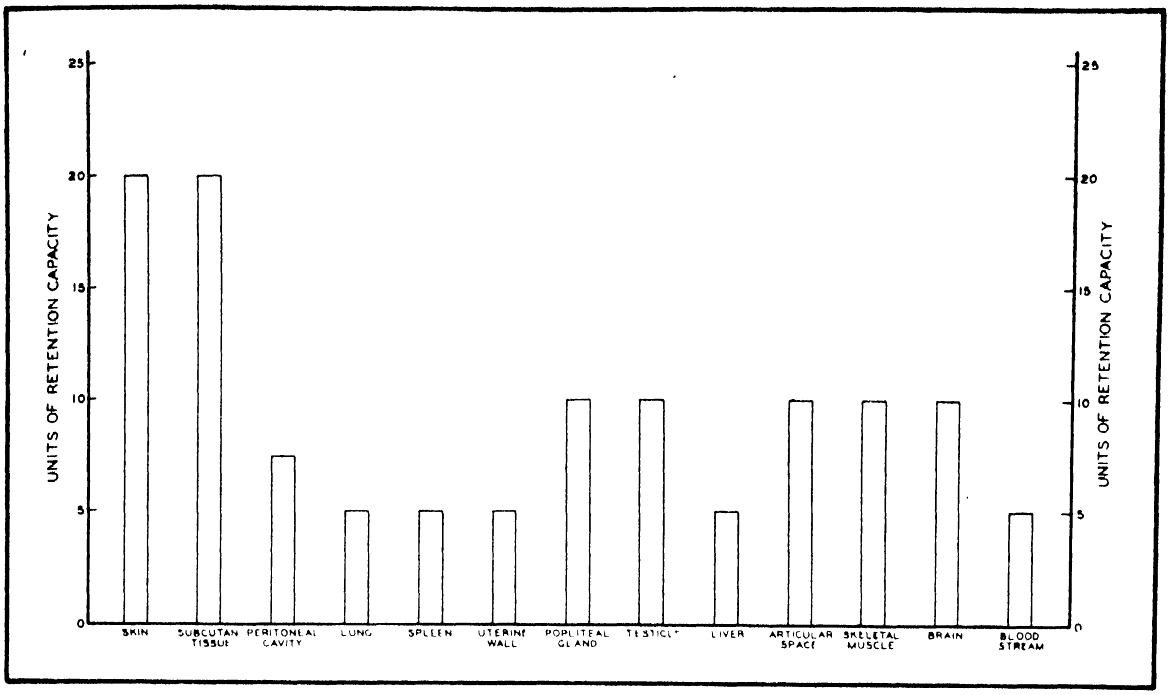


Chart 1. Retention, or localizing, capacity of different tissues of nonimmunized rabbits for protein antigen (showing a range of 5–20 units).

the table and charts, some experimental results of considerable interest. For example, the method has shown that the localizing phenomenon in the skin can be detected 48 hours after an immunizing injection. Since antibodies become detectable in about a week after an immunizing injection, it would

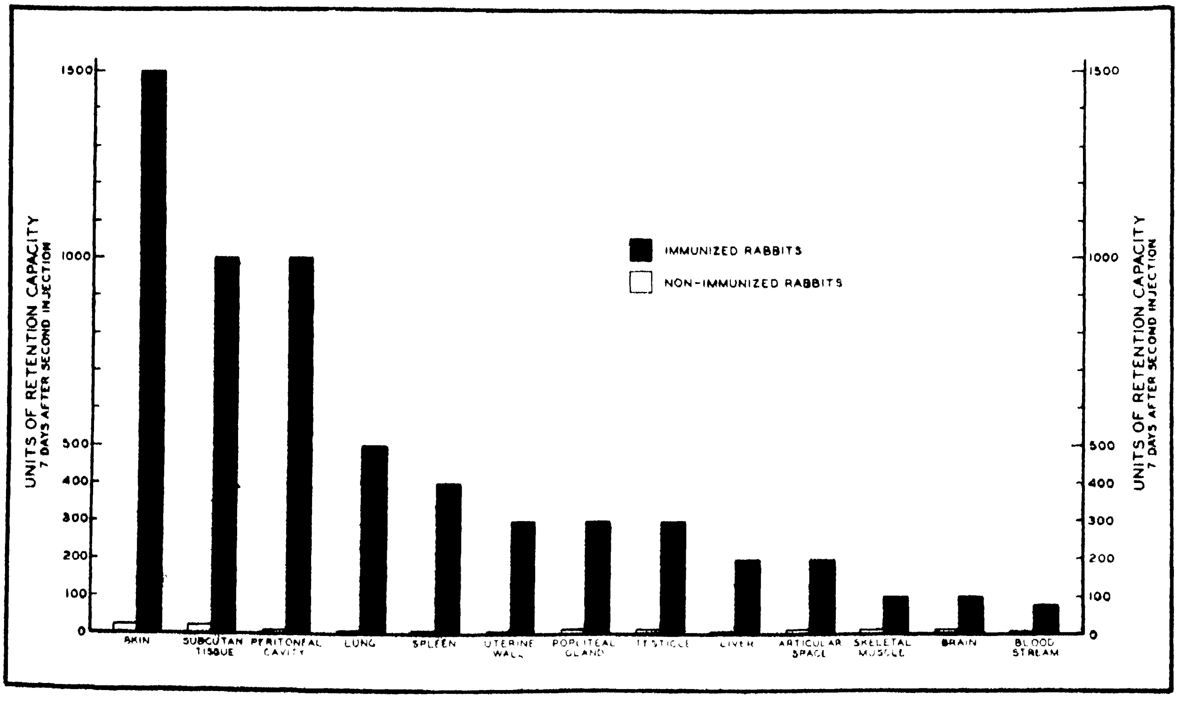


Chart 2. Retention, or localizing, capacity of different tissues of rabbits for specific antigen following the injection of two immunizing doses of protein antigen (showing a range of 75–1,500 units).

appear that the localizing phenomenon is perhaps one of the first manifestations in immunity.

The method has brought to light additional data with regard to the localizing phenomenon. For example, it showed that the tissues of young animals do not possess as marked localizing powers in immunity as the tissues of adult animals, which helps to explain the greater susceptibility of the young to infection. Also, when nonimmunized animals are injected with antibodies from immunized animals and are thereby passively immunized, the localizing capacities of their tissues do not show the same pattern as in actively immunized animals. The localizing capacities of the different tissues in the passively immunized animals are relatively similar in range. Thus, although the localizing capacities of the skin and skeletal muscle in these animals are increased over the capacities in nonimmunized animals, the localizing capacity of the skin is only about twice that of muscle, the same ratio as in nonimmunized animals. In actively immunized animals, however, as we have seen, the localizing capacity of the skin is more than ten times that of muscle.

This finding would suggest that the localizing capacity of the tissues in immunity is a function that manifests itself over and above circulating antibodies. What it is desired to emphasize, however, is that the study of the localizing capacities of different tissues in different animals under various conditions of immunization by the method herewith described opens up a highly promising field for the study of immunity. The method is an elaborate one: the data in the table and charts required some 200 rabbits. But if we are to fully understand immunologic reactions to microorganisms and other antigens, we must understand not only the reactions of the body as a whole but also of the individual tissues.

TURNING to the results in the tables and charts, it cannot be said that clinicians have not recognized differences in the immunologic response of different tissues. Suppose a person has a staphylococcus infection of the skin, commonly called a boil. Then suppose another person has an infection in muscle by staphylococci of the same virulence and the same number, an infection commonly referred to as an abscess. It is a matter of common knowledge to clinicians that the abscess is of far greater danger, for it might mean the establishment by the staphylococci of other foci of infection in different parts of the body. In the case of the boil, this possibility is altogether unlikely.

When we recall that the localizing capacity of skeletal muscle is only about one tenth that of the skin, it would be expected that the chances of escape by the staphylococci from the muscle infection would be far greater than the chances of escape from the skin. One must take into consideration, of course, that the skin infection is surrounded by a relatively thick fibrous wall, whereas the muscle infection does not have so pronounced a wall, which again emphasizes that different tissues play different roles in immunity.

After giving so much emphasis to the localizing capacities of the tissues in immunity, it might perhaps be well to consider briefly the tissues in the absence of immunity. According to the table and Chart 1, the localizing capacities of the tissues of the nonimmunized animal are apparently very slight, or perhaps negligible. This is evident not only from experimental findings in the laboratory but also from clinical findings in human beings. For instance, human beings lack immunity to the spirochete *pallidum*, the causative organism of syphilis. Hence, when the spirochetes gain entry into some area of the skin or mucous membrane, they soon reach the blood stream. A period of incubation then follows when there is no indication in the local area that a highly virulent microorganism has gained a foothold in the body.

The same holds true of tularemia. Human beings lack immunity to this disease. The result is that when the *tularense* microorganisms gain entry at some point in the skin, they are not localized at that point but soon reach the blood stream. What often happens is that a butcher will contract the infection while skinning an infected jack rabbit. He may have a scratch on his hand that facilitates his getting the infection. As the microorganisms gain entry through the scratch, there is at first no local nor systemic indication that highly virulent microorganisms have worked their way into the body.

Taking a third illustration, we might inject tubercle bacilli under the skin of a guinea pig. This animal lacks natural immunity to these bacilli, and when they are injected they rapidly escape from the area of injection and reach the blood stream.

Briefly, the tissues of a nonimmune host are unable to localize microorganisms or protein antigens and prevent them from entering the blood stream and becoming widespread throughout the body. This lack of the localizing capacity, however, is relative and not absolute. In relation to the marked localizing capacity of the tissues in immunity, the localizing capacity in the absence of immunity is apparently negligible. Of great interest is the fact that during the period of incubation in infection and immunity, virulent microorganisms can circulate in the blood stream without apparent harm to the host.

When it comes to the question of how the tissues in immunity localize a specific antigen, perhaps a wise answer would be "I don't know." For we are entering here into the realm of theory, and sometimes good data are "spoiled" by wrong theories. I have been impressed at times by the view that a scientist should present data and omit explanatory theories; for the data will live—the theories might die the next day. At other times, I have been impressed by the view that a scientist must present theories explanatory of his data, else they are uncorrelated facts and, like the pieces of a jigsaw puzzle, meaningless—that a wrong theory is better than no theory. It might be best, therefore, to consider some theories.

Before presenting my own theory of the basis of localization, I wish to present the theories of two distinguished American pathologists. Opie, who has investigated the localization of injected protein in the skin of specifically immunized rabbits, came to the conclusion that the protein combines locally with circulating antibodies (Inflammation and Immunity, *J. of Immunol.,* 1929, **17**, 329). The end result of this combination is the formation of a precipitate in vivo, just as a precipitate is formed when antigen and antibody are brought together in vitro. Opie believes that the union of antigen and antibody in the area of injection is toxic to the tissue and the resulting injury calls forth a local inflammatory reaction.

This fact should be emphasized: namely, when localization of antigens or of microorganisms takes place in a tissue of an immunized animal, a local inflammatory reaction occurs. Menkin (*Dynamics of Inflammation.* New York: Macmillan, 1940), who has extensively studied many aspects of inflammation, believes that it is the local inflammatory reaction that is responsible for the localization of the antigen. It is known that inflammatory tissue possesses marked localizing capacity, not only for specific antigens but also for inert substances, such as dyes. Menkin's explanation overlaps to some extent that of Opie. The antigen is held in the area of injection by its union with circulating antibodies. The toxic effect of this union produces local tissue injury and, in turn, an inflammatory reaction. The inflammatory tissue then intensifies the localizing response.

Not being a pathologist, I should not presume to question the views of two distinguished workers in a tissue response associated with inflammation. To me, however, immunity is a physiologic function of all cells, and my experiments have been carried out only from this point of view. If, therefore, I must encroach upon the field of pathology by touching upon the inflammatory reaction to a specific antigen in immunity, my aim is to consider this reaction only in its broadest sense.

Actually, my experiments do not question the generally accepted view that inflammatory tissue possesses a marked capacity for localizing specific antigen and nonspecific substances. I differ from Opie and Menkin only in the question of the first and initial step in localization. My experiments indicate that the first step in localization is the result of some colloid-chemical union between the tissue cells and the antigens; that this union is similar to that which occurs between white corpuscles and bacteria in phagocytosis and between antigen and antibody preliminary to agglutination and precipitation in the test tube.

Briefly, I believe that as a result of immunization, all the body cells develop the property of identifying the specific antigen and of entering into union with it. Certain cells, such as reticulo-endothelial cells, possess this property to a greater extent than other cells. The cells of the exposed tissues of the body possess this property to a marked degree, but all body cells possess it to some degree.

Hence, when we inject microorganisms or other antigens into a tissue of an immunized animal and the injected material is localized or retained or fixed—to use different nomenclatures—in the area of injection, we see the end result of colloid-chemical union between the local tissue cells and the injected material. The same holds true of infection under natural conditions. If I, who possess natural immunity to streptococci, develop a streptococcic sore throat, it means that the surface cells of the throat, having come into contact with these microorganisms, have entered into chemical union with them and, as a result, localized them in such a way that they cannot enter the blood stream and produce a septicemia.

When tissue cells enter into chemical union with microorganisms or antigens and hold them localized, what effect might such union and localization have on the cells? The cells are bound to become injured, since their normal processes are interfered with. Just as the phagocyte, by holding bacteria in its body, pays with its life to protect the host, the tissue cells which hold microorganisms fixed to prevent them from entering the blood stream and becoming widespread evidently undergo injury and even death to protect the body as a whole.

The moment tissue injury occurs in a given area in the body, a chain of events takes place aimed at destroying the substance or substances responsible for the injury and at healing of the injured tissue. Inflammation, which includes the dilation of the capillaries in the area, the accumulation of fluids and phagocytes, and the formation of a fibrous wall to circumscribe the area, is thus believed to be the sequel to the localization of microorganisms or antigen in a tissue of an immunized host.

Inflammatory reactions, occurring in connection with the localization of antigen, show different characteristics. For example, let us say we have before us a rabbit immunized by several injections of horse serum. The question is, What type of inflammatory reactions will the animal show to the injections of 0.01 cc, 0.1 cc, and 1 cc of horse serum? On injecting these amounts in different areas under the skin, it is found that 0.01 cc causes a small, slightly raised inflammatory area which reverts to normality in 48–72 hours; that 0.1 cc results in a larger inflammatory area which takes about a week to revert to normality; and that the 1-cc amount calls forth not only a relatively large inflammatory area, but the center of this area becomes black, indicating the cutting off of the blood supply, with resulting death of tissue. It takes some months before this area is fully healed.

The relationship between an inflammatory reaction, which pathologists have long emphasized to be a defensive response of the body to tissue injury and inflammation with tissue necrosis, interested me as a student of immunity. The death of the tissue of an immunized animal right in the center of the defensive inflammatory reaction seemed at first difficult to explain immunologically.

Let us then re-examine this experiment. The injection of 0.01 cc and 0.1 cc of horse serum under the skin of the immunized rabbit called forth local, raised inflammatory areas without any indication of tissue necrosis. Studies of the fate of the injected horse serum indicate that it is destroyed by the inflammatory reaction. When the inflammatory reaction subsides and the areas assume normality, the injected horse serum has disappeared. These results must mean that the animal was sufficiently immunized to call forth a defensive inflammatory reaction of such strength as to completely destroy these small amounts of horse serum and to heal the injured tissue.

Suppose the inflammatory reaction mustered by the immunized animal is insufficiently strong to destroy the injected horse serum? Undestroyed horse serum will then remain within the inflammatory area. The picture then changes from increased blood volume, associated with inflammation, to the cutting off of the blood supply, presumably by the clogging of the blood vessels with thrombi. Instead of a raised inflammatory area, a sunken-tissue necrotic area is noted.

This is what happened when, in our experiment, we injected an entire cubic centimeter of horse serum under the skin of the rabbit. The rabbit's immunity, strong enough to call forth inflammatory reactions to destroy 0.01 cc and 0.1 cc of injected horse serum, was not strong enough to destroy 1 cc, and the undestroyed horse serum within the inflammatory area changed the defensive inflammatory reaction to a local tissue necrotic reaction.

I have published three simple methods for changing a defensive inflammatory reaction to the injection of an antigen in an immune animal into a tissue necrotic reaction. We have just considered Method 1. Method 2 consists of calling forth a local inflammatory reaction in the skin of an immunized rabbit to the injection of a specific antigen, and then injecting a trace of the same antigen within the inflammatory area. The point where the specific antigen is injected will become purple and then black in a matter of minutes, indicating death of tissue. Method 3 consists of again injecting a relatively small amount of antigen in a tissue, such as the skin, of an immunized rabbit. An inflammatory reaction will appear at the site of the injection. A relatively large amount of the antigen is then injected into the blood stream. The attraction which the local inflammatory area exerts on the circulating antigen results in the accumulation of the antigen within the inflammatory area, and, when the amount accumulated becomes greater than the inflammatory reaction can destroy, tissue necrosis will set in within the inflammatory area.

These three methods are readily applicable to bacterial suspensions. For example, an injection of a small dose of vaccine of colon bacilli into the skin of a rabbit will call forth a raised inflammatory area which will subside to normal in a few days. A large dose of the same vaccine will result in an inflammatory reaction with central tissue necrosis. An inflammatory reaction called forth by a small dose of the vaccine will change to a tissue necrotic reaction by the injection of sufficient vaccine in the blood stream. Also, an inflammatory reaction resulting from a small dose of the vaccine can be changed to a tissue necrotic response at a given point if a small amount of the vaccine is injected directly into the inflammatory area.

This change from inflammation to tissue necrosis becomes understandable by introducing teleologic reasoning. In the struggle between a host and microorganisms, the host is on top, so to speak, as long as his defensive mechanisms can localize and destroy the attackers. If the defensive mechanisms are incapable of destroying the microorganisms, the latter gain the upper hand in the local area, and, as a result, they destroy the tissues of the host. Since chemical antigens exhibit immunologic characteristics similar to those of living microorganisms, we see the same relationship between inflammation and tissue necrosis applied to protein antigen, such as horse serum. Briefly, an inflammatory reaction in immunity is a defensive reaction of a host against microorganisms (or antigens), and tissue necrosis within such an inflammatory reaction may be looked upon as a defensive reaction of the microorganisms (or antigens) against the host.

I HAVE tried to emphasize that when we lack immunity to given microorganisms or to protein antigens, our tissues lack the capacity to localize them to any degree in the area of contact. The result is that they gain entry into the blood stream in a matter of minutes. One of the first manifestations of immunity is the development of the capacity to localize the specific microorganisms or antigens in the area of contact, with the result that they do not gain access to the blood stream. The cutaneous tissue possesses a localizing capacity above that of other tissues, a situation which is understandable, since the greater the localizing capacity of the surface tissue, the greater the protection of the animal from blood-stream invasion.

Localization is a widespread response to microorganisms under natural conditions in infection. As long as the infected person can keep the microorganisms localized, provided they do not produce soluble toxin, he has the upper hand. When he loses this localizing capacity—and this happens when an infected person becomes overwhelmed by the microorganisms—death may ensue.

Localization, although protective to the body as a whole, is injurious to the local tissue, just as phagocytosis is protective to the body as a whole but injurious to the phagocytes. When a tissue in a given area localizes microorganisms, it holds onto them, presumably by colloidal-chemical union. Such union interferes with the physiologic function of the area and leads to injury of the local cells. The body then rushes aid to the injured cells by means of a local inflammatory reaction. The fluid and phagocytes that accumulate in the area are often sufficient to destroy the microorganisms or antigen; then healing of the local area takes place. If the inflammatory reaction is too weak to cope with the microorganisms or antigen, the situation is reversed in favor of the microorganisms or antigen, just as in warfare, if, in a given zone, the defensive efforts are incapable of destroying the entrenched enemy, the enemy causes the destruction instead.

With this approach to the phenomenon of localization and its aftermath, inflammation, let me describe an experiment on anaphylaxis in the guinea pig. A small amount of protein, such as horse serum, is injected in a guinea pig. The animal will show no apparent effect, since it possesses no natural immunity to the horse serum. Several weeks later a small amount of horse serum is injected directly into the blood stream. The animal almost immediately gives evidence of shock; it breathes with great difficulty and it dies from asphyxia within a few minutes.

How would you and I have interpreted this experiment? We would probably have assumed that the first injection of the horse serum in the guinea pig had immunized the animal. This immunity led, of course, to the development of the localization capacity for horse serum so as to prevent it from entering the blood stream. Hence, the injection of the horse serum directly into the blood stream antagonized this localizing mechanism and led to immunologic shock.

We would have concluded that the guinea pig's immunologic make-up is so delicately adjusted that it cannot be tampered with experimentally; that one cannot, by means of a syringe and needle, shoot antigen directly into the blood stream of that animal after building up immunologic mechanisms to prevent the antigen from reaching the blood stream. The animal's response is very much like that of a nation which, before the days of the airplane has built up an almost impregnable wall to prevent an enemy from gaining entry, and then suddenly discovers that the enemy has found its way into the very heart of the country. Shock and havoc would be bound to result.

Evidently, that country with the defensive wall which surrounds it was not in a state of the opposite of defense; namely, on the enemy side. It was in a defensive state, but its defense was not adequate to cope with an unexpected emergency situation. The same situation, it would seem, applied to the guinea pig. Its defensive mechanisms had been developed to cope with natural immunologic conditions, and it apparently lacked provisions to defend itself against this particular laboratory experiment. Immunized animals other than guinea pigs and human beings apparently have provisions to cope with injections given directly into the blood stream. This is very fortunate, especially in the case of human beings, since, in diagnosis and therapy, methods of injection have become so widespread a practice.

If, then, there had been no concept of anaphylaxis, or "against defense," Arthus, very likely, would not have proposed a concept of local anaphylaxis to explain the local inflammatory reaction in the rabbit to repeated injections of horse serum. For the localization of the antigen in the area of injection and the subsequent inflammatory reactions are obviously defensive mechanisms.

Without traditional concepts of anaphylaxis and of local anaphylaxis, today's textbooks on immunology would probably present discussions of tissue immunity side by side with humoral and phagocytic immunity, emphasizing the defensive reactions of the tissues under natural conditions of infection and immunity in harmony with the reactions of defensive antibodies and phagocytes. Then there would be a presentation of the responses of a host to injections under various conditions, perhaps divided into two categories: the responses that parallel natural conditions of infection and immunity and the responses that do not parallel these conditions. It is in the latter section that the behavior of the guinea pig and of other animals—and indeed the behavior of man—to various forms of injection which result in reactions that occur only under conditions of injection would be considered. Then, finally, there would be a section on disturbances in the immunologic function, in which the allergies of man would be discussed.

With regard to allergy, in *Tissue Immunity* (Springfield, Ill.: Charles C Thomas, 1936, 681), I stated:

> Neither immunity nor allergy, in our opinion, can be fully understood without recognizing the tremendous burden of defense carried by the exposed tissues of the body. Through evolutionary ages these tissues have had the task of warding off microorganisms, and preventing their entrance into the blood stream and into the deeper tissues. Is it not significant then that these surface tissues should be largely involved in allergic disturbances of man? It is this great burden that these tissues carry, it seems to us, that is related to these disturbances. Like over-vigilant guards, the surface cells begin to treat harmless substances as though they were harmful microorganisms, and react with great intensity on slight provocation.

It would appear that the allergic person suffers from hyperactivity of the immunologic function. Hyperactivity of a physiologic function is one of the most common disturbances of man. Excessive activity of the thyroid gland (hyperthyroidism), excessive secretion of acid in the stomach (hyperacidity), and high blood pressure (hypertension) might be mentioned as examples. The allergic person is generally "hyperimmune" to a particular pollen or to some other substance, just as the immune person may possess immunity to one particular strain of microorganisms and not necessarily to others.

If I may look into the future, I would say that the immunologic manifestations of the individual tissues in relation to those of the body as a whole, and the factors that influence these manifestations, will be studied to an increasing extent. The relationship between the anatomic structure of individual tissues and their immunologic function has hardly been explored. Future students of immunity will wish to know more fully the anatomic bases for the immunologic differences of the various tissues. They will want to understand more fully the immunologic capabilities of the different tissues, as well as their immunologic disturbances, such as those manifested in the allergies. Infections and the allergies occur primarily in certain tissues; hence, the more that is known about the immunologic function of these tissues and the functions of antibodies and phagocytes, the better we shall be equipped to cope with these disturbances.

⁂

AN IDEAL PARTNERSHIP

L. R. CLEVELAND

Professor Cleveland, of the Biological Laboratories, Harvard University, received half the Annual Thousand Dollar AAAS Prize in 1924.

ANIMALS, like humans, form good, bad, and indifferent partnerships. Perhaps the best-known example where both partners derive more or less equal benefit from living and working together is that of termites, or white ants, and one-celled flagellate protozoa which live in their insect partner. This partnership is a very old one. It existed in certain wood-feeding roaches, or Blattidae, before termites were given off from this ancient group of insects, and it still exists in them today very much as it did 200–300 million years ago. It has changed little if at all since termites diverged from roaches prior to the Tertiary.

Some roaches, however, formed a partnership with bacteria, and this partnership, like that with protozoa, was probably passed on to termites in the course of evolution. In other words, the protozoan partners were not replaced by bacteria. So much more is known regarding the partnership of insects and protozoa that the partnership between bacteria and insects will not be considered here—although if better known, it might be very interesting. Since the partnership between protozoa and roaches is older and more nearly perfect from the standpoint of both partners, more attention will be given it.

Both roach and protozoa use the same food—wood. Almost any kind of wood, from hard and sound to soft and quite rotten, can be used; the main requirement is that it be fairly moist. The roach *Cryptocercus punctulatus* lives deep within the wood that it eats, in tunnels just wide enough for its body. All traffic is one-way. Movement is in, never out, until most of the log is consumed or until unfavorable conditions of one kind or another make it necessary to look for a new home. There is no housing shortage.

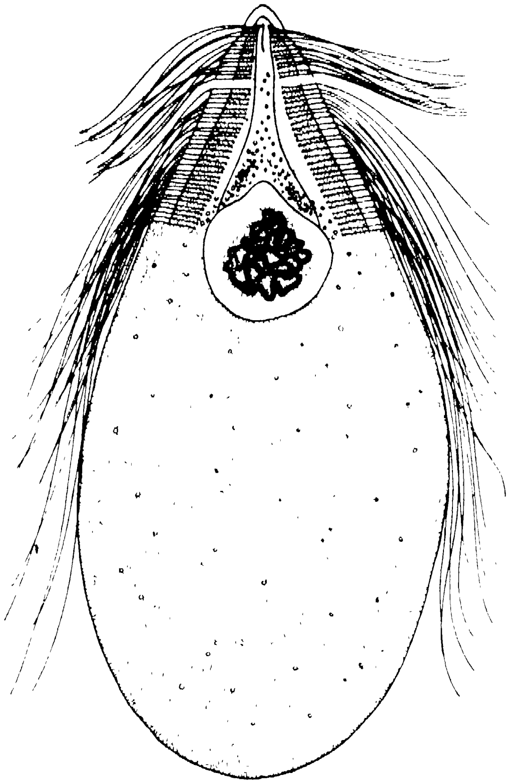
In such an environment, the roach, like the protozoa within it, lives in darkness. It comes out into the light only when searching for new and better living quarters; this, on the average, happens perhaps once every ten or fifteen years. Such secluded conditions assure both partners almost complete freedom from enemies. This, together with an abundant food supply and virtually no competition, would seem to offer important population control problems. Long ago, this was probably true, but, because of certain well-established adaptations made by both partners, the problem no longer exists.

The roach eats the wood and passes it on to the protozoa, which live only in the greatly enlarged hind portion of its intestine. The protozoa are so thick that room for more is made only when death occurs; medium-sized ones fit around big ones, and small ones around medium-sized ones, like so many watermelons, apples, and grapes in a bin. The roach has mouth parts which cut the wood into particles small enough for the one-celled protozoa to take into their bodies. If a few particles too large for the protozoa to take in should be swallowed, they never reach the protozoa because en route the roach has a screen in its crop for holding them back. There are three other valves elsewhere in the roach's intestine, each serving a different function in the partnership.

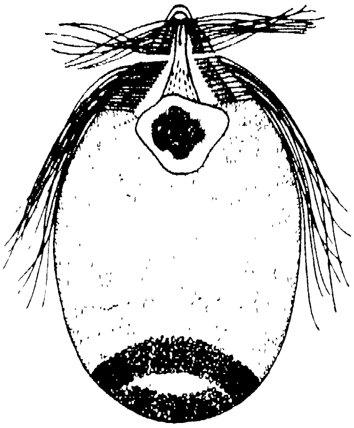
Even though this partnership has persisted for a very long time—possibly longer than any other—the two partners, in certain respects, still differ greatly physiologically. It is fortunate that this is so, for otherwise it would be impossible to experiment with them and learn what each does. If every change in environment that killed one killed the other, there would be no way to separate the partners and learn what each is capable of doing alone, and thus how each helps the other in the partnership.

Withholding not only of wood, but of all nutritious materials, affects the protozoa first and more adversely than the roach. This is especially true when asbestos moistened with water is fed the roach (great quantities are eaten, sometimes even when wood is also present). But this is not a satisfactory method of separating the partners; it will kill all individuals of some genera of the protozoa and most of those of other genera and still leave the roach alive. Yet it never kills all the protozoa, and, when the roach is returned to a diet of wood, the protozoan population begins to come back. Such an experiment lasts for many days, and, even though the roach does not die, it is injured considerably.

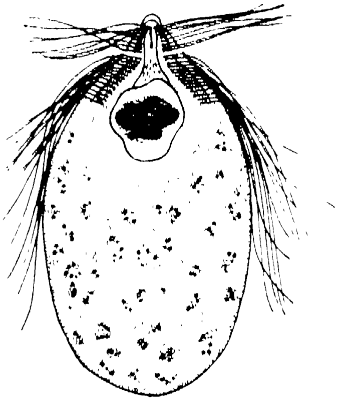
Since the roach can withstand a temperature 3°–4° higher than that required to kill all the



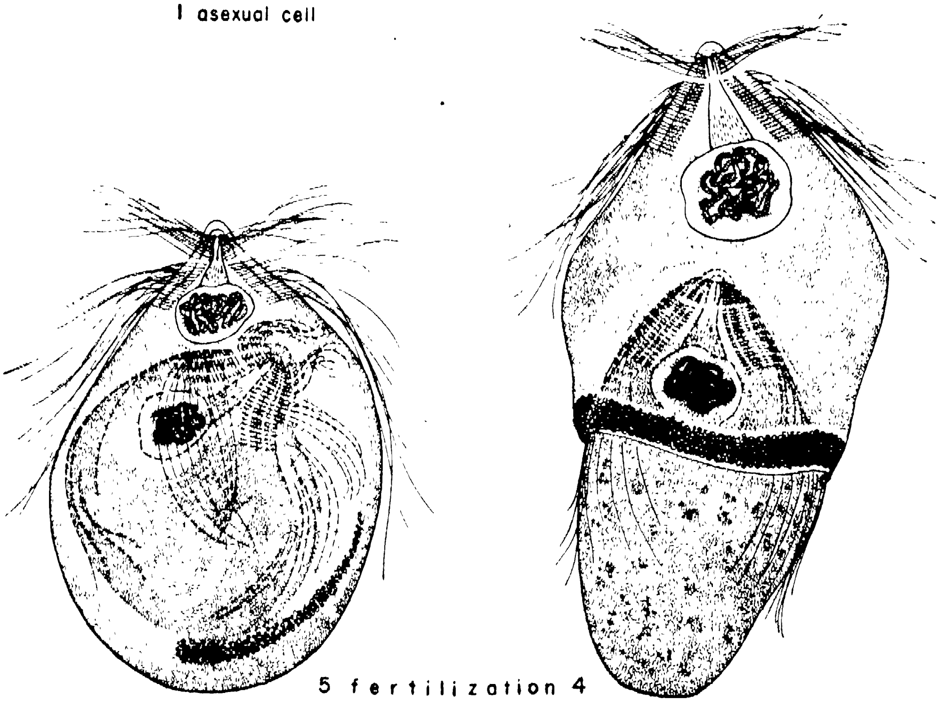
1 asexual cell



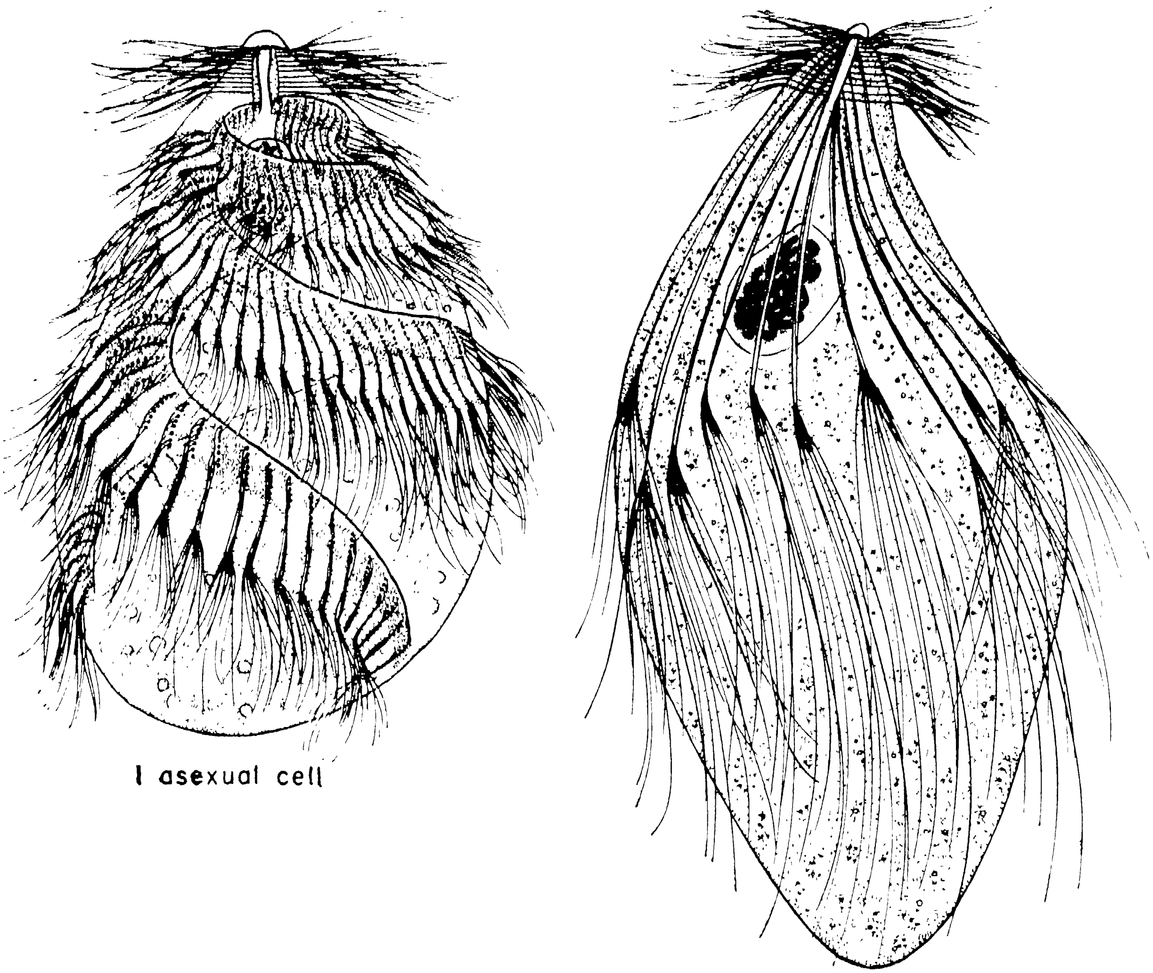
2 egg



3 sperm



5 fertilization 4



1 asexual cell

2 sexual cell

Fig. 2. *Leptospironympha wachula.*

protozoa within it, another type of experiment can be carried out in one hour or in twenty-four—depending on the temperature—and the roach is not injured by it.

An even more satisfactory method for killing the protozoa and leaving the roach uninjured is to use oxygen, which is very much more poisonous for the protozoa than for the roach. The time required to break the partnership depends on the concentration of oxygen used: two hours when four atmospheres are used; several days when one atmosphere or less is used. Temperature plays an important role in oxygen toxicity (a subject that cannot be considered here).

One can also obtain an egg case passed by a roach, keep it until the young hatch, and thus secure a population of roaches naturally free of protozoa. In termites the protozoa are killed every time their partner molts. By isolating termites and keeping them until they molt, one can obtain a population naturally free of protozoa.

Irrespective of the method used in breaking the partnership, the roach cannot live without its protozoa; the same is true of termites. And the protozoa cannot live in any other animal, nor free in nature—not in water, in soil, or in wood. Each partner is therefore absolutely dependent on the other for its existence. Many experiments have been carried out in attempts to learn the details of this dependency. Some facts have been established

Fig. 1. *Trichonympha.* 1, asexual haploid cell which under the influence of the molting hormone is converted in a single division into two unlike sexual cells (1 and 2); 4, sperm is entering egg through its ring of fertilization granules; 5, sperm has entered egg completely, is losing its organelles, and its cytoplasm has fused with that of the egg. Soon the nuclei will fuse and thus complete the process of fertilization.

quite clearly, but there are still many details regarding the precise nature of the partnership that are obscure.

Cellulase, the enzyme responsible for breaking down cellulose, the principal constituent of wood, is present only in the protozoa. When a roach or a termite loses its protozoa, either naturally or by experimental means, it loses its cellulase—no tissues of the body contain it. When the roach regains its protozoa, the cellulase reappears. One species of the protozoa has been grown in culture where it was able to produce cellulase indefinitely.

Both the roach and its protozoa have lived together for such a long time on a diet very low in protein that neither is able to survive if fed much of this substance. They are also unable to tolerate large amounts of carbohydrates other than cellulose. They are able, however, to live very well for many months on a diet of pure cellulose. This is probably due to the fact that the low rate of metabolism of the roach (seven or eight years to reach maturity) and the protozoa requires only a small amount of nitrogen, which the roach, like certain other insects, is able to conserve.

The roach can pass the protozoa on to its young only at the time of molting; hence, molting and hatching of the young must occur at the same time. Otherwise the young will die—and a fair number do—because the partnership is not established.

At each end of that portion of the roach's intestine known as the hind-gut, there is a valve, or screen, which will let the protozoa enter but will not permit their exit in either direction except during molting, when these structures are discarded and replaced by new ones. Thus, during the fall, winter, and spring months, when no young are produced, no protozoa are allowed to pass out with fecal material. From the standpoint of the protozoa, this is highly desirable, because if they passed out at a time when no young were available to take them on as partners it would mean certain death. From the standpoint of the roach, it is also desirable, because no useful purpose would be served if it released its partners when it had no young to take them on. This system of valves, then, works to the mutual advantage of both partners.

A third valve, which is situated in the middle of the hind-gut, and which has a powerful muscular pump to operate it, serves to force the substances elaborated by the protozoa from the wood into the mid-gut, where they come in contact with epithelial cells that absorb them. These substances cannot be absorbed in the hind-gut because it is lined with chitin. In forcing the substances from the hind-gut to the mid-gut the pump does not carry the protozoa along, nor does it carry wood particles; a valve between the two regions of the intestine permits only fluid to pass from one to the other anteriorly.

At the time of molting, and at no other time, some of the protozoa, particularly those in the lower portion of the hind-gut, become more resistant to external conditions and are thus able to survive outside the body long enough to ensure their transmission to the young roaches shortly after they hatch.

In termites the partnership operates differently. In the first place, the protozoa can leave the hind-gut of the termite any time. Ordinarily, they are not passed out with the fecal pellets, but are in the liquid material that sometimes leaves the intestine shortly after a pellet is passed. Instead of resistant forms of the protozoa being produced at molting, the protozoa die and the partnership has to be re-established following each molt. In termites, then, there is no need to correlate hatching of young and molting. This is an advantage in that the young may be hatched over a longer period, but it is a disadvantage to have to pass on the protozoa at hatching and at every molt thereafter. Also, more casualties occur than in roaches. However, the handicap of having to re-establish the partnership so many times in the life of a single termite is more than made up for by the fact that termites—with one exception—do not produce egg cases. Instead, they produce huge queens, virtual egg-laying machines, capable of laying countless thousands of eggs.

Perhaps the most interesting feature of the partnership is the reaction of both partners to the same hormone. In the roach it produces molting and thus allows the insect to increase in size; in the protozoa, it produces several types of sexual behavior, some of which, incidentally, throw considerable light on the origin and evolution of sexual processes in general. Some genera of the protozoa have benefited from this phase of the partnership, since the hormone causes them to produce gametes and thus enables them to utilize whatever evolutionary advantages biparental inheritance offers. The advantage must be considerable since this roach has seven families, twelve genera, and twenty-seven species of protozoa in it.

This is the first time the secretions of the cells of one animal have been shown to possess the ability to convert the asexual cells of another animal into sexual cells and thereby exert a direct influence on the course of its evolution and development. Other examples similar to this one will probably be uncovered by further investigations.

The life cycles and evolution of many parasitic protozoa have probably been influenced to a considerable extent by their hosts.

The illustrations show the great nuclear and cytoplasmic changes that occur in a cell when it is under the influence of the molting hormone. In the genus *Trichonympha,* for example, an asexual cell, by a single division of its nucleus and cytoplasm, produces two gametes, a sperm and an egg. The sperm enters the egg, and its entire contents, cytoplasmic and nuclear, fuse with those of the egg to form a new individual (Fig. 1, 1-5). Each new individual undergoes two meiotic divisions within twenty-four to forty-eight hours after its formation. This serves to return the chromosomes to the normal haploid number and to replace quickly those protozoa given to the newly hatched roaches.

In the genus *Leptospironympha* even greater changes occur in the transition from asexual to sexual cells. The large spiral flagellar bands of the asexual cell are entirely absent in the gametes (Fig. 2).

These sexual forms of protozoa are never produced except when the roach molts. Heating its head inhibits the production of the hormone which makes it molt. If kept at a constant temperature of 18° C, a roach never molts. A brief period of chilling—an artificial winter—is necessary.

It is thus possible by one experimental procedure to inhibit indefinitely molting and the production of the sexual forms of the protozoa, and by another to produce molting and the production of sexual forms of the protozoa more frequently than occurs naturally.

* * *

COWBIRD

Small parasite,
why do you leave your eggs
in other nests?
Can't you nurse your own,
or must you tend the cattle in the fields,
leaving your offspring
to a phoebe's ire,
or to a vireo's generous tolerance?
Must you think only of your
own bright needs?
You feed on grasshoppers,
or on the seeds
of grain or foxtail grass,
while in some alien nest
a small brown head
wakes to a foster breast!

MAE WINKLER GOODMAN

NERVES IN VIVO

CARL CASKEY SPEIDEL

The Annual Thousand Dollar AAAS Prize was awarded to Dr. Speidel, professor of anatomy, University of Virginia, in 1931.

NINETEEN years ago I published a paper on the behavior of regenerating tissues in frog tadpoles that were treated with thyroid gland extract. During that investigation certain puzzling pigment tips were noted. In attempting to watch the origin and fate of these tips in living animals under the microscope, my attention was soon diverted to nerves in the vicinity. Since that time a study of tadpole nerve fibers in vivo has been my major research interest.*

Early investigations revealed the details of nerve growth and regeneration. Direct microscopic observations showed how the pioneer nerve sprouts grew out, how the satellite sheath cells migrated outward along the nerve sprouts and multiplied, and how the nerve sprouts and sheath cells then cooperated to form an insulating sheath of a fatty substance called myelin. In all this work it was found possible to keep track of individual nerve fibers and sheath cells. (The technique was described briefly in an article for this journal: **37**, 47–49, 1933.)

Further investigations continued along several lines: an experimental study was made of the reactions of nerve fibers to various degrees of irritation and injury, together with the steps of recovery or of degeneration. Among the injurious treatments or agents used were electricity, X-rays, heat and cold, salt solutions, strong anesthetics, endocrine gland extracts, alcohol, metrazol, mustard compounds, acids, alkalies, sulfa drugs, and the infliction of wounds by cutting or bruising.

A separate paper was devoted to the effects of alcohol on nerve fibers, and another one to the effects of metrazol. Particular attention was also paid to the behavior of nerve endings. The modern use of metrazol, insulin, and electric shock in the treatment of some human mental disorders made these studies of tadpole nerves of timely interest. Direct evidence was secured that such treatments could cause alterations in the distribution of nerve endings in tadpoles. This suggested a structural basis for the change in the mental condition of human patients similarly treated. (The principal adjustments of nerve endings in general were summarized in a paper presented before The Harvey Society in 1941.)

Nerve fibers are intimately related to other tissues, and it seemed advisable to study these also. A great deal of time was devoted to striated muscle. The same technique of observation was used to watch the growth and regeneration of individual muscle fibers, the minute changes in cross striae during contraction and clotting, and the steps of recovery or degeneration following various grades of injury after different kinds of treatment.

Other kinds of cells were studied also, though less intensively. These included the cells of the blood vessels and blood, lymph vessels, connective tissue, epithelium, notochord, and pigment.

Early attempts were made to record the growth of nerve fibers by fast-motion or time-lapse movies. Although difficulties in technique were encountered, some very satisfactory cine-photomicrographs were obtained. These were first exhibited in 1932, one of the first exhibitions being given at the meeting of the AAAS in December of that year at Atlantic City. Since that time I have tried steadily to build up a motion-picture film library of various kinds of cellular activities. Pictures of this type are superb for demonstration purposes. (The method of making fast-motion microscopic movies of tadpole tissues was described in an article appearing earlier this year in the *American Scientist.*)

NERVES AND SENSE ORGANS

For the past few years my attention has been devoted chiefly to the relation between nerves and special sense organs. A study of these has shown clearly how one structure may affect another in some subtle manner, and how regressive changes will ensue if the normal relationship is eliminated.

The frog tadpole's tail contains a series of special sense organs called lateral-line organs (Fig. 1). They are located in two lines, one along the middle of the tail, the other in the dorsal fin. They look much like the taste buds of the human tongue (Fig. 2). They are sensitive to chemical variations in the water in which the tadpole lives and also to vibrations and to currents. They are innervated by branches of the vagus nerve, a cranial nerve. In both frog and man this vagus nerve also serves the heart, lungs, stomach, and other important structures.

---

* Aided by grants from the National Research Council (Committee on Grants-in-Aid); American Medical Association (Committee on Scientific Research); American Association for the Advancement of Science; Virginia Academy of Science; and the Penrose Fund, American Philosophical Society.

Vagus nerve growth and innervation of organs following tail-tip removal. If the tip of the tail is cut off, regeneration soon takes place and a new small tip develops rapidly. Into this grow fibers from the cut vagus nerve stumps and cells from the organs nearest the wound. A few days is sufficient for the lateral-line branch of the vagus nerve to give rise to very active sprouts (Fig. 3). The growth of this nerve is quite like that of the spinal nerves in the vicinity. Motion pictures beautifully portray its progress.

Ordinarily, the regeneration of the cut lateral-line nerve proceeds concomitantly with regeneration of organs, the latter arising from a placode, or cord of cells, that grows from the last organ into the newly developing tail tip. Normally, the nerve sends side branches into most of the new organs shortly after they become differentiated from the placode. Careful observation in a few favorable cases reveals some of the details (Fig. 4). Delicate naked nerve endings grow into the organs and become intimately related to the special sense cells. They function to receive sensory impulses from the organs, and these impulses are then conducted to the brain by way of the lateral-line and vagus nerves.

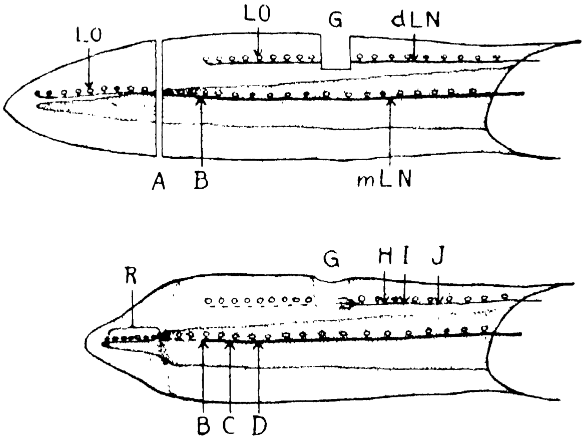


Fig. 1. Lateral view of frog tadpole's tail showing nerves and sense organs of lateral-line system and sites for operations.

Upper sketch shows position of the two lines (*small circles*) of lateral-line organs (*LO*); and their accompanying nerves, the main lateral-line nerve (*mLN*); and the dorsal lateral-line nerve (*dLN*). (Similar nerves and organs on left side not shown.) The tip of the tail may be cut off, as at *A*, and at the same time the main lateral-line nerve may be transected at *B*, thus denervating organs to the left. In like manner, a gap (*G*) cut in the dorsal fin causes denervation of organs to the left in this region.

Lower sketch shows condition of tail about 10 days later. The tip has regenerated somewhat, and a new line of organs (*R*) has grown into it. Beyond *B* the nerve has degenerated (*broken line*); *C* and *D* represent points at which additional successive nerve transections are made to keep terminal organs denervated. In the dorsal fin the gap has filled with regenerating tissue into which nerve fibers are growing from the right. Degenerating portion of severed nerve (*broken line, left*) and (*circles*) the accompanying denervated organs. To ensure continued denervation of these organs, additional transections of nerve may be made successively at *H, I,* and *J.*

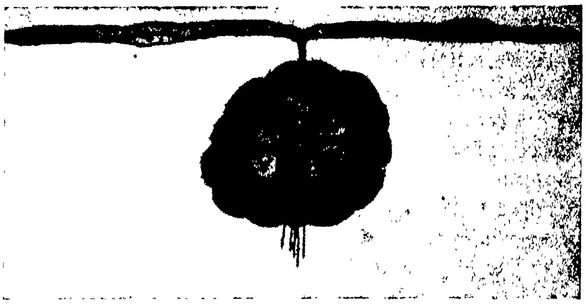


Fig. 2. A lateral-line sense organ and its nerve supply viewed microscopically from lateral surface of tail. In this organ 6 sensory cells are visible near center, grouped about a shallow pit. Five of the sensory cells are provided with long sensory hairs, which jut out through a central pore into the water. Except at the pore where the sensory cells are located, the whole organ is covered with a thin flat layer of skin cells (not shown). From the accompanying nerve fiber, between the 2 myelin segments illustrated, comes a side branch which enters the organ and divides into sub-branches, each of which terminates in a delicate end bulb. (This sketch, semischematic in nature, combines structures which under the microscope are actually at various levels of focus.)

For the past 70 years investigators have been trying to determine the extent to which sense organs depend upon their specific nerve supply for their structural integrity. Two sharply contrasting views are held, derived from research work on both lateral-line organs and taste organs. One view emphasizes the paramount importance of the nerves for the origin, growth, regeneration, and maintenance of the organs. The other view stresses the independence of the organs and holds that the specific nerves are unnecessary for the origin of new organs, or for their growth, regeneration, and maintenance. My recent experiments on tadpoles, however, indicate clearly that neither of these views is wholly correct.

Regeneration of denervated organs. A very effective method of determining whether lateral-line sense organs can arise without lateral-line nerve influence was used. This involved the clipping off of the tail tip, combined with several successive transections in an anterior direction of the lateral-line nerve (cf. Fig. 1). In this way, while the organs nearest the line of tail amputation were kept denervated, they were at the same time stimulated to strong regenerative activity. The first of the nerve transections could be made before, after, or at the same time as the tail-tip amputation. There was no difference in the end result. In all cases, the regeneration of new organs proceeded normally from placode cells which arose from the organs at the wound edge (Fig. 5). Mature organs with sensory hairs and other characteristic specialized features were formed. Such organs without nerve supply could persist for at least several weeks, until the time for metamorphosis with accompanying reduction of the tail brought the observations to an end. This type of experiment showed clearly that the specific nerve supply was not necessary for the regeneration and early growth of the lateral-line organs.

Maintenance of denervated organs. Experiments were next undertaken to find out whether nerveless organs would preserve their structural organization indefinitely, or whether regressive changes would ultimately ensue. Green frog tadpoles were used because these remained in the tadpole stage for one or two years. Individual organs in the dorsal fin region (cf. Fig. 1) were watched for periods of 3–21 months.

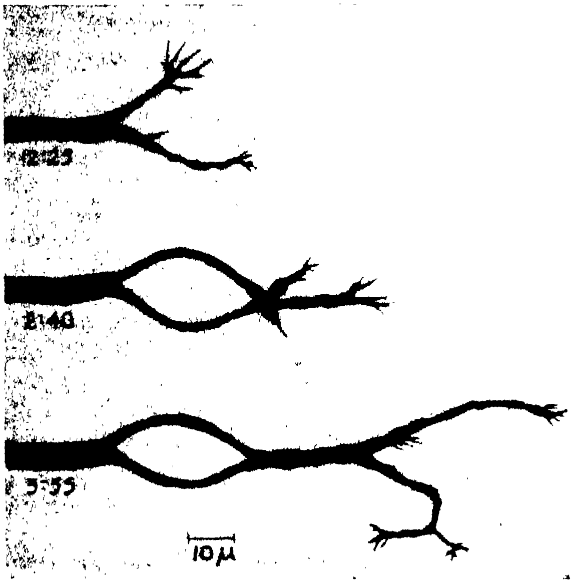


Fig. 3. Regenerating fibers of the lateral-line nerve, 3 days after removal of the tip of the tail. The sketches, made from motion-picture films, show 3 positions of the growing nerve sprouts over a 3½-hour period: at 12:25 P.M., 2:40 P.M., and 3:55 P.M., respectively.

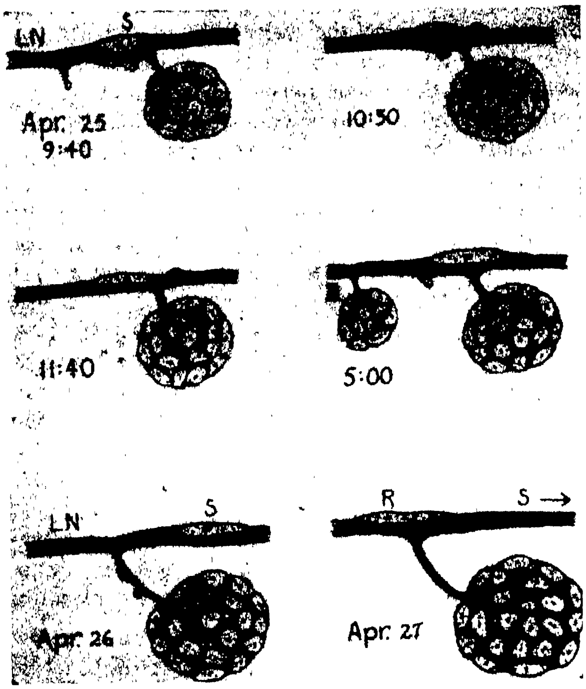


Fig. 4. Lateral-line organ innervation in a rapidly regenerating zone (from Speidel, C. C., *J. Comp. Neur.*, **87**, 1947). Following tail-tip removal on Apr. 19, regeneration of organs and nerves took place as a new tip grew out. On Apr. 25, at 9:40 A.M., a young organ was visible, supplied by a short branch from the accompanying lateral-line nerve (*LN*). A sheath cell (*S*) was nearby. Growth changes in the nerve endings within the organ were noticeable during the next 7 hours, as shown at 10:30 A.M., 11:40 A.M., and 5:00 P.M. Between 11:40 A.M. and 5:00 P.M. there also arose a new branch (*B*) which supplied a new small organ just becoming differentiated from regenerating placode cells in this zone. On Apr. 26 and 27 further growth of the nerve endings was discernible. At this time the organ was supplied with 4 sensory hairs. (These were located at a more superficial level of focus and are not included here.) Sheath cell *S* moved to the right, out of the field; sheath cell *R* moved into the field from the left. (A motion-picture record of this case was obtained.)

Histories of hundreds of organs in many different tadpoles showed that organs rendered nerveless for periods up to about three months did not necessarily degenerate. For longer periods, however, regressive changes became increasingly noticeable. These included atrophy (reduction in size), dedifferentiation (loss of some features of specialization), and degeneration (death of the component cells).

The case illustrated here (Fig. 6) strikingly demonstrates the long-range trophic effect of the specific nerve supply on the sense organs. In this tadpole, after an operation on the dorsal fin which severed both dorsal lateral-line nerves, regenerating fibers from both right and left proximal stumps entered the distal stump of the left side but not of the right side. A comparison was possible, therefore, between the reinnervated organs on the left and the nerveless organs on the right. Significant differences were not apparent for the first 3 months, but they became increasingly conspicuous during later months.

The two stages illustrated, 1 month and 10 months, respectively, after the operation, show that whereas there was a normal growth increase on the reinnervated left side from a total of 28 to 50 organs, there was a marked decrease on the denervated right side from 23 to 8 organs. This number on the right side decreased to 7 organs before the observations were terminated nearly a year after the operation; at the same time the number on the left increased to 66. Outside the zone of operation on both sides proximal to the cut, where the organs were normally innervated, their number also increased.

Most of the surviving nerveless organs on the right side exhibited marked atrophy and varying degrees of dedifferentiation. The organs and their component cells were smaller. The sensory hairs were for the most part shorter and fewer in number, though there was a good deal of variation.

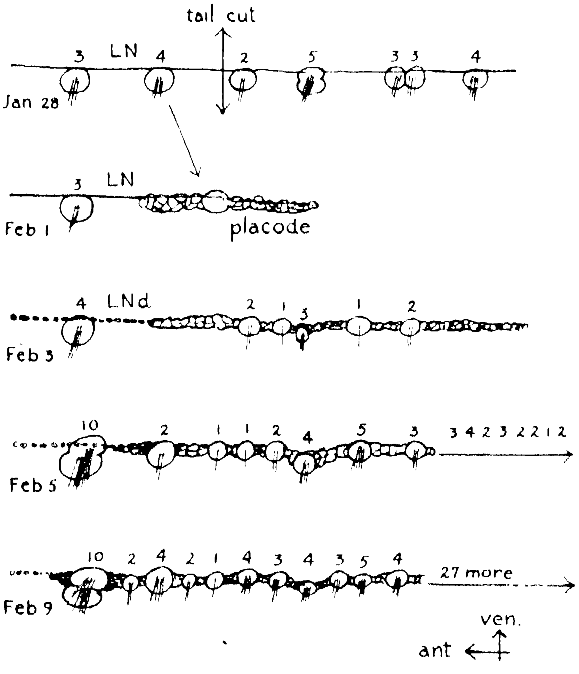


Fig. 5. Regeneration of denervated lateral-line organs (from Speidel, *J. Comp. Neur.*, **87**, 1947). The tail tip was cut off on Jan. 28 at the site indicated (*double arrow*). Three days later organ nearest wound had become transformed into a placode, or cord of cells. This was denervated and kept denervated by nerve transections farther anteriorly on Feb. 1, 3, 6, and 9. By Feb. 3, five new organs were visible. (The numerals designate the number of sensory hairs in each organ.) By Feb. 5 number of new organs had increased to 15; by Feb. 9, to 37. The average number of sensory hairs per organ also increased.

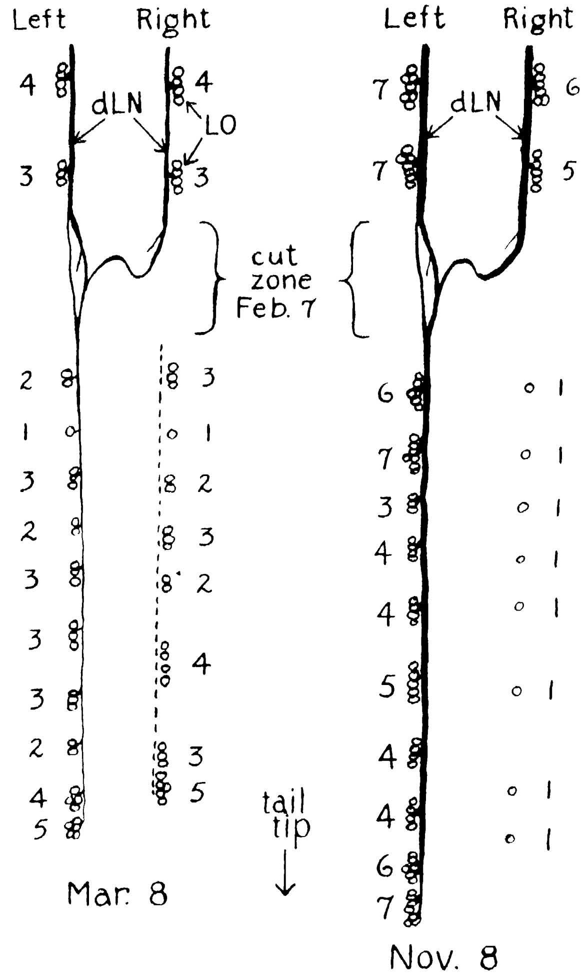
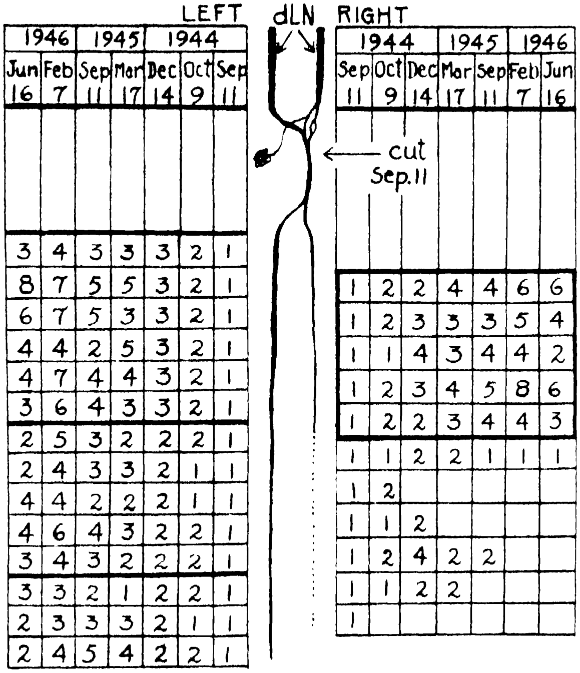


Fig. 6. Diagram of the dorsal lateral-line nerves and organs of both right and left sides showing the long-range changes in organ numbers in denervated and reinnervated zones (from Speidel, *Am. J. Anat.*, **82**, 1948). The nerves (*dLN*) and organs (*LO*) are represented as if seen from the dorsal aspect. A gap was cut in the dorsal fin on Feb. 7 at site indicated (*brace*). Cf. Fig. 1, gap *A*. By Mar. 8 regenerating fibers from both proximal stumps had entered the distal stump of left side only. On this side distal to the wound there were 28 organs in 10 groups; on the right side there were 23 organs in 8 groups. By Nov. 8, however, there were 50 organs on the reinnervated left side, but only 8 single organs on the nerveless right side. Proximal to the cut zone the number of organs increased on both sides.

Thirteen other case histories were obtained in which regenerating nerve fibers reinnervated the distal stump and organs of one side only. The results were like those of the case just described.

Two interesting case histories of partial reinnervation were obtained from tadpoles which were kept under observation for record times of 21 and 19 months, respectively, after operation. In one of these (Fig. 7) the regenerating nerve fibers on the right side crossed the wound and reinnervated the



LEFT

1946		1945		1944		
Jun 16	Feb 7	Sep 11	Mar 17	Dec 14	Oct 9	Sep 11
3	4	3	3	3	2	1
8	7	5	5	3	2	1
6	7	5	3	3	2	1
4	4	2	5	3	2	1
4	7	4	4	3	2	1
3	6	4	3	3	2	1
2	5	3	2	2	2	1
2	4	3	3	2	1	1
4	4	2	2	2	1	1
4	6	4	3	2	2	1
3	4	3	2	2	2	1
3	3	2	1	2	2	1
2	3	3	3	2	1	1
2	4	5	4	2	2	1

RIGHT

1944			1945		1946	
Sep 11	Oct 9	Dec 14	Mar 17	Sep 11	Feb 7	Jun 16
1	2	2	4	4	6	6
1	2	3	3	3	5	4
1	1	4	3	4	4	2
1	2	3	4	5	8	6
1	2	2	3	4	4	3
1	1	2	2	1	1	1
1	2					
1	1	2				
1	2	4	2	2		
1	1	2	2			
1						

Fig. 7. History of lateral-line organs following partial reinnervation of the distal nerve stump on one side and complete reinnervation on the other (from Speidel, *Am. J. Anat.*, **82**, 1948). Both right and left dorsal lateral-line nerves (*dLN*) were severed by a gap cut in the dorsal fin on Sept. 11, 1944 (*arrow*). Second and third cuts were made nearer the root of the tail on Sept. 14 and 18 (cf. Fig. 1, gap *G* and cuts *H* and *I*). Regenerating fibers from each proximal stump grew into scar zone and formed a common trunk, which divided and gave branches to each distal stump. On left side, entire nerve stump and its associated organs became reinnervated. On right side, however, the innervating fibers, 2 in number, grew only halfway through the distal stump and reinnervated organs in this zone only. The terminal half of right distal stump and the organs of this zone remained completely devoid of nerve fibers (*dotted line*). On Sept. 11, 1944, there were 11 single organs on right side and 14 on left. (The numeral in each block indicates number of organs in a group.) The changes in organ number are shown for Oct. 9 and Dec. 14, 1944; for Mar. 17 and Sept. 11, 1945; and for Feb. 7 and June 16, 1946.

proximal 6 groups of organs only, leaving the distal 6 groups on the same side quite nerveless. On the left side all 14 groups of organs were reinnervated. During the following months the organs supplied with nerves steadily multiplied. The greatest number was reached about seventeen months after the operation. This increase was from an original number of 14 on September 11, 1944, to 68 on February 7, 1946. (After February 1946 the number of organs decreased somewhat as the time approached for metamorphosis with accompanying tail resorption.) In sharp contrast, during the same period the nerveless organs on the right side decreased from 6 to 1.

In the other case history of partial reinnervation, which is not illustrated here, a single regenerating nerve fiber grew across the wound zone and reinnervated the proximal 7 groups of organs on one side, but not the distal 9 groups on the same side. On the other side, there was no reinnervation of any of the 14 groups of organs across the wound. Nineteen months after the operation only 1 organ of the entire 23 nerveless groups survived, as contrasted with 15 organs that composed the 7 groups innervated by the single nerve fiber.

These two case histories, as well as that given in Figure 6, bring out also that there is a significant correlation between the number of organs in a group and the number of innervating nerve fibers. Thus, a single nerve fiber is capable of supporting groups of organs which may attain a maximum average of 2.4 organs per group. Two nerve fibers are capable of supporting groups of organs which may attain a maximum average of 5 organs per group. Three or more nerve fibers may support a maximum average of more than 6 organs per group.

The foregoing examples show beyond doubt that the lateral-line nerve fibers exert a strong long-range trophic influence over the sense organs that they supply. Permanently denervated organs deprived of this influence cease growing after a time, and then undergo regressive changes leading to ultimate degeneration.

No evidence has been seen to indicate that nerves may induce sense organs to form from indifferent epithelium, a view which is held by many investigators. On the contrary, in these experiments organs arise only from pre-existing organs, or from the partially dedifferentiated remnants of organs.

Fate of organless nerves. Regenerating lateral-line nerve fibers in wound zones occasionally go astray and fail to reach any lateral-line organ. These may be termed "organless nerve fibers." Such fibers, during the first month or two, may become ensheathed with myelin and appear to be mature fibers; nevertheless, they suffer regressive changes later. These changes usually become conspicuous 3–6 months after the operation. The myelin sheath becomes less massive, and the enclosed axis cylinder becomes smaller. Then the myelin is lost entirely, and ultimately the nerve substance itself degenerates. Thus, the preservation of the normal structure of lateral-line nerve fibers appears to be significantly correlated with sense-organ innervation. In other words, sense organs exert an important trophic influence on their nerves.

SOME ACCOMPLISHMENTS AND LIMITATIONS OF REACTION RATE THEORY*

HENRY EYRING

Professor Eyring, of the Graduate School, University of Utah, received the Annual Thousand Dollar AAAS Prize in 1932.

THERE are two extreme types of reaction rates. One kind is typified by ordinary radioactive decay where the rate is unaffected by ordinary changes in temperature and pressure. The second is ordinary chemical reactions dependent on both temperature and pressure. For both types of reaction, the rate can be represented as

$$rate = \Sigma_i n_i \nu_i \gamma_i \quad (1),$$

where n_i is the number of reacting systems in the ith state, ν_i is the number of times per second that the system in the ith state vibrates in the direction normal to the reaction barrier, and γ_i is the chance on each assault that the system successfully traverses the barrier.

If the rate is not affected by temperature or pressure, the values of n_i are in general known from the chemical analysis of the material. For equilibrium systems sensitive to temperature and pressure, the appropriate statistical mechanical calculation is required. This calculation, as well as the calculation of ν_i and γ_i, depends on adequate knowledge of the potential energy surface, especially in the neighborhood of the minimum and the barrier saddle point. In principle, it is possible to calculate all such surfaces for molecular complexes. For nuclei, potential surfaces are only known in the region where Coulomb's law holds. For distances closer than about 3×10^{-12} Angstroms (depending somewhat on the atom), the unknown nuclear forces determine the shape of the surface. However, Condon and Gurney and Gamow showed how barrier leakage could explain the radioactive decay of the elements and the capture of a swiftly moving particle colliding with the nucleus.

The fraction of molecules decaying per second radioactively was found to be approximately

$$k = \frac{1}{a}(2\mu E)^{\frac{1}{2}} e - \frac{4\Pi}{h} \int (2\mu(V-E)^{\frac{1}{2}} dx \quad (2).$$

Here a is the width of the interior of the atom. E is the energy of the escaping particle, μ is the corresponding reduced mass, and V is the potential energy of the barrier. The integral is to be taken from the point of entering the barrier to where it emerges. This treatment is satisfactory in so far as it can be tested without a better knowledge of the nature of the nuclear forces.

Before the above treatment was applied to the nucleus, an analogous treatment of molecular isomerization was made by Hund. Actually, the integral for barrier penetration in equation (2) is roughly the same magnitude for molecules as for the nucleus. This is because, although the barriers are about a million times as high for the nucleus, they are also usually over a thousand times thinner. The result is that chemical reactants almost invariably find it easier at ordinary temperatures to surmount the barrier rather than to tunnel through it. The most notable exception is the case where the three hydrogen atoms in ammonia first form a pyramid with the hydrogen base on one side of the nitrogen apex and then on the other. This turning wrong side out happens about 10^{10} times per second and is a true tunneling very much like radioactive decay. Examples of this kind are extremely rare, however. Most reactions involve the surmounting of the barrier. In 1928 London showed how the quantum mechanics could be used to construct potential barriers and pointed out that, in general, the surmounting of the barrier usually proceeded without electronic reorganizations of the kind occurring in electronic transitions.

Polanyi and Eyring devised means of constructing potential surfaces from London's approximate formula. These were sufficiently accurate to show that tunneling is in general negligible in chemistry and to form the basis for a quantitative statistical theory of reaction rates. In principle, we can presumably calculate the rate of any chemical change. In practice, it has only been possible so far to show from first principles that, for the simplest of reactions, $H + H_{2\,para} = H_{2\,ortho} + H$, the activation energy is less than 19 K calories, whereas experimentally it is found to be about 7 K calories. Nevertheless, we now have a detailed theory of reaction rates. Our only really serious limitation in predicting reaction rates a priori is the lack of workable methods for calculating barrier heights. This is a difficult problem, but it is of sufficient importance that it will surely be solved eventually.

---

* Owing to lack of space, references have been omitted.

The methods of perturbation theory lead to the London-type formula, which gives the energy for a system of atoms in terms of the binding energy between pairs. The binding energy between atom pairs is considered to be of two types: a classical "coulombic" binding, which is about 14 percent of the total and is simply additive; and the "exchange," or valence, binding which constitutes the remainder.

The well-known approximate London formula for the energy E for four monovalent atoms is

$$E = Q - \{\tfrac{1}{2}(\alpha-\beta)^2(\alpha-\gamma)^2(\beta-\gamma)^2\}^{\frac{1}{2}} \qquad (3).$$

Here the total coulombic binding is included in Q, α may be taken as the sum of the exchange part of the two initial bonds before reaction, and β is the sum of the two bonds formed after the reaction; γ is the sum of the two bonds which are present only in the activated state along with α and β.

The radical in (3) may be shown to be equal to the length of a vector found by adding β onto the end of α at an angle of 60° and γ onto the end of β, making the 60° angle which will make the vector sum smallest. A chemical reaction starts with α large and with β and γ zero and ends with β large and α and γ zero. The activated state will thus come approximately when $\alpha = \beta$ and the total binding will be less than α or β because of γ. For three monovalent atoms, equation (3) still applies with the same meaning except that α, β, and γ now stand for a single bond in each case. Equation (3) and its analogues for more electrons provide a useful basis for constructing approximate potential surfaces in general.

It is of interest to compare the molecular orbital calculation of energy with the results of equation (3), which is based on the method of bond eigenfunctions. The approximate calculated energy for the system taking the state of the separated atoms as nq, where n is the number of atoms, is for two hydrogen atoms $E = 2q + 2\beta$; for three hydrogen atoms on a line, two equispaced from a central atom, $E = 3q + 2.82\beta$; for three hydrogen atoms forming an equilateral triangle, $E = 3q + 3\beta$; and for four hydrogen atoms forming a square, $E = 4q + 4\beta$.

To make the results for the molecular orbital method agree with experiment, we must assume that the repulsion between nuclei and between electrons, which has so far been neglected, will act to change E for three atoms on a line from 2.82β to about 1.8β, with correspondingly bigger corrections for a triangle and a square. Qualitatively, the results would then seem sensible. A systematic application of the molecular orbital method to activated complexes should be carried out. A beginning has been made by Sherman and Van Vleck and more recently by Pearson.

The statistical theory for reaction rates takes the form

$$=(A)(B)\;.\;.\;.\;K^{\ddagger}\frac{kT}{h}K \qquad (4).$$

where (A), (B), etc., are the concentration of reactants. $K^{\ddagger}$ is the equilibrium constant between the normal and activated state and is readily calculable in terms of partition function when the quantum mechanical calculation of barrier shape has been carried through; $\frac{kT}{h}$ is the frequency at the absolute temperature T. At room temperature, it has the value 5.6×10^{12}. The transmission coefficient K is calculable from quantum mechanics when the barrier shape is known, but can usually be taken as unity.

The specific reaction rate constant k' can thus be written as

$$\begin{aligned} k' &\equiv K\frac{kT}{h}K^{\ddagger} = K\frac{kT}{h}e^{-\frac{\Delta F^{\ddagger}}{RT}} \\ &= K\frac{kT}{h}e^{-\frac{\Delta H^{\ddagger}}{RT}}e^{-\frac{\Delta S^{\ddagger}}{R}} \\ &= K\frac{kT}{h}e^{-\frac{\Delta E^{\ddagger}+p\Delta V^{\ddagger}-T\Delta S^{\ddagger}}{RT}} \\ &= K\frac{kT}{h}e^{-\frac{\Delta F_o^{\ddagger}+\int\Delta V^{\ddagger}dp}{RT}} \\ &\quad \frac{kT}{h}e^{-\frac{\Delta F_o^{\ddagger}+\overline{\Delta V^{\ddagger}p}}{RT}} \end{aligned} \qquad (5).$$

Here $\Delta F^{\ddagger}$ is the Gibbs free energy of activation at pressure p, and $\Delta Fo^{\ddagger}$ is this value at $p = o$. $\Delta H^{\ddagger}$ is the heat of activation; $\Delta S^{\ddagger}$ is the entropy of activation; $\Delta V^{\ddagger}$ is the increase in volume between the normal and the activated state. This equilibrium theory is applicable to all rate processes, both physical and chemical, where the activated state does not involve excessive barrier leakage. Where there is leakage, the corresponding contribution can be added to the contribution (5) for passing over the barrier. The only serious difficulty in estimating reaction rates a priori is the estimation of $\Delta E^{\ddagger}$.

a) Association reactions involving the breaking of no bonds ordinarily have $\Delta E^{\ddagger} = 0$.

b) Substitution reactions of the type $A + B - C \rightarrow A - B + C$ have $\Delta E^{\ddagger}$ equal to 5 percent of the bond $B - C$ if A is an atom and if the bond $A - B$ is stronger than $B - C$. This defines the activation energy for the reverse reaction since it must be greater by the energy of reaction.

c) The reaction $A - B + C - D \rightarrow A - C + B - D$ has a $\Delta E^{\ddagger}$ about 28 percent of the two bonds broken if it is proceeding in the exothermal direction. The reaction $A - B + C - D \rightarrow A - D + B - C$ is likewise possible. Presumably that process will be most rapid which is most exothermal. This would be an interesting point to investigate.

d) In viscous flow, the free energy of activation is equal to the heat of vaporization of the flowing unit (usually a molecule) divided by 2.8. Diffusion very frequently proceeds by the same mechanism with the same activation-free energy.

e) In many organic reactions, the energy of activation is the energy of ionization of one of the reactants. Thus, if one nitrates solutions of benzene or of toluene in sulfuric acid, they go at the same rate, but if a benzene-toluene mixture is nitrated, the toluene is 29 times as apt to nitrate as benzene. This proves the critical complex does not contain benzene or toluene, but when formed it reacts with one or the other rather than decomposes spontaneously—reacting 29 times faster with the toluene molecule. This is in line with the usual situation in organic reactions. Thus, frequently, reactive complexes are formed which may react alternatively in a variety of ways.

f) Optically active synthesis, so common in biological systems, is certain proof of the existence of an optically active enzyme which acts as a templet, making better secondary bonds with the favored optically activated complex than with the other. This of course does not preclude simultaneous enzyme action on primary bonds. Such action, however, does not favor one optical isomer over the other.

g) Solvents with high dielectric constant, such as water, are such effective catalysts of intercombination between ionic crystals that there is no measurable slowness of solution and reaction.

THERE are many other general influences affecting individual reaction rates, but we turn now to another type of question. As we consider the natural rate processes occurring around us, they usually turn out to be a complex of the elementary processes for which the general theory has been outlined. This complex of elementary processes can often be thought of as being in fact a machine. We consider as a first example, very briefly, the process of growing old. Children from young or from old parents do not necessarily reveal the difference in the age of their parents. It thus is not individual cells that are growing old. Actually, in a certain sense, each individual is as old as the race; and, under favorable circumstances, the race might continue forever. Each individual is a sort of backwater along the stream of life. In the individual, certain cells have become specialized to form organs, and failure or unbalance between these organs sooner or later must spell disaster for the individual. This may be brought about by a series of elementary chemical processes or by physical damage. In any case, the interesting problem involves a whole complex of processes which will require reaction rate theory as a chief tool in its solution. Considerable progress has already been made in applying rate theory to living cells.

The weathering of a building or a rock on a mountain is another complicated series of events. Here chemical processes of solution proceed along cracks, started by unequal heating and cooling, that are then enlarged by freezing and thawing. Again, we require our theory of elementary rate processes if we are to bring order out of chaos.

As a final somewhat complicated example, consider mountain building. Here, besides the wrinkling that is to be expected with the contraction accompanying solidification of a liquid, we require an explanation of why mountain building seems still to be continuing about as briskly as ever. Clearly, some heat engine is at work which requires a continuing supply of heat. We think we know that no chemical reactions could be supplying the amount of heat required, so we turn to radioactivity. The known radioactive decay in granite supplies roughly 5 calories/million years/gram. Since the specific heat is about 1/3 calorie/gram, this would mean a temperature rise of about 15 degrees/million years at depths where the heat loss by conduction is small. In the course of time, such rocks must melt and so expand about 10 percent with an almost irresistible force. Thus, either the overburden will move up or strata will be pushed sideways or invaded, or liquid will pour out onto the surface, whichever process is easiest. With the melting of sufficient rock, heat losses are stepped up owing to convection; such losses are increased also by the foundering of part of the overburden and by the liquid finding its way to the surface. These processes, when they get in full swing, will cool the mass faster than heat is supplied, and a period of solidification and subsidence sets in, only to be followed in time by remelting. Thus, we have almost endless cycles of uplift and subsidence wherever the radioactive "yeast" is present. Especially is this true in the deltas formed at the margins of continents, which in due course rise to make mountainous continental margins. Thus one confidently anticipates that a great mountain chain will eventually rise from the sediments now depositing at the mouth of the Mississippi.

In the earth, radioactivity is concentrated in the acid crust, which forms the continents. This concentration may be due in part to chemical affinity. However, since radioactive material has the property of becoming heated, its surroundings as a result automatically become lighter and so move upward, to be finally captured with other light materials in the surface crust. Explosions such as those at Krakatoa are the natural result of overheating large volumes of gases, such as water, and the distillates from partially oxidized organic materials, such as CO_2, NH_3, and the hydrocarbons. As radioactivity slowly heats buried sediments, oil distilled from buried organic matter moves upward along porous strata, dissolving material as it goes, to be trapped in appropriately situated domes. In other places where the temperature gets high enough, as around the edges of batholiths, natural smelting occurs and ore deposits form. Joly, especially, has emphasized the role of radioactivity and isostasy in geology. Although his views have been criticized and although the criticisms are probably justified in part, still radioactivity must be the chief fuel in the engine warping the surface of the earth.

Finally, we come to the dilemma that challenges all who would understand the broader aspects of reaction rate theory. The second law of thermodynamics tells us that the entropy of the world always increases, and the result of reactions is to move ever closer to the final equilibrium. In this "heat death," life and all the changes that interest us will have ceased except for possible fluctuations from equilibrium. Reaction rate theory is a theory of the possible fluctuations of reacting molecules from equilibrium. A reaction occurs at a moderate rate if the activated state must acquire 40 times the average energy of an oscillator, as a fluctuation at equilibrium; but if it must acquire 2 or 3 times this energy, one must wait geologic ages for the fluctuation to happen. It thus becomes fantastically improbable to think of a hot sun and a relatively cool earth ever originating by simple fluctuations from a region at constant temperature.

On the other hand, if the expanding universe because of inertia expands too far, only then to reverse itself and contract too far, we have in the expanded state a system with great randomness and relatively low energy density, reverting after some billions of years to a state of great material and energy density, which will of necessity be a state also of great order. During the expanding phase spontaneous processes tend to decrease the order and increase the entropy, whereas during contraction the reverse will be true. In this view, the second law of thermodynamics is a consequence of living in the expanding phase of the cycle. During the contracting phase matter and energy seem to be pouring in toward the center from outer space, spontaneously assuming states of greater order. In this way impossible fluctuations become the comparatively orderly consequences of a cyclic process. In any case, we are much further from a complete understanding of how systems out of equilibrium arise than we are from understanding how they subsequently proceed toward equilibrium.

* * *

A PLANT PHYSIOLOGIST LOOKS AT THE CANCER PROBLEM

PHILIP R. WHITE

Dr. White, of The Institute for Cancer Research, Philadelphia, received the Annual Thousand Dollar AAAS Prize in 1937.

IT IS a tragic platitude, one which, because it is a platitude, has remained too long beneath the surface of the public consciousness, that cancer destroys more human lives than any other disease except "heart failure." The heart maladies are mostly degenerative diseases. We will conquer them when we understand the nature of aging. Cancer, on the other hand, is par excellence *the* regenerative malady. I have deliberately refrained from calling it a "disease" because to most of us disease represents weakness, degeneration, breakdown. Cancer is primarily none of these but, rather, a lusty, undisciplined growth. Degeneration and weakness are only secondary sequelae thereto.

To many medical men, "cancer" is synonymous with "carcinoma," hence an infiltrating, metastasing, malignant growth of epithelial origin. The sarcomas, osteomas, etc., are excluded from this narrow category even when malignant. To the biologist and to most patients this is too narrow. A man dying of a malignant osteoma does not care whether it is of epithelial origin; it is to him a cancer. I am not a medical man, but a plant physiologist. In bringing me into the field of cancer research, it is tacitly understood that I will look at the problems from a plant physiologist's point of view. As a plant physiologist I take a broader view, and define cancer as Webster does, as "any malignant tumor." Malignant is "tending to produce death." A tumor is "any noninflammatory mass of tissue which is independent and unrestrained in growth and structure, without normal physiologic function, and arising without obvious cause from pre-existing tissue."

If one examines this definition carefully, it will be seen that, stripped to its essentials, the important words are "independent, unrestrained, unphysiological, and noninflammatory." Nowhere is there any mention of the sort of organism involved, or the tissue, or the type of cell. It might equally well be ectoderm or mesoderm, bone or nerve, or skin. There is nothing to indicate if it is mammal or invertebrate or plant. The entire definition rests upon a characterization of the malignant cell. These adjectives "independent, unrestrained, unphysiologic, noninflammatory" set the malignant cell off from normal cells but do nothing more. If this is a sound definition, then cancer is an aberration of cells, no more, no less, and anyone who studies these aberrant cells is, per se, an investigator of cancer. It is through this gate that a plant physiologist may enter.

One fact has been brought home to me with increasing force repeatedly during the past few years. That is the unexpected realization that, while the concept of the cell as an *anatomical* unit has become an unconscious part of our thinking, the concept of the cell as a *physiological* unit has not penetrated even the surface of most of our minds. Our students (and teachers!) have clothed Hooke's dead chamber with the living sarcode of Du Jardin, yet have left it no more than a well-papered and -furnished room in the vast tenement of the body. The place of the cell as an *independent unit of function* has not penetrated to the general consciousness of scientists in spite of the clarity of the concepts formulated more than a century ago by Dutrochet and Schwann. I am sure that the first reaction of many readers will be to deny this, but I am equally sure that upon further consideration they will agree. In fact, if I interpret the written word rightly, such authorities as Conklin in 1940 (*Cell and Protoplasm Concepts,* 10th Symposium of the AAAS, #14, 1940) and Whitman before him in 1893 (*The Inadequacy of the Cell Theory of Development,* 1893) deny the correctness of that concept.

Yet it seems to me that it must be a sound concept, as anyone must testify who has had the experience of watching the movement of the connective tissue cells, the migration of nerve fibrils, even the spread of an epithelial membrane in tissue culture. I think that nowhere else is the complete functional independence of the cells which make up our bodies so clearly shown. In fact, as one watches the fibroblasts and macrophages moving hither and thither across the screen in a time-lapse movie of a tissue culture, one is reminded of two things; a swarm of bees as it is about to leave the

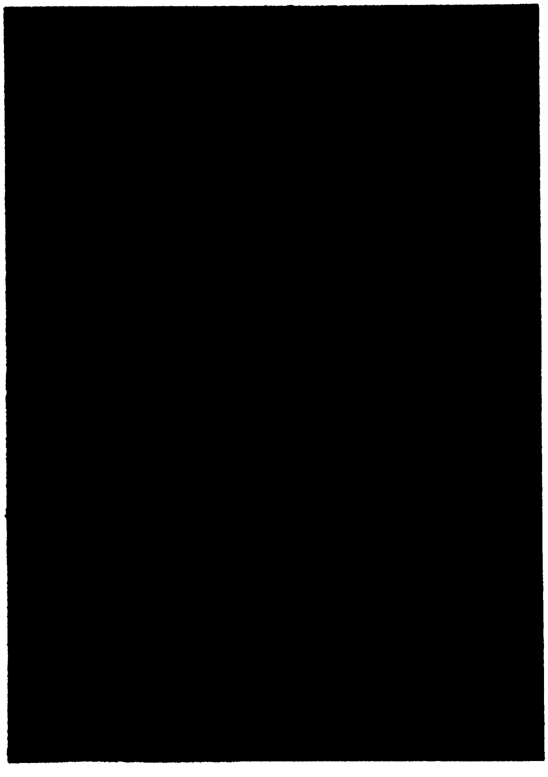
Crown gall on sunflower plant, resulting from a single needle-prick inoculation with a broth culture of *Phytomonas tumefaciens.*

old hive, and the classic pictures of the molecules in a gas, or, perhaps better still, the electrons in a solid. One recalls Schwann's words:

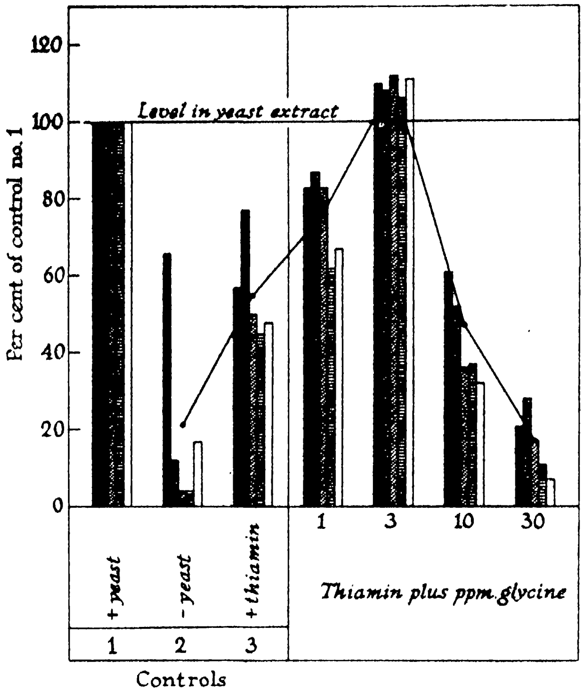
> That not every cell, when separated from the organism, *does*, in fact, grow (when so separated) is no more an argument against this theory [of physiological independence] than is the fact that a bee soon dies when separated from its swarm a valid argument against the individual life of the bee.

One might add that the successful cultivation of cells outside the organism, in tissue cultures, removes even this argument. The cell *is* a physiological unit. When a gland secretes pituitrin it is the *cell* that is carrying on the physiological process, not the gland; when a nerve carries a stimulus it is the *nerve cell* that performs the conduction, not the complex system which we call a nerve; when a muscle contracts it is the individual cells which carry out the physicochemical changes responsible for contraction; when a tissue respires it is the individual cells which carry on the process. The tissues and organs of which an organism is constructed are merely means of integrating the varied activities of the constituent cells into a machine that can function. True, this integration is essential for the normal functioning of the organism as a whole, but it is not essential for the normal functioning of the individual parts, the cells.

It seems strange that this fact has not really penetrated below the surface of our thinking. We still think either in terms of heart action, kidney function, muscle contraction, blood circulation—that is, in terms of *organ* function—or, at the other extreme, of pancreatic juice digestion, visual purple, cytochrome, insulin—that is, in terms of chemical processes outside the cell. In none of these are we thinking in terms of the cell itself. Disfunction as we usually encounter it, except in cancer, is probably more often a matter of failure of the *integrating* mechanisms than it is of failure of the cells. In fact, the most ubiquitous example of integrative failure, aging and death, is not a failure at the cellular level, as the unlimited survival of



Tomato root tips cultivated for 1 year on a nutrient chemically defined except for inclusion of an extract of 100 mg of yeast per liter. This unknown was later replaced by synthetic materials. *Right*, a fragment about 1 cm long, as introduced into the nutrient. *Left*, culture after 12 days' growth. (From White, P. R. *Plant Phys.*, 1934, 9.)



Histogram showing effect on growth of excised tomato roots of substitution of thiamin and of thiamin plus glycine for the yeast extract. Roots were subsequently grown in this thiamin-glycine nutrient for 4 years without diminution of growth rate, before its substitution by a slightly more complex defined nutrient. (From White, P. R. *Plant Phys.*, 1939, 14.)

tissue cultures has clearly shown, but a failure at the integrative, organismal level.

I have said that we do not yet think in terms of the cell as an independent physiological unit. Perhaps this is not important in dealing with generalized diseases such as arthritis or pernicious anemia, nor even with specific organ ailments such as kidney disease or heart failure, but in diseases of a specific type of cell, irrespective of its location, such as leukemia, multiple sclerosis, or Hodgkin's disease, it is of very great importance. It is imperative that we overcome this inhibition in our thinking.

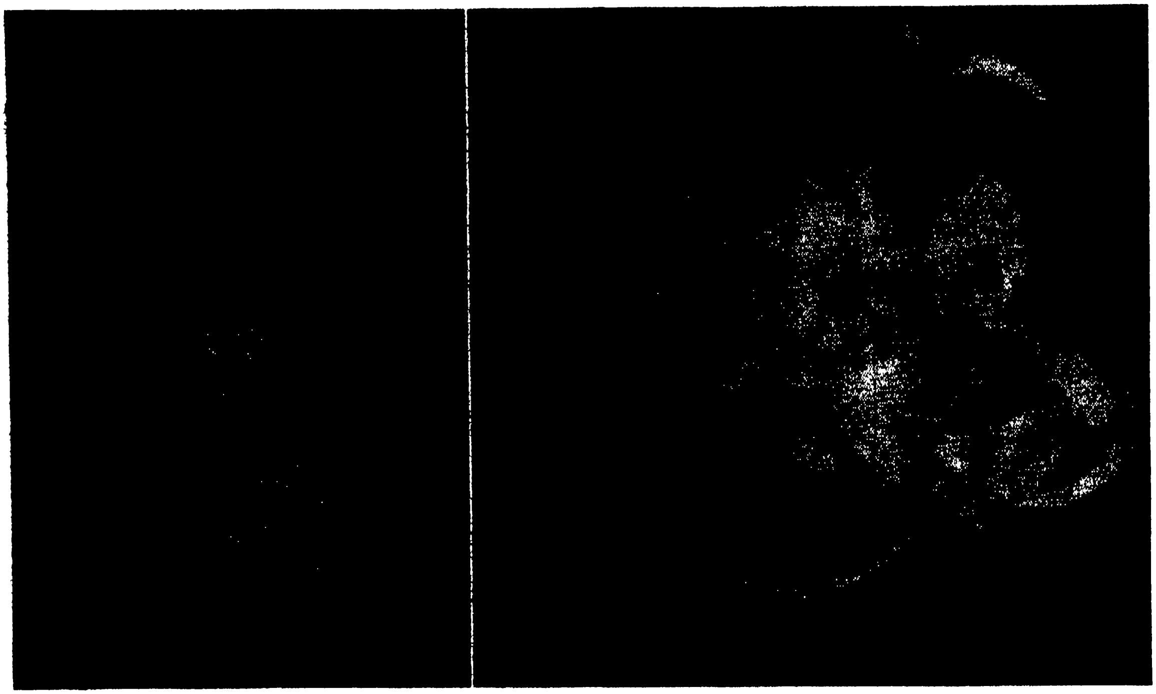
But it is not quite enough that we think of cells merely as physiological units, for if we do that we may be satisfied to study their processes in unicellular organisms, protozoa, bacteria, etc., as indeed most "cellular physiologists" have done. Study of the cell as a physiological unit will be medically useful only if we never lose sight of the fact that what we are really interested in is its function as a unit integrated into a multicellular organism. This I take to be Conklin's real reason for emphasizing the organismal viewpoint. We isolate the cell not so much because we are interested in its unitary behavior per se as because that is the only means by which we can distinguish between those facets of its behavior that are integral to itself, and consequently cannot be modified, and those which are imposed by its surroundings, and therefore *can* be modified, potentially to the benefit of the organism.

This is, therefore, the philosophy of the tissue culturist, a philosophy which permits a plant physiologist to study cancer.

One of the grave disappointments of the last generation has been the relatively minor contributions that tissue culture has made to our understanding of cancer. At the time the technique was first being developed in the early 1910s, it seemed to hold such very great promise. It was early discovered that cancer cells would usually liquefy plasma more rapidly than normal cells, but it soon became evident that epithelium in general tends to liquefy plasma. Lipman and others verified with tissue cultures what had already been determined on tissue slices, that tumor cells have a somewhat different level of anaerobic glycolysis. The higher sensitivity of cancer cells to X-rays, long known from clinical observations, was verified in tissue cultures by Strangeways and others. These were not new facts. Perhaps the most important contribution that tissue culture has made so far is the apparent demonstration by Gey, and perhaps by Earle (depending on one's interpretation of the results), that normal cells can be spontaneously changed into cancer cells without apparent cause. This last observation has not been verified by others and is open to alternative explanations. It represents no more than a proof (if it be one) of something we have long suspected of being true, but it does give us a means of studying the phenomenon and ultimately of unraveling the tangled web of causation behind it.

ALL this may seem a bit remote from plant physiology in general and water movement in plants in particular. The remoteness is not an illusion, but it is a matter of history. My studies in water movement, while a natural outcome of certain phases of my root studies, which were themselves a means toward my more basic goals of tissue nutrition, were nevertheless a *divertissement*, a temporary excursion into green pastures walled off from my main field. That main field is and always has been the study of cancer.

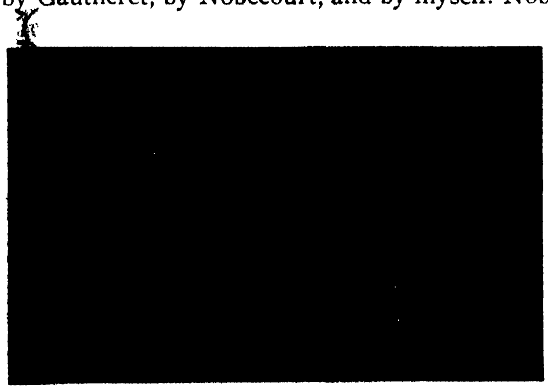
My own first approach to the fields of both cancer and of tissue culture was made a little more than twenty years ago. "Crown gall" had long been known as a "plant cancer." Twenty



Two cultures of a hereditary tumor from the hybrid *Nicotiana langsdorffii* × *N. glauca.* That at the left was grown undivided for 20 weeks on a nutrient made semisolid by addition of agar. It has remained undifferentiated. That at the right was kept for 10 weeks on the agar nutrient and then transferred for 10 weeks to a nutrient identical except for the omission of the agar, resulting in the culture being immersed in the nutrient fluid. Under these conditions it has formed highly organized stems and leaves. (From White, P. R. *Bull. Torrey Bot. Club,* 1939, **66.**)

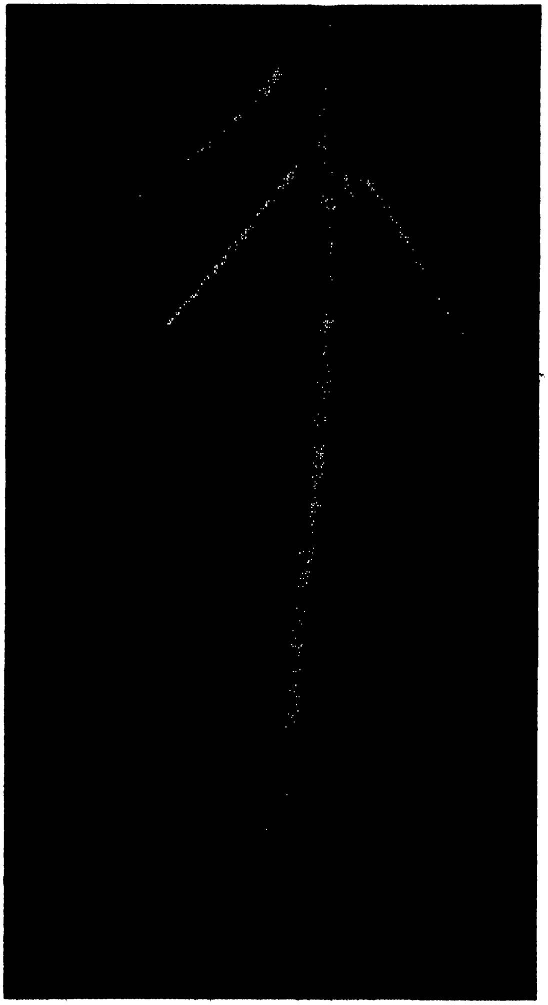
years ago the desirability of growing crown-gall tissue in vitro for more accurate control and closer study was already well recognized. The possibility of accomplishing such cultures seemed remote because of the presumed universal presence of crown-gall bacteria in the tumors. If the phenomenon of tumor inception in plant tissues was to be studied in detail, it seemed imperative to devise methods of growing not tumor tissues but normal tissues. These could then be treated, perhaps with bacterial products, to transform them into tumor tissues in vitro without the cultures being overrun with bacteria. Such was the reasoning. Methods for growing such normal plant tissues were not available at that time, and the task of developing these methods fell to me.

The cultivation of plant tissues proved difficult, slow to master, but not impossible. Past history had shown many of the things which could not be done with facility. It was clear that the basic problems were nutritional ones, and that these problems would be most easily solved by study of rapid-growing, perennially meristematic organs, of which the root tip presented a prime example. It was for that reason that preliminary work was concentrated not on crown gall itself, nor even on tissues commonly subject to crown-gall infection, but on sound, healthy root tips. The preliminary goal was essentially reached in 1939 by the perfection of synthetic nutrients for excised root tips by Robbins, by Bonner, and by myself, and the simultaneous extension of the use of nutrients of this sort to the cultivation of less organized tissues by Gautheret, by Nobécourt, and by myself. Nobé-



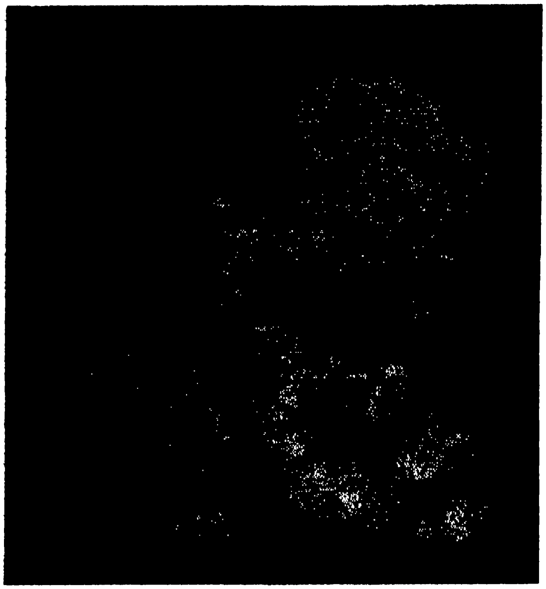
Stems of *Vinca rosea* into which were grafted fragments of tissue derived originally from crown-gall tumors rendered bacteria-free by fever therapy (Braun) and grown for 1 year as tissue cultures. Massive sterile tumors result from such grafts. (From White, P. R. *Amer. J. Bot.* 1945, **32.**)

court, like myself, was led to investigate the question originally by an interest in crown gall. Gautheret has been led by somewhat different paths to make very important contributions to the crown-gall question. Subsequent developments have all rested on the variety of procedures published in 1939.



A sunflower plant inoculated with crown-gall bacteria just above the cotyledons when a seedling. Secondary tumors have appeared at various points above the site of inoculation. These are typically free of crown-gall bacteria. From them bacteria-free tumor tissue cultures can be isolated. (From White, P. R., and Braun, A. C. *Cancer Research.* 1942, **2.**)

It was toward the end of this preparatory decade that I jumped the fence outlining my original problem to examine briefly a quite unrelated phenomenon, the secretory capacity of excised roots under in vitro conditions. It was for this unplanned departure from the straight and narrow



A culture of carrot maintained in vitro for 6½ years. Cultures of this strain when "habituated" by cultivation on an idoleacetic acid medium and then transferred to an IAA-free medium take on properties closely resembling those of tumor tissues. (From Gautheret, R. J. 6th Growth Symposium, 1947.)

path that I unexpectedly garnered the award (should I say "the wages of sin"?) that is responsible for my writing this present article. I have not returned to that subject since. I might have answered your editor's request by writing a paper on "Studies on Water Movement in Plants Which Have Not Been Made in the Past Decade." I cherish the delusion that what I am writing is more important.

Science often progresses by a happy concatination of seemingly unrelated events. At just about the time our technique for cultivation of normal tissues, on which we had spent ten years, was developed to a point of real usefulness, an unexpected break occurred in our favor. In 1941, Armin Braun rediscovered the bacteria-free secondary tumors which arise at a distance from the primary focus of infection in cases of crown gall of certain plants in the family Compositae. These had been described by Erwin F. Smith a quarter of a century earlier, but erroneously interpreted. This discovery immediately placed at our disposal bacteria-free crown-gall tissue, of what soon proved to be considerable variety, for in vitro study in tissue cultures without the expected necessity for going through the intermediate stage of working with normal tissues. Followed quickly by Braun's studies on thermal behavior of crown gall (likewise an expansion of earlier incomplete and neglected studies by Riker) and by De Ropp's redemonstration (again an observation of Smith's, misinterpreted by him) of the bacteria-free nature of many primary crown-gall tissues, these investigations unexpectedly placed a whole range of crown-gall tissues within the scope of our recently acquired culture methods. We thus now have at our command (1) normal tissues which can be caused to become tumorous under in vitro conditions. (This Gautheret appears to have accomplished in what he calls "habituation" brought about by treatment with naphthalene acetic acid, a substance analogues of which can be obtained from crown-gall bacteria.) These can be used as bases of comparison with crown-gall tumor cultures. We also have at our command (2) tissues which are tumorous at the time of isolation (crown-gall secondaries) and can therefore serve for comparison with normal tissues, for comparison with tissues made tumorous by in vitro treatment, and, last, for treatment in vitro with procedures which we may hope will give us the converse, "curing" a tumorous tissue, rendering it again normal in behavior.

We are only just beginning this phase of our studies. It would be a mistake to suppose that we have yet even approximated these goals. There are endless details to be worked out. We may even hope for entirely unexpected leads in the future. Nevertheless I think we have shown that since cancer is an aberration of cells, and since this sort of aberration can and does occur in many different sorts of organisms, including plants, plant physiologists have a not inconsiderable contribution to make to the study of cancer. Although an AAAS award for a paper on "Root Pressure, an Unappreciated Force in Sap Movement" may seem a strange basis on which to build a career in cancer research, there can be no doubt that the heartening effect of such a symbol of approbation from one's peers does contribute mightily toward the maintenance of productive energy, interest, and accomplishment.

◆

PHOTOGRAPHIC EXHIBITIONS

Entries in the *Second Annual International Photography-in-Science Salon* will be received by The Scientific Monthly July 26–August 16, 1948, inclusive. They will be judged on August 21, and those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, September 1–21. Already booked through April 1949, showings after that date may be arranged.

The Biological Photographic Association will hold an exhibition in Philadelphia, September 8–10, in Houston Hall, University of Pennsylvania, at which prints, color transparencies, and motion pictures in the field of biological photography will be shown. There will also be organized symposia and demonstrations.

PROTOPLASMIC CONTRACTILITY

PRESSURE EXPERIMENTS ON THE MOTILITY OF LIVING CELLS

DOUGLAS MARSLAND*

Professor Marsland, Department of Biology, New York University, shared the Annual Thousand Dollar AAAS Prize in 1941 with Dr. Dugald E. S. Brown, director of the Bermuda Biological Station for Research, and Professor Frank H. Johnson, of Princeton University, whose article also appears in this issue.

THE pressure of our atmosphere, which bears directly down upon all land-dwelling animals and plants, does not impose a very heavy burden. At sea level, in fact, this pressure amounts to only 14.7 pounds per square inch; and even if an animal migrates from the highest mountain to the deepest cave, the pressure changes are rather small. Such small variations are important indirectly, because they drastically alter the quantity of oxygen and other gases that penetrate into the protoplasm and body fluids. But the direct effects of pressure upon the vital processes of land-dwelling organisms are almost negligible.

The deep sea, however, imposes a different set of conditions. The cumulative weight of the heavy overlying water increases with the depth, and the pressure keeps mounting, roughly at the rate of one pound for every two feet below the surface level. At a depth of three miles, where a wide variety of species are known to live (Fig. 1), the pressure has climbed to almost 8,000 lbs./in^2. Such pressures so drastically modify the vital activities of an organism that truly deep-sea forms cannot survive at surface pressure, and, conversely, a surface form is likely to die if suddenly exposed to deep-sea conditions. Accordingly, we are faced with the problem of understanding how pressure affects the fundamental life processes of the protoplasm. This problem is important because life originated in the aquatic environment, and pressure has played a significant role in determining the evolution of aquatic species.

It is of critical importance in cell physiology to acquire an understanding of the various chemical and physical reactions which go on continually in the living protoplasm. These metabolic reactions provide energy without which the vital activities of the animal must come to a halt. The physiologist continues, therefore, to expose cells to many different experimental conditions. He uses high and low temperatures, much and little oxygen, or all manner of toxic and nontoxic compounds—knowing that the resulting changes in the behavior of the cells are bound to reveal something about the fundamental chemical and physical processes of the protoplasm. The influence of high pressure upon the activities of living matter has not received an adequate share of attention, although pressure experimentation has gradually gained momentum since 1880. This modern period in the field of pressure physiology began with the important deep-sea dredging expedition of the *Talisman*, which proved that many organisms live at depths exceeding eight miles and at pressures approaching 15,000 lbs./in^2.

The protoplasmic reactions, of course, are numerous and very complex. A starting point can be reached, however, by considering how pressure may influence any given physical or chemical reaction without reference to where it may occur. If we take, for example, the freezing of water into ice, at a temperature of 0° C and at atmospheric pressure, the water-ice system is at equilibrium. Under these conditions freezing and thawing are exactly balanced so that no net change takes place; but, if the pressure is raised, the ice will melt. In other words, pressure is capable of shifting the equilibrium of the water-ice system, and it has been found that pressure exerts a similar effect upon reversible reactions in the protoplasmic system.

An analysis of the action of pressure upon physical and chemical reactions leads to the general conclusion that volume changes are of critical importance. Pressure invariably opposes any change that increases the volume of the system, while simultaneously it favors any change that decreases the volume. In the water-ice equilibrium, for example, water expands as it freezes and pressure opposes this change, whereas thawing involves shrinkage of volume, which is favored by the pressure. Moreover, since volume changes occur in a majority of physical and chemical reactions regardless of whether these occur in living or in nonliving

---

* The author wishes to make grateful acknowledgment to Dr. Dugald E. S. Brown, director of the Bermuda Biological Station for Research, who collaborated directly in the work on amoeboid movement and in the studies on the equilibria of various inanimate gels, and who has been a generous counselor throughout all the other work.

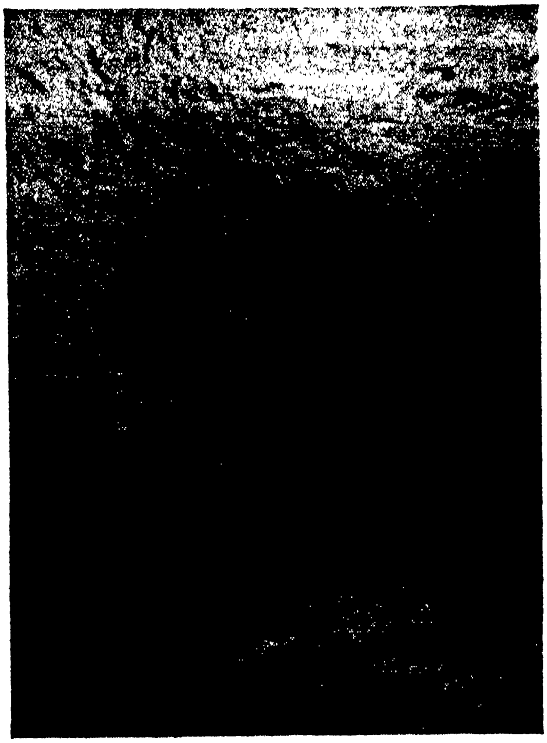


Fig. 1. Remarkable deep-sea photograph showing bottom of the ocean at a depth exceeding 1 mile (2,120 meters). Note large unidentified fish and the numerous tracks and castings in the sand. This and other deep-sea photographs taken by the staff of the Woods Hole Oceanographic Institution under the leadership of Maurice Ewing indicate that a wide variety of species inhabit the ocean depths.

matter, it is not surprising to find that the metabolic reactions of living cells are very sensitive to changes in the environmental pressure; this explains why the visible activities of cells are so drastically modified according to pressure conditions.

In the protoplasm one important reaction, the sol-gel reaction, goes on more or less continuously. For many years physiologists have noted that protoplasm keeps changing back and forth from the sol state, which is almost as fluid as water, to the gel state, which displays a consistency like firmly set gelatin. The functional significance of these protoplasmic gelations was not understood very clearly, however, until quite recently, and the experiments in which living cells have been exposed to high pressure have been very useful in clarifying this classic problem. It was soon found that the gelling of the protoplasm, like the freezing of ice, is drastically inhibited when the pressure is raised. When firmly gelled, apparently the protoplasm possesses contractile powers that enable the cell to change shape and display motility. And, conversely, when the protoplasm is liquefied, as by the solling action of high pressure, contractility is lost and the cell becomes immobile. This, in short, is a general conclusion that has emerged from the pressure experiments. But since our purpose is to describe the results in more detail, main attention will be given to amoeboid movement and to the movements of dividing cells, with a brief consideration of the motility of certain pigment cells.

Amoeboid movement. Perhaps the most primitive form of locomotion is amoeboid movement, which can be observed directly in *Amoeba* and related one-celled animals. This movement is not restricted to the locomotion of such lowly forms, however. It is displayed by many cells in the human body, such as the white blood cells—which leave the capillary vessels in search of infective bacteria—and many connective tissue cells, which also are free to wander through the tissue spaces. Moreover, in the embryo, before the tissues assume their adult form, many embryonic cells employ amoeboid movement as they migrate to their proper stations in the body, where final differentiation will occur.

The fascinating simplicity of amoeboid movement can be observed to best advantage when a microscope is brought to bear upon a large *Amoeba* in which only a single pseudopodium is being formed (Fig. 2). Here one sees the smooth flowing of the protoplasm, which carries the nucleus, vacuoles, and granules forward through a central channel of the cell, in the direction of the advancing pseudopodium. It is seen also that the superficial part of the protoplasm just under the surface

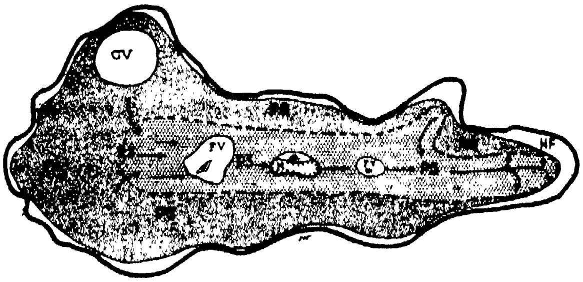


Fig. 2. Diagram of *Amoeba* showing the mechanism of amoeboid movement. Forward-streaming of the fluid protoplasm, or plasmasol (*PS*), is caused by a contractile force exerted by a surrounding layer of gelated protoplasm, the plasmagel (*PG*). This streaming carries the nucleus (*N*), food vacuoles (*FV*), and other granules out into the actively extending pseudopodium (*PS*). New gel keeps forming at *RG*, reinforcing the wall of the pseudopodium as it lengthens, and the old gel at the other end (*RS*) of the *Amoeba* keeps undergoing solation, forming new plasmasol. Other structures shown are the contractile vacuole (*CV*), which serves to pump excess water from the cell, and the hyaline fluid (*HF*), which exudes through the contracting plasmagel, especially at ends of the pseudopodia.

membrane of the cell does not participate in the central streaming; rather, it maintains a fixed position and seems to form a wall that guides the flowing protoplasm outward through a definite channel. It is thus clear from direct observation that the protoplasm of the *Amoeba* is differentiated into two parts: the plasmasol, the fluid, deep-lying part, which flows while the *Amoeba* is moving; and the plasmagel, the firmly gelated surface part which encases the plasmasol. A cycle of sol-gel changes is necessary if the amoeboid movement is to continue. New gel is added to the walls of the plasmagel tube just behind the advancing tip of the pseudopodium (Fig. 2), and the old gel at the opposite end of the cell is continually transforming into new plasmasol, which joins the forward stream. The flowing of the plasmasol appears to originate from a contractile force exerted by the surrounding tube of plasmagel. However, prior to the pressure studies this "contractile hypothesis" was supported by very little experimental evidence.

The problem of observing the *Amoeba* while it is being subjected to high pressure requires a special chamber, made of stainless steel. Very strong glass (Herculite) must be used in the windows, since otherwise the thickness of the windows would not permit the cells in the chamber to lie within reach of the lens system of the microscope. Even so, it is necessary to use a special objective (Leitz, U.M. 4) which has an exceptionally long working reach; and since the cells tend to fall to the bottom of the chamber, an inverted microscope must be employed. Cells are observed at a magnification of 600 diameters. Various pumps may be used to build up the pressure in the chamber, although in most of the present experiments an automobile jack of the hydraulic type was modified to serve as a pump.

If we start with an actively flowing *Amoeba* in the chamber and observe what happens when the pressure is suddenly raised to a high level (8,000 lbs./in^2.), the effect is quick and dramatic. Immediately the elongate pseudopodium collapses and soon the whole cell rounds up into an immobile sphere. Apparently, pressure induces a drastic liquefaction of the plasmagel, and now the protoplasm behaves like any small droplet of fluid in which a rounded form results from tensional forces at the liquid surface.

While the pressure is maintained no sign of streaming can be detected in the rounded *Amoeba*, and gradually the nucleus and other visible particles in the protoplasm begin to fall into the lower half of the cell. As soon as the pressure is released, however, activity begins again. Within two minutes, one or more pseudopodia begin to thrust out from the cell, and very soon locomotion becomes very active again.

Not all parts of the plasmagel of the *Amoeba* are equally susceptible to the liquefying action of high pressure. In fact, the newly formed gel near the pseudopodial tip displays a distinctly weaker structure than the older gel in and near the body of the cell. Such a difference shows very plainly when more moderate pressures are applied to specimens with very long slender pseudopodia—a form assumed by *Amoebae* when placed in distilled water rather than in natural pond water. When the pressure approaches 6,500 lbs./in^2., the first collapse and rounding of the protoplasm involves only the extremity of each pseudopodium (Fig. 3). Even at 6,500 pounds, however, the whole cell gradually becomes rounded and immobile unless the high

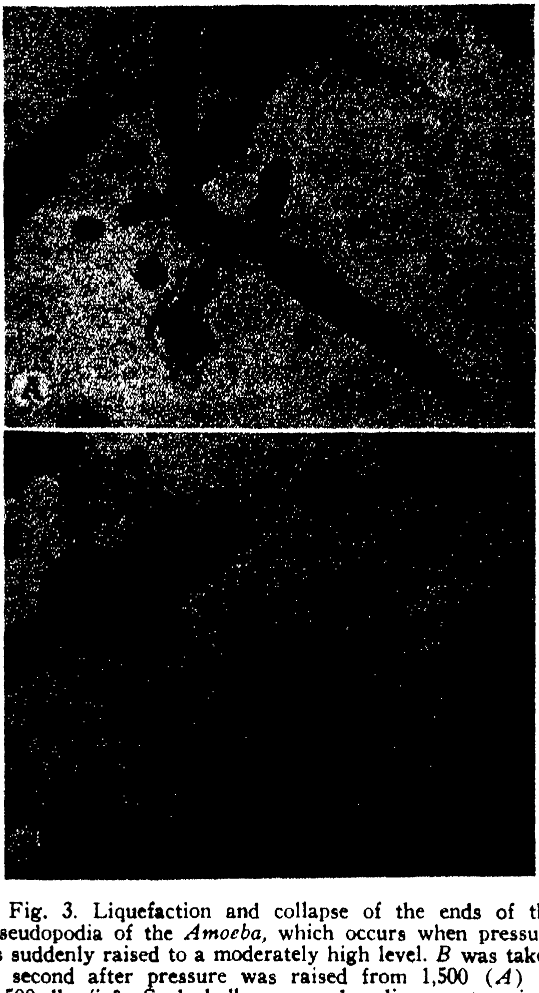


Fig. 3. Liquefaction and collapse of the ends of the pseudopodia of the *Amoeba*, which occurs when pressure is suddenly raised to a moderately high level. *B* was taken 1 second after pressure was raised from 1,500 (*A*) to 6,500 lbs./in^2. Such bulbous pseudopodia are transient, however, since the whole cell soon rounds into an immobile sphere if the pressure is maintained. At 6,500 lbs./in^2. the strength of the plasmagel is reduced to less than 20 per-cent of the atmospheric value.

pressure is released within a minute or two. Thus the newly formed gel near the advancing end of a pseudopodium, being relatively weak, disintegrates quickly even at moderate pressures, whereas the solation of the older gel is less drastic, and the rounding of the protoplasm occurs less rapidly.

Quantitative measurements of the pressure effect upon the gel strength in the *Amoeba* have also been made, and these data support the conclusion that movement is inhibited in proportion to the weakening of the plasmagel structure. For these experiments a special chamber was constructed in which the pressure could be maintained while simultaneously the *Amoebae* were being whirled in a centrifuge. Under centrifugal force the heavier elements in the protoplasm, such as the nucleus and visible granules, tend to be thrown into one end of the cell, whereas lighter components, such as the contractile vacuole and fat droplets, tend to be buoyed into the other end (Fig. 4). Such a displacement of particles through the protoplasm cannot occur, however, if the gel strength is too great. If the gel structure is weakened by pressure, the rapidity of the displacement provides an index of the residual strength. Thus it is possible to show that, under pressures adequate to induce a rounding of the *Amoeba*, the protoplasm becomes almost as fluid as water.

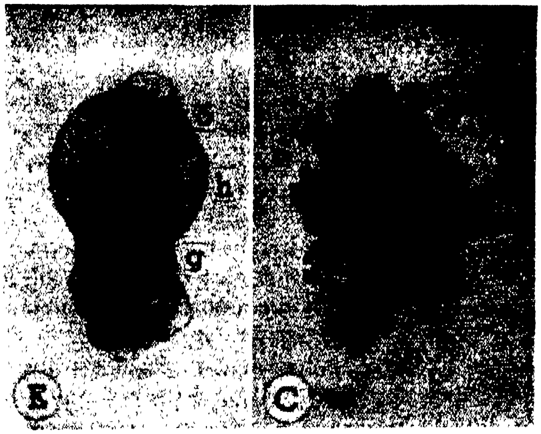


Fig. 4. Under high pressure the gelated protoplasm of the cell undergoes liquefaction and loses its contractile power. These two cells (*Amoebae*) were whirled simultaneously in the same centrifuge, but *E* was compressed at 8,000 lbs./in^2.; whereas *C* was at atmospheric pressure. In *C* the protoplasmic gel is too stiff to allow any displacement of imbedded particles, but in *E* all the heavier granules (*g*) have been thrown into the lower part of the cell, whereas the lighter oil droplets (*o*) have been buoyed into the upper part, leaving a clear, or hyaline, zone (*h*) between. Note that the nucleus (large, dark, round body) is heavy and goes to the centrifugal end, whereas the contractile vacuole (round clear vesicle in the oil cap) is much lighter.

The pressure experiments demonstrate, then, that a gelation of the protoplasm is essential to the execution of amoeboid movement, and that complete paralysis ensues when the gel structure of the protoplasm is destroyed. The plasmagel of the *Amoeba*, which has the form of a tube enclosing the plasmasol, exerts a contractile force upon the plasmasol, causing it to flow outward toward the tip of the pseudopodium where the gel strength is at a minimum. To sustain the movement there must be a cycle of sol-gel changes whereby new sol keeps forming posteriorly and new gel keeps building up anteriorly; if these reactions are deranged by pressure or other means, amoeboid movement cannot occur.

Cell division. The importance of cell division cannot be overemphasized, since this is the only known method by which new cells come into being. Each cell of an animal or plant represents the current link in an uninterrupted chain of cell divisions which has extended back into ancient times and which may extend into the infinity of the future.

Cell division is best observed in the very early embryo when a single large cell, the fertilized egg, begins to divide (Fig. 5) and continues redividing until the many cells of the adult body have been formed. In fact, these early cleavage divisions occur on a very precisely timed schedule. Given the exact temperature and the time at which the sperm are added to a batch of unfertilized eggs one can plan an experiment knowing precisely when each of the successive cleavage divisions will occur.

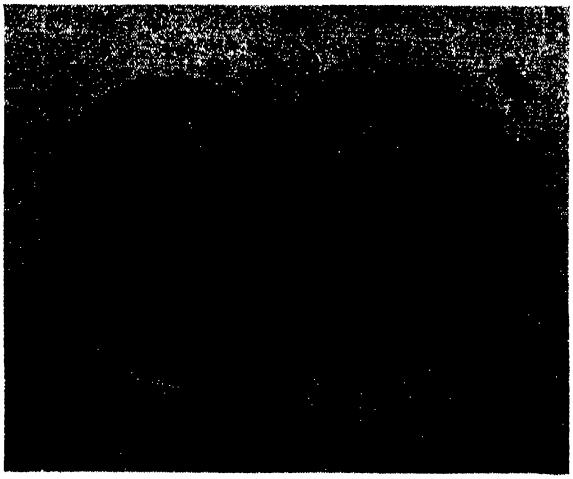


Fig. 5. Dividing egg cell (mag. × 1,360). This is the first of the many divisions by which the numerous cells of the developing embryo are to be formed. The chromosomes, spindle, asters, and other important structures may be identified by comparing this photograph with Figure 6. (Courtesy, General Biological Supply House.)

Subsequent to insemination, the sperm head, or male pronucleus, penetrates into the egg cell, migrates toward the center of the egg, and finally fuses with the nucleus of the egg (female pronucleus). Thus the nucleus of the fertilized egg is a duplex body, made up half and half of chromosomes of maternal and paternal origin. And, since each chromosome perpetuates itself throughout the successive cell divisions in the developing embryo, each of the parents has an equal potentiality in determining the heritable qualities of the new individual.

Focusing attention on the first cleavage division at the time when the cytoplasm of the egg cell is pinching itself into daughter halves, we find a configuration such as is shown in Figures 5 and 6.

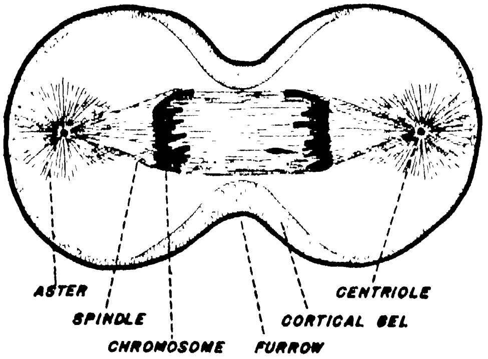


Fig. 6. Diagram of a dividing egg cell, showing important structures visible when the cell has been fixed and stained.

Already the chromosomes have divided and the daughter chromosomes have been moved toward the ends of the spindle. Also, the cleavage furrow has made good progress in the work of cutting through from the equator of egg toward the center of the spindle. These movements can be observed directly in the living egg, and an interesting problem is to find out how they are effected.

The egg cell is definitely comparable to the *Amoeba* in that the cytoplasm shows a similar differentiation into a gelated layer at the surface and a much more fluid solated portion occupying the central region of the cell. In the unfertilized egg, however, the plasmagel is not very firmly set. At this time it is quite easy to effect a displacement of pigment granules which are embedded in the cortical gel (as well as in the deeper sol) when the cells are placed in a relatively weak centrifugal field, such as 1,700 times gravity. But in the fertilized egg, especially when the time for furrowing approaches, the strength of the cortical gel undergoes an increase that is more than tenfold. Now a centrifugal field of 17,000 times gravity is not strong enough to budge the cortical granules (Fig. 7). Moreover, the gel strength of the cortex reaches a maximum in a bandlike region which encircles the equator of the cell where the cleavage furrow will soon appear. As one observes the dividing egg, this gelated cleavage girdle (heavy stippling in Fig. 6) appears to contract, constricting the cell like an hourglass and finally cutting the egg into daughter halves. This conclusion is well substantiated by experiments in which dividing cells are subjected to the solating effects of increased hydrostatic pressure.

A dividing egg in the pressure chamber at the time when the hourglass form is well developed can be observed while the pressure climbs. When the pressure reaches 5,000 lbs./in^2. or more, the progress of the furrow halts abruptly (Fig. 8). Then the furrow begins to recede. The recession is relatively slow at lower, but quite rapid at higher, pressures, and within 2–4 minutes the egg has reassumed its original spherical form. The retreat of the furrow reverses itself, however, as soon as the pressure is released (Fig 8). In fact, one can induce the furrow to advance and retreat alternately for several times by releasing the pressure after each compression. Finally, however, if the furrow is held back for more than 15 minutes, it will fail to cut through when the pressure is released. Then no refurrowing will occur until it

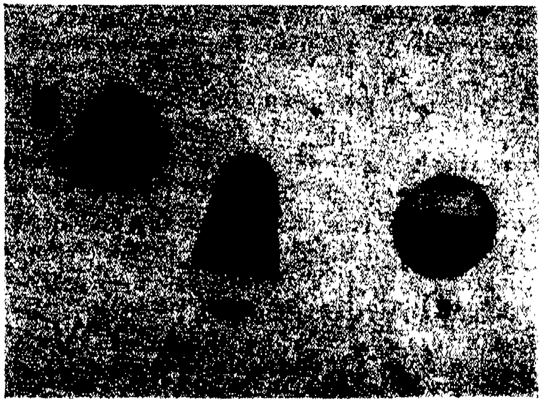


Fig. 7. A tenfold increase in the strength of the cortical gel of the egg cell occurs just before cell divides. All these cells were centrifuged simultaneously at the same high force (17,000 × gravity). In the unfertilized eggs (*A*) the structure of the cortical is weak, allowing the pigment granules to be thrown into a black, densely packed mass at the heavy end of the stretched cell and completely clearing the hyaline zone. But in the fertilized egg (*B*) 5 minutes before the furrow will appear, the pigment granules remain fixed in the strongly gelated cortex so that (*a*) the cortex of the hyaline zone remains granular, (*b*) a dense black pigment mass is *not* formed, and (*c*) the cell remains unstretched by the high centrifugal force.

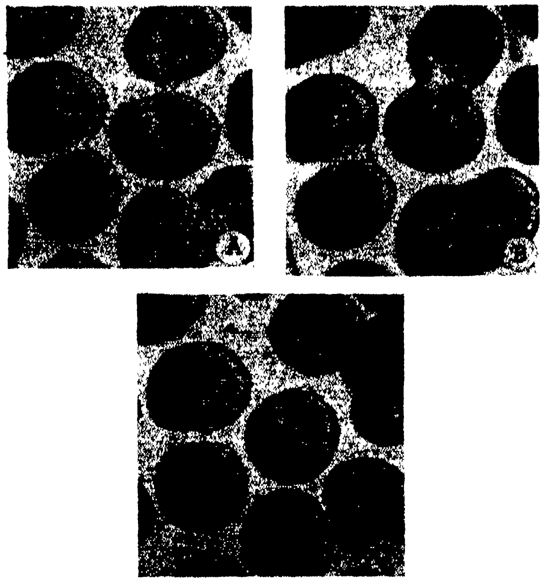


Fig. 8. The retreat of the cleavage furrow which occurs when a dividing egg is compressed at pressures above 5,000 lbs./in^2. In *A*, at atmospheric pressure, the advancing furrows have cut almost completely through, so that the daughter cells are connected by mere strands of protoplasm. Yet in *B*, 2 minutes after the pressure was raised to 6,500 lbs./in^2., there was a very marked retreat, which, however, was reversed as soon as the pressure was released (*C*). Note particularly the series b_1, b_2 *and* b_3, which shows the furrow most clearly. The fertilization membranes of these eggs were removed to allow for a clearer view of the furrows.

is time for the second cleavage, whereupon a double furrowing usually occurs; thus three or four daughter cells are simultaneously formed from the single parent cell.

At pressures below 5,000 lbs./in^2., the furrow does not recede, but the rate at which it cuts through the egg is definitely retarded. At the 4,000-lb. level, for example, it takes 9 minutes for the furrow to pass from the equator to the spindle center, instead of the normal atmospheric time of 3 minutes (at 20° C). In fact, the curve obtained by plotting this retardation as a function of pressure fits very closely when superimposed upon a plot of the cortical-gel strength in relation to pressure, as measured by the pressure-centrifuge technique at the time when cleavage is occurring (Fig. 9).

The simplest and most direct explanation of the pressure effects upon the various egg cells that have been studied is that cleavage results from the contraction of the equatorial girdle of gelated protoplasm. In the gelated state, apparently, the protoplasm possesses contractile properties lacking in the solated system. Therefore, the forces which enable the cell to change its form during both cleavage and amoeboid movement are fundamentally similar in nature.

That the spindle and asters of the dividing cell (Fig. 6) are also gel structures which are susceptible to dissolution under high pressure appears evident from the work of Daniel Pease. No trace of the spindle and asters can be found in egg cells which are killed and stained immediately after a short exposure to pressures of 5,000–6,000 lbs./in^2.; this level of pressure is adequate to abolish the displacement of the daughter chromosomes from the center toward the ends of the spindle area. If the cell is not killed directly after the pressure is released, several new asters begin to appear in the cytoplasm, and, if one of these asters happens to come into contact with the chromosomes, a peculiar half-spindle develops in the region between the chromosomes and the astral center. The fibers of the half-spindle become attached to some of the chromosomes, and it is only in the presence of such half-spindles that any movement of the chromosomes toward the astral center can be observed. Experiments by Pease substantiate the view that the "traction fibers" of the spindle are gel structures which effect a displacement of the chromosomes because of their contractility.

Motility of pigment cells. In the skin of many fishes and certain other animals one finds a multi-

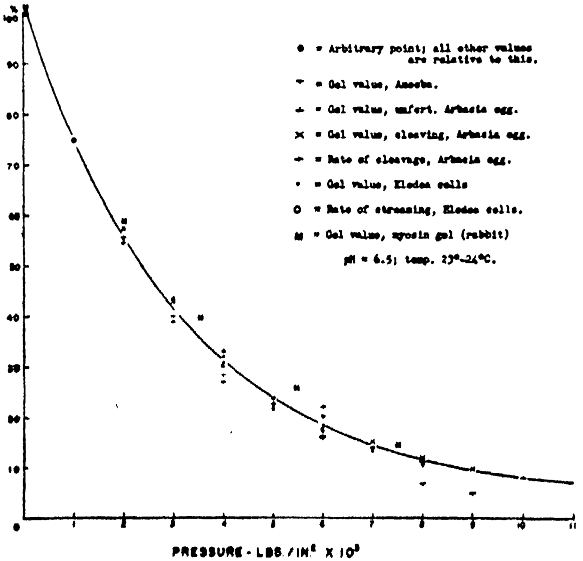


Fig. 9. Quantitative data which substantiate the conclusion that contractility is a function of the protoplasmic gelations. In each case the gel strength at a given pressure is expressed as a percentage of the atmospheric value as determined by the centrifugal methods. Also, rates of activity at each pressure are expressed on a percentage basis.

tude of heavily pigmented cells which enable the fish to change the shade and intensity of its color. By changing its own color to match the patterns of its different backgrounds, the fish can shift from scene to scene in its natural habitat without undue exposure to predaceous enemies.

Figure 10 shows a group of the black pigment cells, called melanophores, in the skin of the common killiefish (*Fundulus heteroclitus*). Here one can see the mechanism of the color change. Each cell possesses a fixed number of branches which radiate from the nucleated cell body, reaching widely out into the colorless area between the cells. If the skin is to assume a very light hue, all the pigment is withdrawn from the branches into the central body of each cell, forming a number of tiny spots too small for the eye to discern except when aided by the microscope. When the fish takes on a very dark hue, the densely pigmented protoplasm of each melanophore expands, refilling the radiating branches, and the whole skin area appears quite uniformly dark when viewed without a microscope.

Experiments performed on intact fish have proved that the metabolic reactions which initiate expansion and contraction in the pigment cells are under the dual control of the nervous and endocrine systems. More fundamentally, however, this expansion and contraction involves an alternate solling and gelling of the protoplasm, as is indicated by pressure experiments on the isolated scales of *Fundulus*. If such an isolated scale is immersed in a weak solution of adrenalin and placed in the microscope pressure chamber, all the cells are seen to be fully contracted, each into a very small, densely packed pigmented mass. As the pressure is gradually built up, one observes a graded expansion of each pigment cell (Fig. 11). At about 2,500 lbs./in². all the melanophores are about half expanded; a full expansion is not reached until the pressure climbs to 7,000 lbs./in². Moreover, the degree of expansion appears to represent a true equilibrium that can be shifted back and forth according to the pressure conditions. When the pressure is raised and lowered alternately, the same degree of expansion is observed for each specific pressure regardless of whether the pressure is falling from a higher or climbing from a lower level.

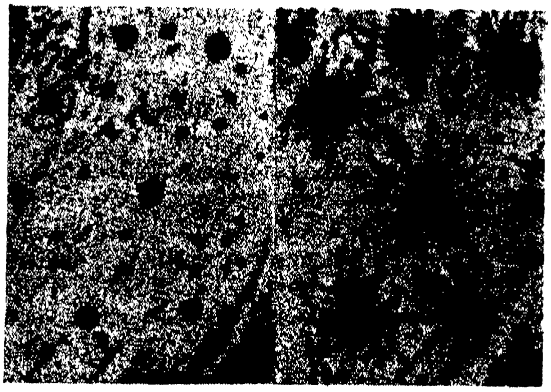


Fig. 10. Numerous black pigment cells (melanophores) in the skin of a fish, which enable the animal to change color according to its surroundings. The fish assumes a lighter hue when the pigment cells are contracted (*left*), and darkening occurs when the melanophores expand (*right*).

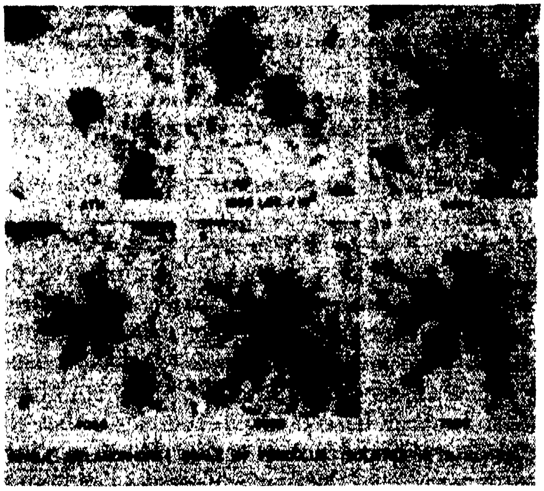


Fig. 11. Graded expansion of the pigment cells which occurs as pressure is gradually increased. This effect, apparently, results directly from the action of pressure upon the sol-gel equilibrium of the protoplasm, which loses contractility in proportion to the reduction of gel strength.

Quantitative measurements of the contractility of the melanophore in relation to the strength of its gel structure are not possible: first, because the shape of the cells precludes a free displacement of the pigment granules in the centrifugal field; and, second, because the strength of the melanophore gel system is very great—so great, indeed, that ultracentrifugal forces are necessary to displace the pigment and an ultracentrifuge pressure chamber has not yet been constructed. However, qualitative measurements of the contractility at various pressures give an excellent fit when compared to the pressure-gelation curve for other cells. For example, the half-expanded form is observed at 2,500 lbs./in²., where the gel strength (Fig. 9) amounts to 50 percent of its atmospheric value, and at 4,000 lbs./in²., where the gel strength is reduced 60–70 percent, the melanophores are expanded to about two thirds their full capacity. Consequently, it seems safe to conclude that the movements of unicellular melanophores in fish are fundamentally similar to amoeboid movement and to the cleavage movements of dividing cells. In each case contractility results from a gelation of the protoplasm, and, when such gelations are inhibited, contractility is reduced proportionately.

ALTHOUGH just one of many protoplasmic reactions has been considered here, it is interesting to contemplate how all the life processes of deep-sea organisms must be adjusted and attuned to the tremendous pressures of their native environment. Deep-sea animals encounter not only high pressures but also low temperatures—so low, indeed, that they approach the limit tolerable to living things. It must be concluded, therefore, that the metabolic reactions of deep-sea forms are modified in such a way that the living processes can still go on despite the unusual extremes of temperature and pressure. Generally speaking, temperature and pressure have opposite effects upon physical and chemical processes, and hence low temperature and high pressure produce additive derangements of the metabolic reactions. If a surface animal is suddenly exposed to truly deep-sea conditions, it experiences a double insult to its life process, and death becomes inevitable. In the course of evolution, on the other hand, natural selection has had ample time to effect a graded adaptation of certain species toward a fitness to cope with the deep-sea environment. But the conditions of temperature and pressure provide an effective barrier that tends to prevent a free migration of species up from, and down into, the abysmal depths. It is not surprising to note, therefore, from the samplings of various dredging expeditions, that oceanic species tend to display a stratification such that species recovered from moderate depths are quite different from those brought up from deeper regions.

Finally, summarizing the more specific results of the experiments on the effects of pressure on the sol-gel equilibria of various cells, it has been found that gelation endows the protoplasm with contractile properties. Thus, protoplasmic gelations are instrumental in the execution of a variety of cellular movements, including cleavage, the transportation of the chromosomes during cell division, amoeboid locomotion, and certain other protoplasmic movements that have not been considered in this account.

* * *

CRYSTALLIZATION

A midnight pool of endless depth
Is stirred in transient rhythm.
Within its warming torrents form paths
Of cloudy, nebulous lustre.
Tiny shimmering stars join in ceaseless flight
Caught in a crystal forest—
Star-bound in an ocean fantasy.

CARMEN KENNY

SOME PROBLEMS OF PLANT NUTRITION

D. R. HOAGLAND and D. I. ARNON

Professor D. R. Hoagland, of the Division of Plant Nutrition, University of California, and Dr. D. I. Arnon, of the California Agricultural Experiment Station, shared the Annual Thousand Dollar AAAS Prize in 1940.

DESPITE its rather general title, this article will be confined to observations on the nutrition of higher green plants, and especially to a limited group of these plants—those of economic interest. Discussion of the nutrition of such lower forms of plant life as bacteria and fungi is necessarily excluded from this brief review.

The frequently encountered contemporary lack of appreciation of the fundamentals of plant nutrition has an interesting historical corollary. Despite the agelong association of mankind with crops and forests, the crucial concepts of plant nutrition have been evolved very recently—a little more than a century ago. It was only in the early part of the nineteenth century that evidence was marshaled to demonstrate that plants are made of chemical elements from three sources—air, water, and soil—and, what was particularly striking, that the bulk of plant substance, usually about 90 percent of its dry weight, was made up of the three elements carbon, oxygen, and hydrogen, derived primarily from air and water, rather than from soil. The novel and revolutionary aspect of this concept had to do with the assimilation by the plant of a key element, carbon. It was shown that this was accomplished not by the traditionally known organ of nutrition—the roots—but by the leaves and other green aerial portions of the plant, under the influence of light, during a process now known as photosynthesis.

The failure of some of the most gifted observers and keenest intellects from Aristotle down to comprehend the nature of photosynthesis may serve as an excellent illustration of the importance of the experimental approach, and of the dependence on scientific developments in other fields, to the understanding of nature. No amount of the most careful direct observation of a green leaf exposed to bright sunlight has yielded a clue to its activity as the seat of carbon assimilation. It was only after the discovery of oxygen by Priestley and the elucidation of the nature of combustion and of the chemical composition of water and carbonic acid by Lavoisier, in the latter half of the eighteenth century, that the way was cleared for the understanding of photosynthesis. For this we are indebted to such men as Ingenhousz, Senebier, and DeSaussure.

In the process of photosynthesis the green plant "captures" a portion of the solar energy and, by synthesizing an almost infinite variety of organic carbon compounds, transforms the captured solar radiation into forms of energy usable by man. The green plant is the chief agent in keeping our energy supply from running down in the cycle of life. From the photosynthetic activities of the plant, as they go on now or as they have occurred in past geologic ages, we derive our supply of energy in coal, oil, or currently harvested plant products. By its marvelous synthetic chemical processes the plant yields the complex compounds upon which we depend for our sources of food or other raw materials: for the proteins or their "building blocks," the amino acids, which the animal cannot synthesize (in making these the plant also uses a simple source of nitrogen); for the sugars and other carbohydrates; for the fatty substances; for vitamins; and for other organic compounds essential to the nutrition of the animal. The green plant may indeed be regarded as the supreme chemist presiding over an ever-continuing array of organic syntheses of almost infinite variety and complexity.

The unique role of the green plant has been unchallenged in the past. In recent years, however, the developments in organic chemistry and nuclear physics have occasionally given rise to speculations as to whether this unique position of the green plant will remain without challenge in the future. Nuclear fission is suggested as the alternative source for industrial energy, and artificial chemical synthesis of food as a substitute for natural plant foods. Quite apart from the fact that neither of these contingencies has yet materialized, it is well to keep certain other considerations in mind.

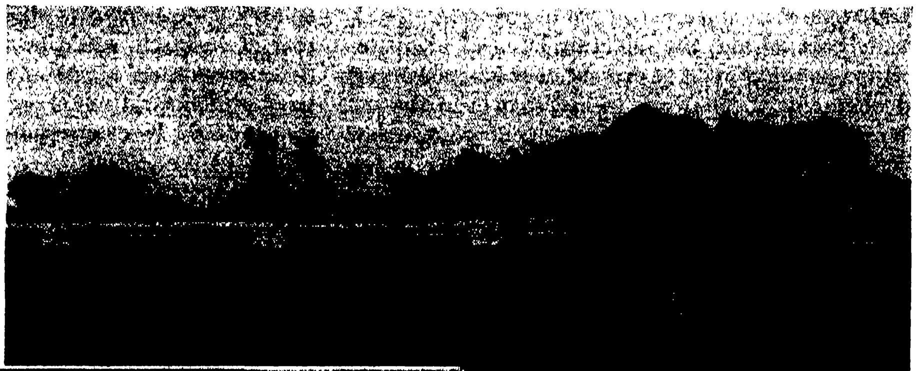
The source of solar energy used by the plant in photosynthesis is atomic transformation in the sun, and in this sense the green plant is already serving as the agent that transforms nuclear energy into forms useful and usable by man. The direct utilization of nuclear fission as a source of industrial energy is still beset by many technical, sociological, and political problems. Assuming that they will all be solved in a manner compatible with the promotion of human welfare, the eventual integration of atomic energy as a source of industrial power into the fabric of modern society appears less revolutionary in consequence than the substitution of synthetic for natural foods. It is true that in recent years several dietary essentials, notably vitamins, have been synthesized by the chemist and are indeed dispensed, often at a cost which compares favorably with that of the natural product. The substances from which they are synthesized, however, are derived from products originally made by the plant in its reduction of carbon dioxide from the air. More important, the vitamins are but a few of the essential food accessories in nutrition, and they are required in rather small amounts. When one contemplates the magnitude of the world's requirements for carbohydrates, fats, and proteins, and the almost infinite variety of natural foods on which man and other animals have learned in their evolutionary development to depend and which they cherish, the size of the undertaking becomes discernible. The chemist would be hard put to it if he had to synthesize the food for the world's population from water, carbon dioxide, and a few inorganic salts, as the plant does.

There is another aspect of this discussion that deserves mention. In contemplating the possibilities of synthetic food production, insufficient consideration is often given to the psychological and aesthetic consequences of such further mechanization of life. It would indeed be a bleak world in which our fields and forests would have to give way to huge industrial establishments, however attractively laid out, to produce the food and timber for human needs. The sustained effort being directed to the study of the mechanism of photosynthesis has not, to our knowledge, among its objectives the displacement of the green plant as the chief converter of solar energy into forms stored in organic carbon compounds.

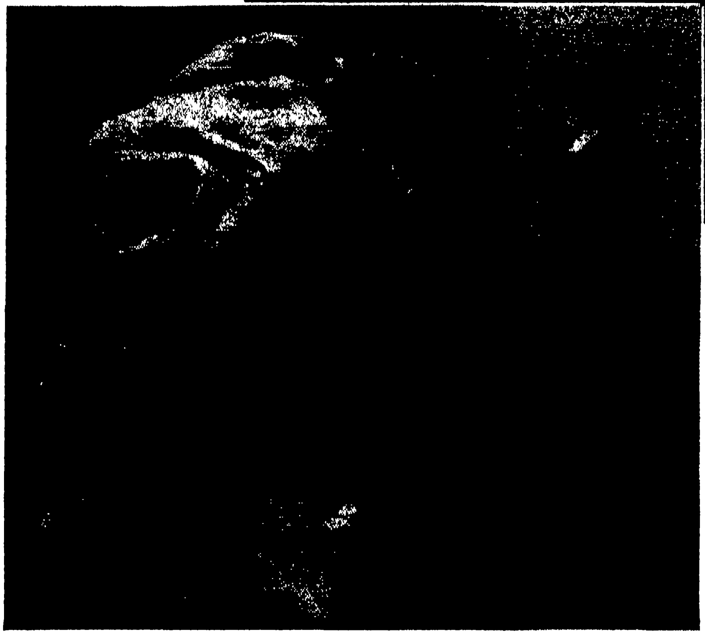
Although life and civilization depend in ultimate analysis upon the photosynthetic activity of plants, popular discussions almost always stress the requirement of plants for certain mineral elements derived from the soil. This is so because often we are able to modify the soil medium in which the plant grows, by fertilization or soil management, so that the yield of the crop will be augmented through an increased supply of mineral elements for the plant; therefore, it has become common to refer to these mineral elements as "plant foods." In reality, the food of plants is the same as that of the animal—carbohydrates, proteins, fats, etc. The difference between plants and animals in this regard is simply that the plant synthesizes its own food as well as that used by animals. The inorganic salts absolutely essential for plant growth serve, in part, as constituents of the organic substances synthesized, and in part as catalyzers of the chemical reactions involved, or else as essential components of the physical chemical system within the plant which must be maintained to provide the internal environment essential for the continued operation of the processes of growth and synthesis.

From the point of view of biological science, the most fascinating task in plant nutrition for the research worker in this field is to learn more of the chemical means used by the plant to carry out the enormous variety of chemical reactions accompanying the growth of the plant—in other words, the study of plant biochemistry. One of the authors has written elsewhere a general discussion of the needs and opportunities for research in plant biochemistry, and merely repeats his observations upon one aspect of this question, namely, that although the basic process of photosynthesis is attracting the attention of a few distinguished chemists or physicists, and although some notable contributions to knowledge of other plant processes have already been made by biochemists, nevertheless, on the whole, the extent of research and progress in the fundamental biochemistry of higher plants has not been impressive in comparison with the brilliant achievements of those biochemists who have addressed their efforts mainly to research on animal biochemistry, or sometimes to that of lower plant forms. Although it seems to be appropriate to emphasize this point of view to give a balanced estimate of the problems of plant nutrition, it is true that at present, to a large extent, plant nutrition (especially of economic plants) is studied in terms of the inorganic requirements of plants, the aspect that has been of most immediate service to those who deal with the problems of agriculture.

The general significance of this research warrants a comment. At the time of the founding of this country, most of the working population was engaged in agriculture; now, according to some estimates, perhaps less than 20 percent of the population is employed in the direct production of food. The immense economic significance of this development and its relation to the growth of modern technology will be readily apparent without extended analysis, although the differentiation between cause and effect would present the difficulties common to all such complex economic questions. This revolutionary change in proportion of the



Showing effect of deficiencies in nutrient solution on growth of the lettuce plant: *N*, lack of nitrogen; *K*, lack of potassium; *P*, lack of phosphorus; *right*, full nutrient solution.



Leaf from lettuce plant grown in full nutrient solution (*left*); from plant supplied all essential inorganic elements except boron (*right*).

total labor force devoted to agricultural production has of course involved many sciences, but understanding of plant nutrition in a broad sense, which includes study of the soil, has a prominent position in this development. As a specific illustration, we think of the use of artificial fertilizers in relation to plant nutrition and soil chemistry, with all that this implies in terms of increased yield of crops.

Another general comment on the study of plant nutrition has been frequently stressed in other discussions by Hoagland and Arnon. Many, even scientific workers in the field, however, do not always recognize this obvious point. The nutrition of the green plant (referring now to plants normally grown in soil) involves perhaps the most complex system studied in any branch of biological science. It is really an attempt to appraise the interactions of three complex systems: the plant organism with all the complexities characteristic of any living organism, together with some attributes peculiar to photosynthetic organisms; the soil system, involving questions related to geology, mineralogy, the properties of colloids, and the growth and effects on the soil of micro- or macroorganisms; and, finally, and frequently of decisive importance for plant growth, the aerial conditions —light intensity and quality, temperature, carbon-dioxide concentration in the atmosphere, wind movements, and humidity. Presiding over the whole system of soil-plant-atmosphere is water in its various forms.

To disentangle the interactions of the several systems is a challenge to any research worker. Indeed, the difficulties are so great that often the most gifted workers are likely to avoid this field of study in favor of fields where the exactness of knowledge and the possibilities of control are greater. But at this period in the world's history no special pleading is needed to make a case for any science that may aid in the production of more and better nutrition for all the human inhabitants of the earth.

Historical background of plant nutrition. As already pointed out, the scientific basis of plant nutrition, as we know it at present, was long shrouded in obscurity. That is not to say that even the ancient civilizations did not take advantage of a few ideas of plant nutrition learned empirically in the exercise of the agricultural art; for example, the frequently observed beneficial influence on the soil's productivity of the growth of leguminous plants, the often favorable influence of animal manures or of plant ashes on crop growth, and the like.

Before we learned that the plant derives its carbonaceous compounds from photosynthetic processes, there were for many centuries wholly erroneous or semimystical views of plant nutrition held by those who thought about the matter at all. One ancient idea, which indeed persisted until perhaps a little more than a century ago, was that plants absorbed their foods partly preformed from the juices of the soil. There was also the era in which natural philosophers interested in plants sought as a primary objective for some so-called "principle of vegetation."

Van Helmont (1577–1644) performed a famous experiment—and it was for its time a noteworthy one—which led him to the false conclusion that the principle of vegetation was water; in other words, that the substance of plants was made from water alone. This conclusion, however, was doubted by another investigator, Woodward, as early as 1699, when he grew certain plants in river or conduit water, in comparison with water to which garden mold had been added. His work led him to believe that plants require certain "terrestial matter" for their growth, and that water alone was not the "principle of vegetation."

This experiment was an early precursor, in a very crude form, of modern investigations which have had as their objective the determination of the inorganic requirements of plants that are absolutely indispensable to their growth, as well as an explanation of the process of photosynthesis. About 1860 the plant physiologists Sachs and Knop elaborated the so-called water-culture method by which plants are grown with their roots, not in soil, but in a solution of nutrient salts. This became one of the chief methods by which the inorganic needs of the plant were learned. It is still among the most important tools of the student of scientific plant nutrition.

It will be recalled that through an injudicious flare of publicity, many people first became aware some years ago that plants of economic interest could be grown without soil, and were led to the belief that perhaps an artificial method of plant culture, long known to the plant physiologist as a tool of research, could be employed, with further development, to supplant commercially the ordinary agricultural methods of growing plants, and that it might be of great aid in increasing the food supply of the world.

Left out of adequate consideration were questions of economic cost and difficulties of technique in growing plants commercially in this way. In fact, many of those who were most enthusiastic about the possibilities of this development had little or no basic knowledge of plant nutrition. It is true that artificial culture methods have been used to some extent in growing certain high-priced crops, especially in greenhouses. Artificial culture methods (gravel culture, usually, rather than water culture) have also been tried in a few places out of doors, such as on coral islands, where no good soil is at hand, especially as a result of war or occupation conditions, in which the competitive economic factors are not operative. But a widespread popular misconception of this subject went occasionally to fantastic extremes and betrayed a lack of even elementary knowledge of plant nutrition.

After the general concept of photosynthesis was established in the last century, investigators turned to a systematic search for the inorganic elements required for plant growth, principally by the method of controlled artificial culture, as already indicated. In the earlier stages of these investigations, however, the control of the nutrient solutions was limited. The nutrient salts used contained impurities, and there were also other sources of contamination. Thus the experiments seemed to show that, as a general mineral requirement for the growth of the higher plants studied, only seven elements provided by the nutrient medium were needed by plants in addition to carbon, hydrogen, and oxygen derived from air and water. As we now know, impurities in nutrient salts or from other sources supplied the additional needed elements. The seven inorganic elements considered indispensable were nitrogen, phosphorus, sulphur, calcium, magnesium, potassium, and iron—all present in the nutrient medium, of course, in the form of their salts. It seemed, therefore, for a long time that in total the plant required only ten chemical elements as indispensable: the seven inorganic elements provided by the culture medium as salts, and carbon from carbon dioxide, with hydrogen and oxygen from water. Our present definite knowledge that plants also require minute amounts of certain other chemical elements is a comparatively recent development of great scientific interest and practical use.

It may be interesting to introduce this aspect of plant nutrition in terms of a few personal experiences in which the authors have participated, although the first indication that the plant required minute amounts of chemical elements not included in the so-called classical list was obtained long before they began their work in this field. One of the writers had been engaged about a quarter of a century ago in studying plants growing in culture solutions in connection with a project that had as its purpose an attempt to evaluate the characteristics of the supposed nutrient solution of soils, the soil solution. Utilizing an artificial solution intended to have a resemblance to the soil solutions of a group of soils under study, marked success was had in growing barley and a few other species of plants in the artificial solution. It was then decided to use this solution for growing bean plants. These plants failed to grow after a short time, however, and the cause was not evident from any information generally available at that time.

About this time, the writer in charge of these experiments made a trip to Europe, and at the famous Rothamsted Experimental Station, near London, he was shown some experiments in which broad-bean plants were growing in different nutrient solutions, of the same composition except that certain solutions contained a minute amount of boron in addition to the other nutrient elements then considered to constitute a complete nutrient solution. The failure of the plants without boron and their good growth with boron was strikingly apparent. The technician in charge in California was instructed by letter to try to grow bean plants in the California nutrient solution in which the bean plants had previously failed, but with addition of a minute amount of boron. The bean plants then grew well. As we are now aware, boron is apparently required by all species of higher plants. The bean plants merely had a larger requirement than barley and some other species of plants.

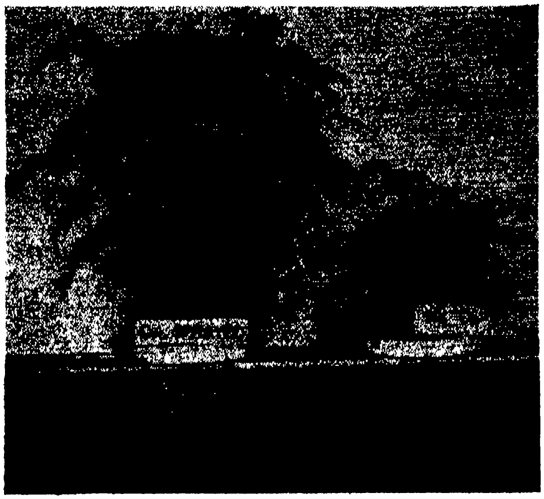
Passing over a long period of time, the laboratory with which the authors are associated had occasion to cooperate in the early thirties with W. H. Chandler, professor of pomology, on a study of a serious disease of deciduous fruit trees, often called "little leaf." Partly owing to accidental circumstances of an experiment, it was discovered that this heretofore highly obscure disease was in fact caused by a zinc deficiency and later, along with other investigators, it was discovered that a related disease, of great economic importance, the "mottle-leaf" of citrus, was also a zinc-deficiency disease. Frequently these diseases can be cured or prevented by applications of zinc or its compounds in the form of sprays or by other means. These treatments are now common practice in the orchards that show this deficiency, and if one wished to speak in terms of the financial value of the commercial application of this discovery, it might represent a very large sum. Zinc deficiencies were also independently and simultaneously observed by workers in other locations and on other species of trees. In certain areas, field crops may also be affected by zinc deficiency. Indeed, there are agricultural regions in several parts of the world in which, in some soils, various crops respond to zinc treatment, although ordinarily the zinc deficiency is of dominant interest in the culture of fruit trees.

Before the essentiality of boron and zinc was established, several investigators had secured evidence that manganese was an essential element for plants; there is also considerable evidence that copper must be added to the list of essential elements.

A much more recent development was the discovery by Arnon and his colleagues, P. R. Stout and others, that certain higher plants, if not all, when grown in nutrient solutions, also need the element molybdenum for their growth. This view was confirmed by Australian workers and, very recently, also by English and Dutch investigators. The Australian workers have also found that in some of their soils certain species of crops respond remarkably to very small applications of molybdenum to the soil.

The practical agricultural significance of investigations with the inorganic elements needed by crops in minute amounts, sometimes called the micronutrient elements, is of a great import in terms of increased production of crops in deficient soils, even though deficiencies are present only under special soil conditions and not to be expected in all, or even in most, soils. A full discussion would far exceed the limits of this article, and recently Walter Stiles has published an excellent monograph treatment of the whole subject (*Trace Elements in Plants and Animals.* New York: Macmillan, 1946).

There is, however, another general aspect of the development of research on the micronutrient elements, now of so much practical agricultural interest, which deserves recognition in this journal. That is that, correlated to the subsequent practical applications, there existed a foundation of knowledge in this field in the research of several investigators who seemed at the time to be doing work of a wholly academic nature. Among these investigators were A. L. Sommer and the late C. B.



Tomato plant (*right*) grown in purified nutrient solution, without copper; plant grown in same solution, but sprayed with very dilute solution containing copper (*left*).

Lipman, at one time working in the authors' division of the College of Agriculture. Intensive and laborious research by these investigators determined the need by plants for certain of the micronutrient elements—in work which demanded the utmost refinement of plant-culture technique. Special purification of nutrient salts, particular care in the preparation of water by distillation, and avoidance of other contaminations were all essential to the purpose of the investigation. It was necessary to show that the plant under study could not complete its life cycle without the element in question, that the element was directly required for the nutrition of the plant, and that no other chemical element could be substituted for it. Truly this was an exacting task.

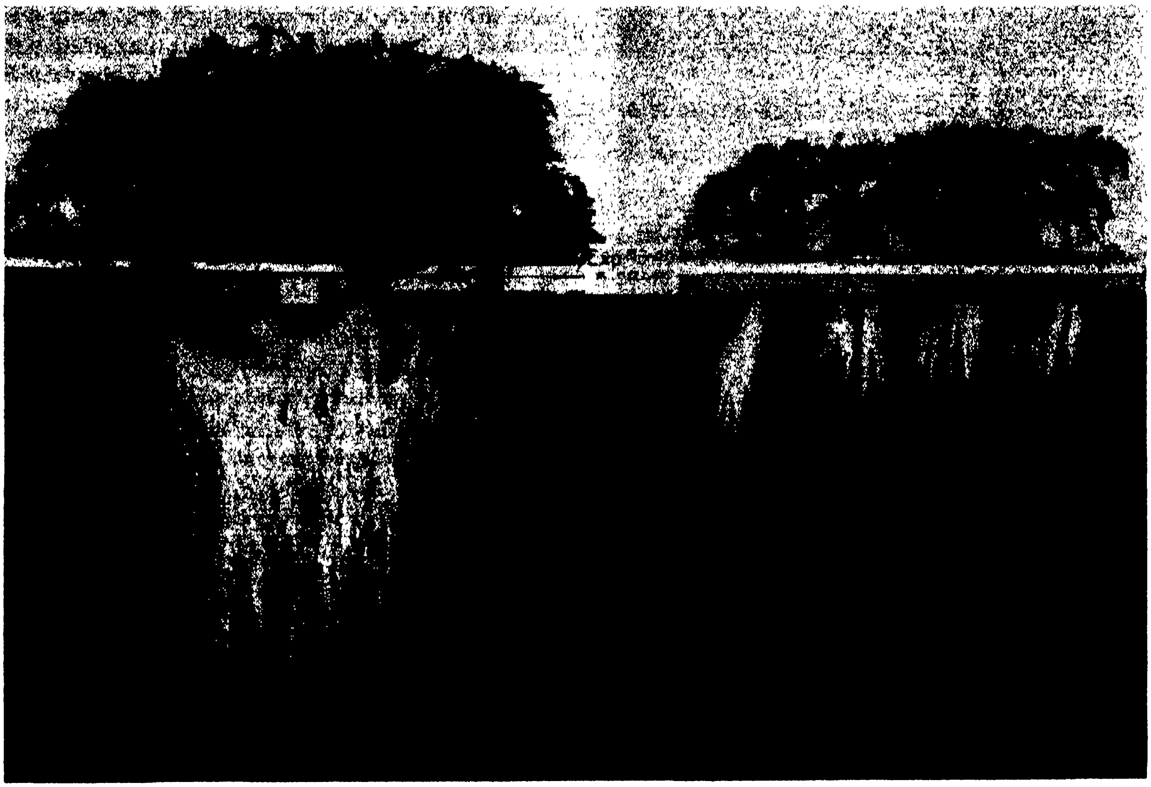
Some of the discoveries that have led to the agricultural applications of the micronutrient elements have had an element of the fortuitous, but progress even in the practical field would not have been so rapid or so assured without the preceding or accompanying basic, or so-called academic, research. Of course this is only the usual course of science and technology, but it seems appropriate to call attention to this further illustration in the biological field of science.

Interrelations between plant and animal nutrition. There is a phase of research on the micronutrient elements, as well as on elements absorbed by the plant in larger amounts, which has an application broader than that of plant nutrition as such. We are referring to the use of the plant as food for humans and animals. The seed and fruit of a plant are generally not subject to large alteration in their composition by changes in the nutrient environment, but the vegetative portions of the plant may be considerably modified, depending on the nutrient medium and its ability to supply mineral elements to the plant. Hence, there may sometimes arise in deficient soils a deficiency of certain mineral elements for the food of the animal, especially for grazing animals, such as deficiency of calcium, phosphate, iron, copper, manganese, iodine, and in a few cases even cobalt, although it is not proved that the latter element is essential to the welfare of the plant itself.

There are special regions in which these deficiencies become of economic importance in the feeding of some animals. Because of the variety of foods from many sources usually consumed, their significance for human nutrition is less apparent; nevertheless, it has been a subject for consideration. Indeed, the whole question has aroused sufficient interest to lead the Federal Department of Agriculture to establish a special laboratory at Cornell University for the investigation of the interrelations of soil, plant nutrition, and animal nutrition. And recently there has appeared an announcement that a large grant has been made to Johns Hopkins University for the study of "trace" elements, to use another term for what we have called the micronutrient elements.

These interrelations are not limited directly to mineral elements derived from the soil, and there has been some discussion of the possibility that the environmental conditions, including the mineral nutrition of the crop plant, may modify its content of organic substances important in the food of humans or animals. Some of the vitamins, especially, have received attention in this regard. To amplify the general point already made, at least as far as food plants are concerned, there is no consistent evidence that the vitamin content of the plant is mainly influenced by its mineral nutrition. On the contrary, much of the existing data support the view that the vitamin content of a plant (vitamin C, particularly, has been investigated) depends primarily on its heredity—that is, the inherent species and variety characteristics—and on the climatic conditions under which the plant is grown rather than on its mineral nutrition.

This is not at all to say that our present knowledge of this aspect of plant nutrition is adequate, but to suggest caution if the claim is made, for example, that the most nutritious food can be produced only when organic fertilizers are used instead of the artificial fertilizers so commonly employed. This is a field of discussion in which it is



Tomato plant (*left*) grown in full nutrient solution with stream of air continuously bubbled through solution; (*right*) plant grown in same solution without air.

easy to make broad generalizations without adequate scientific evidence. To obtain really convincing evidence is most difficult. The questions involved are the most complex of all, since they relate to the system soil-plant-atmosphere and animal. As stated earlier in this article, the green plant is predominantly a synthetic organism, making its own food and that used by animals, or, in more technical terms, such plants are autotrophic.

Nature of the process by which plants absorb inorganic nutrients. One of the scientific questions of outstanding interest for understanding plant nutrition concerns the nature of the processes by which plants take up from the soil or other nutrient media the inorganic elements they require for growth, or other elements present in the medium and absorbed by the plant but not indispensable to it. Parenthetically, it may be said that the plant does not absorb only those elements indispensable to its own growth, but may take up other chemical elements present in the medium, even perhaps when they are toxic to the plant itself.

The older concept often was that plants absorbed inorganic elements along with water, in something akin to a wicklike action. More recent investigations, in which the writers and others have been engaged, show that this simple concept does not express a true idea of the plant's ability to absorb mineral elements from soil or from nutrient solutions. A variety of experiments have been carried out, including those done with excised roots of plants, with storage tissue, and with certain algae which develop relatively huge cells that permit direct observation on the contents of the vacuole. (The latter is mainly a solution of salts in the species of algae investigated.) In the algae, the vacuole is clearly separated from the nutrient solution (in fresh-water algae, this is in nature pond water) in which these plants grow by a layer of living protoplasm and a cell wall. Because of the large size of these plant cells, or vesicles, direct observations are possible on the external and the internal medium of the vacuolar sap. Higher plants are made up of cells so minute that similar direct observations cannot be made, but there is reason to believe that an analogy to the large-celled plants has validity.

The amount of research given to these phenomena of salt absorption has been great, but only a

Radioautograph of tomatoes from plant given minute amount of radioactive zinc, showing the location in the tissues of the radioactive element.

few words can be said here in summary of one of the general conclusions of many experiments. It does not seem that the normal living plant acts through its root system merely like a passive wick. On the contrary, the roots can absorb mineral elements in an active sense, in that they may move the salts from a nutrient solution (which may be very dilute) into the vacuole or other internal aqueous phases of the living cells, not alone by a process of diffusion proceeding from a region of high concentration to one of lower, but also by processes in which the living cells have the remarkable ability to move the inorganic elements (at any rate, some of them) by what may be called an "uphill" process, *against* concentration gradients. This necessarily means that in so doing the plant must expend metabolic energy, and this energy must be derived from the energy of cellular respiration. There is much to indicate that this view is correct.

The practical agricultural interest of this research inheres in giving another scientific foundation for the practical observation that most agricultural plants thrive only when their nutrient medium is well aerated. For this, among other reasons, the porosity and physical status of the soil are of great importance as essential determinants of the properties of a medium in which a plant will thrive. To be sure, there are plants of a few species, like the rice plant, that may grow well in submerged soils, not well aerated. They may in part derive energy from anaerobic fermentative processes or they may not be internally anaerobic in their root systems, because there may be a system of internal aeration in the plant, with oxygen transported from the shoot—where of course it is produced in the process of photosynthesis—to the root.

There are other consequences of the view that plants have the power of active absorption of solutes, requiring a source of metabolic energy. It contributes to our scientific analysis of the interdependence of the top of the plant, where the organic metabolites are manufactured, and the activities of its roots. For the agriculturist it helps to clarify further why it is important, in understanding the so-called availability of mineral nutrient elements in the soil (or added as fertilizers), to consider that this is not merely a question of soil chemistry, but also of those conditions, particularly climatic conditions, governing the synthesis and translocation of organic compounds such as sugars, amino acids, and growth substances from the synthetic parts of the plant to the roots. There they serve for those metabolic activities associated with the processes of growth and salt absorption by the plant. The growth processes in the root also lead to an extension of contacts of root and soil which is of great importance to the absorption of mineral elements by the plant.

The use of isotopes in research on plant nutrition. The reader, even if he has only a slight knowledge of plant nutrition, may be inclined to ask: "But is there not something newer and fresher in the scientific field of plant nutrition?" Chronologically, this question can be answered in the affirmative, even though the general aspect of plant nutrition in mind can scarcely be unknown at this time to anyone interested in scientific development at all, and some pioneer experiments were made long ago with naturally radioactive elements.

We refer, of course, to the recently enlarged possibility of studying certain of the plant's chemical or physical processes with the aid of artificial radioactive isotopes or, occasionally, with stable isotopes. A new tool is provided to study plant nutrition and plant physiology, as well as animal nutrition and physiology. This tool, as far as the artificial radioactive isotopes are concerned, was stimulated by the cyclotron production in quantity of artificial radioactive isotopes of many types to permit their use in biological research, and more recently, and to a far greater extent of general applicability, as a by-product of developments associated with the making of the atomic bomb.

Radioactive elements have the same chemical properties that their nonradioactive counterparts have, but the radioactive isotope can act with the highest degree of sensitivity as a "tag," or tracer, in the plant or animal organism. The purpose of detection of the radioactive tracer element is served by the fact that the radioactive atoms emit radiation with energies ranging from one-tenth to several million electron volts. Instruments of the type of the Geiger-Müller counter can be used to detect this radiation. The ultimate sensitivity of detection of the total amount of a radioactively tagged element is directly proportional to the ratio of radioactive to nonradioactive isotopes in a given sample. Some of the radioactive tracer elements used may represent the same elements utilized normally by the plant in nonradioactive form, such as phosphorus and potassium. Thus the reactions of these elements as they would occur normally in the plant can be traced.

Not only may the movement of an element be traced by such means, without in some experiments destroying the plant, but also it is a property of radioactive substances, through their emission of positrons, beta particles, and gamma rays to activate suitable photographic plates. In this way, intact leaves or sections of plant tissues containing radioactive substances can make their own pictures, called radiographs, which help to explain the way in which the element moves in the plant or is accumulated by it. This method has not yet been used for precise quantitative work, but it is most enlightening for certain types of experiments. P. R. Stout prepared what were apparently some of the earliest radiographs of plants in this manner.

As illustrations of the kind of information made available by the use of radioactive isotopes, there may be cited experiments to determine the rate at which inorganic nutrients accumulate in tissues of varying metabolic activity, and the tissues through which they are transported upward or downward. An application to a problem of direct agricultural interest is the study of the fixation by the soil of certain mineral nutrient elements, such, for example, as phosphorus, and the location in the soil of the parts of the root system utilized in removing from the soil a fertilizer element that has been applied to the soil. The usefulness of the technique for this purpose could not be fully apprehended without a discussion of reactions of soil colloids and fertilizers and the fixation of chemical elements by the soil outside the scope of this article, but the practical significance is obvious.

There are some elements of great importance in research in plant nutrition or in biological research as a whole which have not been prepared in the form of radioactive isotopes of sufficiently long half-life to make feasible their tracing in biochemical or physiological processes in the plant for a suitably long period of time, before the radioactivity has decayed. Nitrogen is a particularly important example. There is the possibility, however, of utilizing in some such cases, compounds tagged, not by their radioactivity, but by containing a higher-than-normal percentage of a rare, stable, heavy isotope of the element in question. To determine that a compound has been enriched with the stable isotope it is necessary to employ a mass spectrometer: work is being inaugurated on certain problems of plant biochemistry in the laboratory of the authors at the present time. This method has already had an important role in researches of various other laboratories. Notable investigations are being made in animal metabolism by the group formerly headed by the late Dr. Schoenheimer at Columbia University.

In considering these remarks, the general reader should bear in mind that the use of isotopes, radioactive or stable, in biological investigations does not mean that a sort of magic has been introduced that will automatically result in the solution of heretofore insoluble problems. The isotopes simply form a new tool of unique properties, which is most effectively employed in the light of fundamental knowledge already developed by other means and with the concurrent aid of other techniques applicable to the problem in hand. The potentialities for use of the new tool are of extreme value for certain research, sometimes enabling information to be gained that without the isotope technique would remain inaccessible. But their introduction should perhaps be appraised with a more balanced judgment than occasionally may be formed from reading popular accounts purporting to make clear the revolutionary effects on biology of the discovery of artificial radioactivity.

EXPERIMENTALLY INDUCED ABNORMAL BEHAVIOR

NORMAN R. F. MAIER

Professor Maier, of the Department of Psychology, University of Michigan, was the recipient of the Annual Thousand Dollar AAAS Prize in 1938.

MOST of what we know about abnormal behavior has been learned from the study of mental patients and the way they respond to various forms of therapy. There are, however, a number of problems concerning the nature of the abnormal that can only be solved by experimental procedures with animals. One of these is to determine whether neurosis is a disease peculiar to man. This raises the question of whether man is so subject to the disease because of his superior mentality or whether the disease is primarily the product of man's way of living. It has been said that it takes imagination and intelligence of a high order to experience conflicts and that personality disorders require a complex personality structure. Obviously, the ability to produce true neurosis in animals will make it possible to answer this question.

The second problem concerns the relation of the symptom to the cause of the symptom. Has nature supplied man with protective mechanisms—processes whereby the organism develops certain unusual responses that serve to prevent a worse condition? Thus, if a patient develops hysterical blindness, is this symptom a means for protecting the patient from seeing something in his environment that causes his anxiety and conflict? Further, does a psychosis represent a patient's escape from reality and hence serve as a solution when reality is too stressful? If one deals only with studies of case histories, one can find support for this thesis, because the many events in a case history permit one to find a logical connection between the symptom and some problem in the patient's life. Further, when a child has enuresis, should one seek in this behavior some reason, or some way in which he thereby solves a problem? Could bed-wetting give him attention he desires? Could it be a way of striking back at strict parents? Only when we control the life histories of individuals and purposely produce symptoms can the relationship between a symptom and its cause be studied.

Animal studies permit one to secure the necessary case-history data for such investigation, but in order to use lower animals one must be able to produce behavior disorders comparable to those found in man. Thus, one of the first steps in animal studies of abnormal behavior was acquiring the ability to produce abnormal symptoms under laboratory conditions.

When our work was begun, success already had been achieved in this field. By the use of the conditioning method, Pavlov produced what he called an "experimental neurosis" in the dog. Liddell's laboratory at Cornell followed this line of attack, and Liddell used sheep and pigs as well as dogs in his research. At Johns Hopkins University, Gannt established a laboratory to continue the type of research initiated by Pavlov. These studies clearly showed that stressful and conflictual situations caused basic behavior disruptions. The disturbance was apparent from the facts that training on discrimination problems was lost, the animal became emotionally unstable (struggled and bit at restraining harnesses), and, in general, the docile animal became most uncooperative. Many symptoms akin to those observed in human patients (such as disturbances in heart rate, and peculiar fears) were seen, but their appearance was difficult to separate from reactions of normal individuals. At the time that our work with rats was begun at the University of Michigan, there was some doubt as to whether the changes produced in the animal were a "true" neurosis. The symptoms were regarded as not sufficiently profound to be convincing, and some psychiatrists argued that neurosis in subhuman animals was impossible because animals below man lacked sufficient imagination, they could not have sex conflicts, or they did not undergo permanent personality changes. Even though some of these criticisms seem to depend upon specialized definitions of neurosis, which exclude the disease from lower animals by defining it as a human disease, it seemed at the time that part of the failure to accept neurosis in animals was based upon the facts that the behavior disturbances observed were not profound enough and that the behavior was produced by a specialized aspect of the conditioning method.

IT IS probable that the attention our study received in 1938 was influenced by the fact that the disturbance produced in the rat was extremely violent and left no doubt about its being abnormal.

The abnormal behavior was in the form of a seizure in which the rat ran madly in a circular pattern. This running was so violent that the nails of the feet became torn. It was not a typical fear pattern, because shelter was not sought and the animal often ran into table legs and walls. The running phase frequently was followed by a convulsion (Fig. 1), which was similar to that produced by drugs; since then we have actually found that the convulsions produced by metrazol have a good deal in common with those produced in the training situation. After the active part of the seizure had passed (one or more minutes), the rat became very passive and its righting reflexes were either absent or greatly depressed. During this period the animal could be molded into almost any position, where it would remain for several minutes—sometimes as long as twenty minutes (Fig. 2).

The situation for producing this behavior was built around our interpretation of the conditioning studies. It seemed that behavior disturbances in the earlier studies arose when an animal was trained to give a certain response to a signal and to withold the response when a different signal was given. When these signals were made more and more alike, the animal had difficulty in determining whether to express or withold the response. By the presentation of a signal that was as much like the withholding signal as the arousal signal, the animal was stimulated to express and withhold the response at the same time. This was a conflict between doing and not doing which we regarded as a basic conflict.

To incorporate this condition in our experiment, we trained rats to discriminate between the two cards in the apparatus shown in Figure 3. The rat was trained to jump to the cards, and when it chose the correct card (the one with a white circle) this card yielded to the jump and gave the rat access to food. When the rat jumped to the wrong card (the one with the black circle), this card remained in place and the animal received a bump on the nose and fell into a protective net below. Under these conditions animals soon learn always to choose the reward card and to avoid the punishment card. After the discrimination is well learned the situation is changed so that only one of the cards is presented (Fig. 4). If this card happens to be the punishment card, the animal, as may be expected, refuses to jump. In order to cause the animal to jump to the punishment card, it is necessary to drive him. This is done by using a jet of air and directing it on the rat. When released, the air makes a hissing noise and is irritating to the rat; consequently the resistance to jumping is broken. The condition of driving the rat to make a response to a card it has been trained to avoid was considered a conflict between doing and not doing, and it was the condition under which the violent seizures most frequently were produced.

Fig. 1. This rat is in the last stages of a convulsive seizure. Previous to the convulsion, it leaped from the discrimination apparatus and ran wildly about the floor in a circular pattern. It then fell on its side with all legs kicking. At the time picture was taken the rat was beating the floor with its forelegs and whipping the floor with its tail.

Fig. 2. Rat in the passive state. When placed in an awkward position, as shown in the photograph, it remains so without showing its normal righting responses. The passive state is more profound and of greater length when a convulsion follows the running stage.

These experiments raised two interesting questions: Was the seizure produced by the conflict, or by the sound of the air? Is the seizure akin to an epileptic attack and, therefore, an abnormality other than a neurosis? The implication of the second question is that the symptoms observed are too profound to be considered a neurosis.

Experimental studies concerning these and other points have been numerous. Our laboratory alone has contributed more than 25 studies, and a total of perhaps 150 studies have been published on some aspects of the rat's abnormal behavior. The studies include the effects of diet, drugs, heredity,

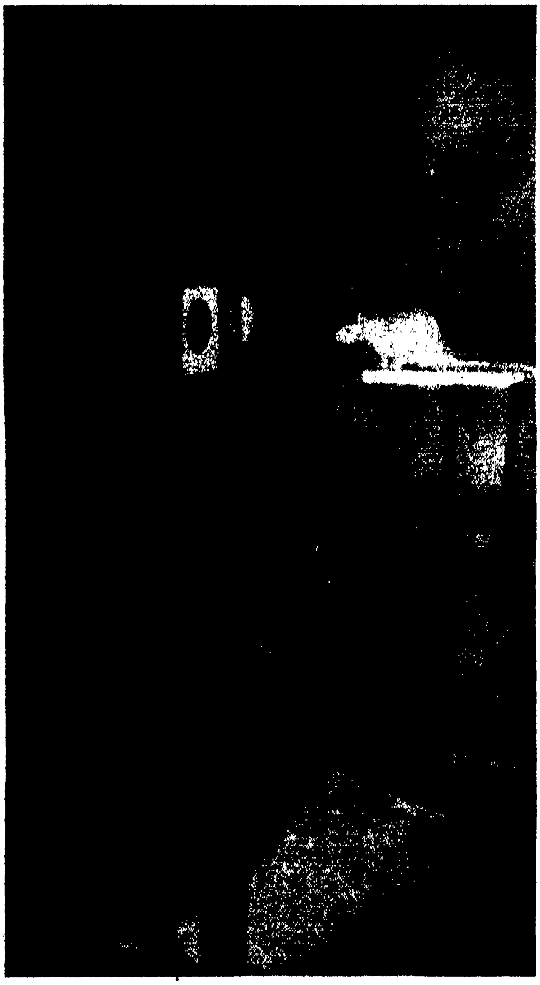


Fig. 3. Photograph of rat on the jumping platform confronted with discrimination cards. The platform behind the large screen is the feeding station which the rat reaches if it strikes the unlocked card. The net near the bottom of the picture catches the rat when it jumps against the punishment card, which is locked in place. Toward the back of the jumping platform, air is led through a hose, and a small nozzle (not visible) directs the air against the back of the animal. Close scrutiny will reveal the ruffled hair caused by the air.

emotionality, and brain injury on seizure susceptibility, as well as studies directed toward determining the nature of the abnormality.

At the present time it seems quite clear that the basic condition for producing the disturbance is conflict. The same auditory conditions, with and without the element of conflict, produce different results. The delay in settling this issue was due to the fact that auditory stimuli, such as the hiss of air, the sound of buzzers and bells, the jingling of keys, supersonic tones, and pure tones of low pitch all produce seizures in some animals. To explain these seizures it was necessary to show that these conditions also produce conflict. Certain sounds are irritants and arouse generalized escape behavior. However, when the animal is confined it is driven to escape, but at the same time its escape is blocked. Thus the animal is trapped in a situation which demands responses and yet inhibits those responses. It has been shown by Dr. Marcuse, of Cornell University, that seizures do not occur when the sound source is fastened to the animal. Under these conditions the escape behavior is permitted even though escape is not accomplished. It also has been shown that the type of confinement and the type of responses made during auditory stimulation influence the appearance of seizures. These facts indicate that auditory stimulation as mere sound stimulation is inadequate for producing seizures. Rather, other conditions must be present, and these other conditions determine whether behavior tensions are built up without permitting a release through some avenue of behavior. It is when these tensions become too great that they break forth as a seizure. The fact that smoke, water spray, and electric shock (applied during conflict) also produce seizures under proper conditions indicate that sound is not unique in its seizure-producing qualities. Since the expression of escape behavior prevents seizures, one is led to conclude that behavior must be blocked while irritants are applied.

The mere fact that conflict and unresolved tensions seem to be essential for producing seizures makes the seizure appear to be a form of neurosis rather than the epileptic attack of a defective organism. Perhaps Dr. Goldstein's classification, "catastrophic reaction," is more adequate than neurosis, since this term implies a form of disorganized behavior which occurs when the environment places demands on the organism that it is incapable of handling. Either "neurosis" or "catastrophic reaction," however, places the emphasis on the abnormality as being one that is situation-induced rather than the response of an injured or defective organism.

Because of the profound nature of the seizure, other aspects of the abnormality reported in our rats frequently have been overlooked. The conflict situation in the rat was highly frustrating and produced nervousness which extended outside the situation. Experimental rats become less likely to breed, and they develop a retiring nature. These behavior alterations are akin to personality changes, since they extend outside the test situation. Further, and more important, is the fact that the frustrating situation produced compulsive behavior in many of the rats, particularly those not showing seizures. Compulsive behavior commonly

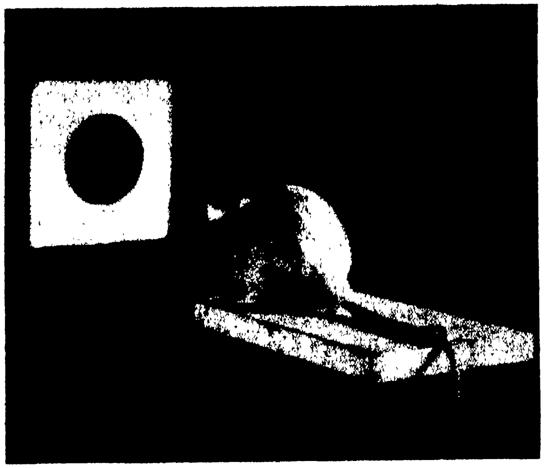


Fig. 4. Close-up of rat in the one-window test. In this test only one card is presented at any one time. The animal is about to be forced to jump to the punishment card. (Nozzle attached to the air hose is shown beside rat's tail.)

appears in neurosis and is one of the most difficult to explain. A classical illustration of compulsive behavior is Lady Macbeth's repeated hand-washing, which is regarded as an effort to cleanse herself from guilt. Because compulsive behavior is a fundamental type of abnormality, it was selected for special study. Thus, from the outset, our studies of abnormal behavior in the rat have dealt with two distinct forms of abnormality.

WHEN a rat is placed in the card-discrimination problem situation in which reward and punishment are applied in a random order, the animal is confronted with an insoluble problem. This fact is soon recognized by the animal, and it expresses its recognition of such a difficult problem by refusing to choose between cards. This refusal is so intense that hunger is not sufficient to cause the animal to take a 50–50 chance on happening to strike the reward card. In order to overcome this resistance, the animal is driven with a blast of air, as described earlier. Occasionally, seizures are produced in this situation, but more commonly the animal jumps at one of the cards, and soon its choices follow a consistent pattern. Usually the rat chooses a card on a position basis, i.e., it chooses the card on the right (or left) side, regardless of which of the cards it is. Once the rat ceases trying out various possibilities and makes its choice on a position basis, this way of choosing becomes the response to the insoluble problem, and the animal never deviates from this procedure once it is established. In practicing the position response, the animal is punished on half its choices and rewarded on the other half, since these are applied in random order. Such a condition should not establish a preferred way of responding, yet under frustration a highly specific response becomes established.

Other animals can be *trained* to show similar position responses. This is accomplished by rewarding animals for choosing the cards on a position basis. Such animals are *motivated* to express position responses, whereas the animals in the insoluble problem situation express their position responses as a consequence of *frustration.*

The question now is, Are responses established under frustration different from those established through motivational training?

It is found that animals that acquire their responses under frustration cannot substitute them for other responses. In other words, they cannot learn new responses even when the situation ceases being insoluble. Not only are they unable to adopt new responses, but they are unable to drop their inadequate position responses. This is true even if they are punished each time they express their old responses. As a matter of fact, punishing them for making their former responses makes them *more* likely to repeat them in the future. Animals with such frustration-induced position responses will choose the punishment cards whenever they are placed on the side of the animals' position preferences. They will even refuse to jump to an open window in which food is clearly displayed when its position does not correspond with their position reaction. This apparently foolish response is illustrated in Figure 5. Before jumping to the punishment card this rat sniffed longingly at the food.

This rigid behavior is in contrast to that of animals that have acquired their position responses under conditions of motivation. These animals readily learn new responses when training condi-

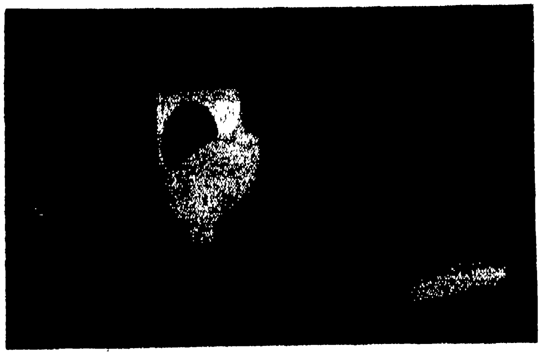


Fig. 5. Rat jumping to the punishment card, which is placed on the side of its right-position fixation. The strong tendency to jump to the right card makes it impossible for the rat to jump to the open window on the left, in which food is exposed. Before jumping to the punishment card, this rat sniffed toward the food and then quickly jumped to the right.

tions are changed, and they are constructively influenced when being punished for errors. If punished too severely, however, they, too, may become frustrated, and they then behave like the above-mentioned animals.

Because frustration makes behavior rigid and unchangeable, we have called the responses acquired under frustration "abnormal fixations" to distinguish them from normal habits. The adjective "abnormal" is used because the strength of the response does not follow the principles of learning in establishing or fixating habits.

Abnormal fixations not only are rigid but they have a compulsive character. This trait can be demonstrated in the following manner. Suppose that after a period of frustration a rat's jump to the card with the white circle is always rewarded, whereas a jump to the card with the black circle is always punished. In this situation a rat with a right-positional fixation will receive punishment whenever the card with the black circle is on the right side, and reward whenever the card with the white circle is on the right side. After a time in this situation, the rat begins to hesitate to jump whenever the punishment card is on the right, or position, side. When forced to jump it strikes the card with its rump (Fig. 5) to avoid a bump on the nose. When the reward card is on the right side, however, the animal jumps readily and hits the card with its nose and forepaws. It is evident that the animal knows which card punishes and which card rewards, and, although it expresses its knowledge of the difference in the cards by the way it jumps, it does not choose the card to jump to on this basis. The right-position fixation apparently prevents the rat from making an adaptive response to the situation, so it is forced to take punishment even though it knows better. This unadaptive behavior is in contrast to that of animals with normal position habits. As soon as they learn which card punishes and which card rewards, they abandon their position responses and, instead, follow the reward card from right to left.

Behavior similar to the abnormal fixations in rats has been demonstrated in college students by Dorothy Marquart, one of our graduate students. After mild frustration in any insoluble problem, the time required by students to learn a simple problem is greatly increased. Since all learning requires the acquisition of a new response, any resistance to change that is produced by frustration results in retarded learning. As might be expected, individual differences were apparent. Some of the students were not frustrated by the mild shock and learned at the normal rate. Those that were frustrated, however, required many more trials than the slowest of the normal learners.

The abnormal fixation is akin to rigid responses found in human beings. Accounts of compulsive behavior, such as is found in kleptomania, phobias, and alcoholism, are common in the literature of abnormality. So-called ritualistic behaviors, in which the person must repeat a senseless routine of activities, are further examples. The fact that some attitudes are rigid and not subject to modification, regardless of how senseless they are from a logical point of view, suggests that they are fixated. Of interest are the facts that rigid attitudes are emotionally loaded and are commonly associated with objects that are threats to a person's security. These observations support the suggestion that rigid attitudes have their basis in frustration. Thus, attitudes on socioeconomic topics, racial questions, and religious questions are least subject to modification, and these attitudes are most rigid during periods of frustration and stress.

THE studies of abnormal behavior in the rat lead to a new theory of frustration. They demonstrate that behavior elicited during a state of frustration has certain unique properties, and that these properties make frustration-induced behavior different in kind from that produced in a motivated state. This basic separation between motivated and frustrated behavior is in contrast to the view which postulates that all behavior has a motive. When it is assumed that all behavior is motivated, it follows that any behavior expressed is a *means* to some *end*. Thus, one is led to assume that if a child steals he is doing it to achieve some goal, or end. It is said that he is solving the problem of satisfying his wants or needs, even though he may be going contrary to some other needs, such as being accepted by society. From this point of view it follows that if we make stealing unattractive (punishing for the act), such behavior will be deterred. If, on the other hand, we recognize that there are two different kinds of behavior, then it follows that there may be two kinds of stealing, one that is motivated and solves the problem of gratifying needs, and one that is frustration-instigated and compulsive in nature. The latter type of behavior solves no problem and has no goal to direct it. It may occur in children from broken homes in which the child has adequate spending money. This type of stealing may involve the theft of objects for which the child has no need or interest. Such behavior is similar to vandalism, in which objects are destroyed rather than taken. The fact that stealing increases with frustration indicates that we must distinguish between the various forms of stealing. The separation of behavior into frustration-instigated and goal-motivated behavior permits just such a distinction.

Once we accept the belief that behavior produced under frustration follows different principles from behavior motivated by goals, we can reorganize our knowledge of the subject of frustration. For example, it is known that destructive (aggressive or hateful) behavior is associated with frustration and that a frustrated person attacks his enemy. This behavior may appear to be problem-solving in nature, but difficulty is encountered in explaining why people who are frustrated so often strike out at innocent bystanders. One can see how the destruction of one's enemies would achieve objectives, but the fact is that frustrated persons do not always express their hates in such a manner as to solve problems. Instead, they create more problems by their hateful behavior. Thus, frustrated parents abuse their children and rationalize that they are training them. The children return the hatred or direct it toward society through delinquent behavior.

It seems useless to probe for problems which hate behavior solves. Instead, our theory suggests that frustration produces hate, and the hatred is directed toward anything that is convenient or is in the individual's attention during his frustration. Some of the animal experiments show that the type of behavior expressed in frustration is determined by its availability to the individual rather than by its effectiveness.

Another form of behavior associated with frustration is that of regression, which represents a type of behavior more childish than the individual's level of development warrants. Thus, bed-wetting in an eight-year-old is a sign of regression. A child that has learned to walk may temporarily revert to creeping when frustrated. Believers in the theory that all behavior is motivated have difficulty in explaining such senseless regressive behaviors. What problem is solved by this type of behavior? Frequently, it is said that the child desires attention. The attention he receives from bed-wetting, however, may be a spanking and degradation. Is this activity solving a problem for the child, or is it aggravating a condition that is already bad? If, however, we assume that frustration produces regression and that this simplification of behavior is a direct result of frustration, then our problem is to seek the source of frustration. The child that regresses may feel rejected. Punishment makes him feel more rejected. On the other hand, love and understanding reduce the state of frustration. It then follows that a child is most likely to be cured if he is given treatment that reduces his frustration, and this is frequently what the practicing psychiatrist recommends. He suggests love and attention because they work. Nevertheless, from a motivation point of view, rewarding a bad response with love should strengthen it. Yet both aggressive and regressive behaviors are reduced when treated with understanding and love.

From our point of view it follows that the behavior expressed gives no clue as to what the frustrated individual needs. A child that is insecure may deveolp a form of ritualistic behavior and so show signs of fixation; he may whine excessively, wet the bed, and have difficulty in learning, thereby showing signs of regression; he may become destructive with toys or be a bully in school, thereby showing aggressive symptoms; or he may show behaviors that are combinations of fixation, regression, and aggression. Regardless of which behaviors are expressed, however, the underlying cause may be the same. If the insecure or rejected child is to be made to feel secure, therapy is achieved in the same way, regardless of the specific symptoms that a given child exhibits.

To show more clearly the difference between motivated and frustrated behavior, we have listed in Table 1 those characteristics of each that seem to be sufficiently common to warrant inclusion (although there may be many others).

The differences in behavior listed in the table are basic, and failure to make these distinctions seems only to lead to inconsistencies and confusion. If these differences are recognized it means that the

TABLE 1

CHARACTERISTICS OF MOTIVATED AND FRUSTRATED BEHAVIOR

Motivation-induced	Frustration-instigated
Goal-oriented	Not directed toward a goal
Tensions reduced when goal is reached	Tensions reduced when behavior is expressed, but increased if behavior leads to more frustration
Punishment deters action	Punishment aggravates state of frustration
Behavior shows variability and resourcefulness in a problem situation	Behavior is stereotyped and rigid
Behavior is constructive	Behavior is nonconstructive or destructive
Behavior reflects choices influenced by consequences	Behavior is compulsive
Learning proceeds and makes for development and maturity	Learning is blocked and behavior regresses

first step in diagnosis is to determine which condition an individual is in when one attempts to correct behavior. The nonfrustrated person is subject to training because he is responsive to training methods, and he can be attracted to substitute goals.

The frustrated individual, however, needs relief from frustration. Can the situation be corrected? If so, then such correction is a form of therapy. Another possibility is to treat the *individual* rather than the *situation*. Can the state of frustration be relieved without making it necessary to change the situation? Actually the expression of a frustrated response reduces the state of frustration. An act of aggression such as writing a hateful letter achieves relief even if the letter is not mailed. Crying (a regressive response to frustration) reduces frustration and the person need not receive the concessions that tears sometimes attain. Rats which showed tendencies to have seizures when frustrated, had fewer seizures when they developed fixations. Thus the various frustration-induced responses seem to relieve the state of frustration, but it must not be supposed that the anticipation of such relief is an essential cause of the behavior. To make this supposition would deny the basic evidence which differentiates motivated and frustration-instigated behavior.

Unfortunately, the expression of frustration-instigated responses frequently leads to further frustration. When one strikes another or verbally abuses him, the other person strikes back and so creates a further problem. Thus the value of the relief gained through expression is offset by the fact that the end the expression has served is one which leads to new frustration. It is for this reason that therapy must permit harmless forms of aggression. Such harmless forms of aggression are encouraged in play therapy and in counselling situations. These permit children and adults to express hostility without having the behavior challenged.

We thus find that the experimentation with animals leads us to a theory of frustration which reorganizes the facts of human behavior and reinterprets the meaning and importance of certain forms of therapy. It has supplied us with certain basic principles which have a firm foundation in that the principles are experimentally derived. Whether the experimentation can proceed to aid us in answering many of our perplexing problems remains to be seen.

Certainly the world situation offers many problems. To what extent are we reading motives into the frustrated behavior of nations with which we come in conflict? To what extent are our behaviors frustrated reactions rather than problem-solving reactions? Can a frustrated world solve its problems? If therapy is needed, who or where can one find a disinterested party that can make the diagnosis? The frustrated person rationalizes and justifies his feelings and actions, and for this reason the patient cannot easily treat himself. Is this also true of the behavior of nations?

* * *

MALARIAL PARASITES AND THEIR MODE OF LIFE*

QUENTIN M. GEIMAN and RALPH W. McKEE

Dr. Geiman, of the Department of Comparative Pathology and Tropical Medicine, and Dr. McKee, of the Department of Biological Chemistry, Harvard Medical School, were the recipients of half the Annual Thousand Dollar AAAS Prize in 1946.

WIDESPREAD sickness and death in warm climates, puzzling riddles, invention and discovery, insight and synthesis, have carved the milestones marking more than two thousand years in the history of malariology. Beginning with the recognizable description of malarial fevers by Hippocrates, and "The Agues and Fevers that plagued our land" in *The Wasps*, by Aristophanes, countless references have been made in popular and scientific writing to one of man's worst infections.

No effort will be made to chronicle the relation of the discovery of the microscope by Leeuwenhoek to the first view of plasmodia in 1880 by the military surgeon Laveran; to demonstrate the part played by the development of polychrome stains in the discovery of the mosquito cycle of malarial parasites by Ross; to relate the explanation of the malarial paroxysm by Golgi to advances in pathology and haematology; or to show how chance led to the discovery of quinine as an antimalarial agent which was man's sole weapon against the disease for almost three hundred years. Instead, this account will deal with the application of closely and distantly related disciplines of knowledge to the study of plasmodia. Methods and results from the fields of nutrition and metabolism of cells and microorganisms are now being applied to studies of malarial parasites. Biochemical, clinical, and cultural procedures are shedding new light on the mode of action of antimalarial drugs. The studies and results to be mentioned have been made with certain species of avian, simian, or human malarias.

MALARIAL infections in man are caused by four species of parasites, *Plasmodium falciparum* of malignant tertian malaria, *Plasmodium vivax* of benign tertian malaria, *P. malariae* of quartan malaria, and *P. ovale* of *ovale* tertian malaria.[1] These human infections have certain features in common, such as mode of transmission by varying species of anopheline mosquitoes, parasitization of red cells, anemia, and the production of chills and fever characteristic of clinical malaria. However, each type of infection has distinctive biological characteristics and response to chemotherapeutic agents.

Malignant tertian malaria, a truly tropical infection of man, is responsible for the greater number of deaths. This parasite, in the primary infection, multiplies rapidly within the host red blood cells, with a periodicity of 48 hours, and produces an overwhelming parasitemia which can infect up to 50 percent of the circulating erythrocytes. During the growth of the parasites in the red cells, the membranes of the host cells are altered physically and chemically so that they clog the small capillaries and rob the surrounding tissues of nutrients and oxygen, resulting in cessation of the function of the involved organ. If the parasites localize in the brain, coma and death can occur within 3–5 days after the onset of the disease.

Benign tertian malaria is the type of infection with 48-hour periodicity which seldom kills. This type of disease is the cause of extensive morbidity because of clinical relapses and of the great difficulty in obtaining radical cures with available antimalarial drugs. The tissue stages apparently are suppressed but not killed by the majority of antimalarial drugs, emerging to produce relapsing malaria when the concentration of the drugs in the blood falls below effective levels.

Quartan malaria is produced by parasites with a cycle in the blood stream which requires 72 hours for completion. This infection can become latent and persist as long as forty years without causing clinical malaria in the interim. Evidence for this latency has been obtained from numerous infections of quartan malaria developing in recipients who had had direct transfusions from a pooled blood bank provided by donors with a previous history of malaria, usually in childhood.

Although experimental human infections in volunteers or in paretics receiving malaria for therapeutic purposes have provided extensive information about the pathogenesis, immunology, and chemotherapy of the disease, studies with avian and simian infections have advanced our knowledge also. Avian species, *P. gallinaceum, P. lophurae,* and *P. cathemerium,* have been used experimentally, and studies with the simian species *P. knowlesi, P. cynomolgi,* and *P. inui* have furnished basic fundamental knowledge of the latter group. Since *P. knowlesi* has a 24-hour asexual cycle, is highly pathogenic for monkeys, and will produce clinical malaria in man, this species has been used for experiments by numerous investigators.

---

*This review was compiled in connection with a malaria research project supported by a grant-in-aid to the Harvard Medical School from the Division of Research Grants and Fellowships, National Institute of Health, U. S. Public Health Service.

Superscript figures refer to sources omitted here owing to lack of space. References will appear in the reprints.

Prior to the widespread use of malaria for the therapeutic treatment of paresis, the discovery of the simian parasite *P. knowlesi,* and extensive studies of avian plasmodia, our knowledge of factors underlying fundamental host-parasite phenomena was very limited. The advent of experimental infections that could be studied and controlled under laboratory conditions provided the tools to fill the many gaps in our knowledge about the life cycle of the parasites and their biochemistry, immunity, physiological pathology, and chemotherapy.

Life cycle of parasites. Careful and thorough work by Huff and Coulston[2] led to the discovery of tissue stages of avian malarial parasites which develop in the cells of the host between the bite of the mosquito and their appearance in the circulating blood. This discovery provided new facts and evidence for the discovery of similar stages in human malarias. The work of Fairley *et al.*[3] showed that tissue stages existed in human malarias (*P. vivax* and *P. falciparum*). These stages have now been found in parenchymatous cells of the liver for two mammalian plasmodia, *P. cynomolgi* of monkeys and *P. vivax* of man.[4] Since the relapsing nature of human malarias, particularly *vivax* malaria, is related to the existence and persistence of these tissue stages, methods and techniques will be sought to study the effects of drugs. This information will also be helpful in finding more effective and less toxic drugs to produce radical cures.

Immunity. The newer knowledge of immune phenomena in malaria and of the relation of immunity to treatment has provided explanations for the nature of acquired immunity, and the concepts affecting the use of clinical or radical cures in achieving malaria control in endemic and hyperendemic malarious areas.[5, 6, 7, 8] The reasons for the low-grade antibody production to antigens derived from asexual stages of plasmodia and the precise metabolic effects of antibodies on growth and multiplication of the parasites have not been studied.

Physiological pathology. Malaria became an acute problem in World War II. Prior to the war, clinical and experimental evidence partially explained the pathogenesis of malarial parasites. Experiences with large numbers of infections contracted in malarious areas during operations in the war emphasized the extent of unsolved problems. The acute and often fatal termination of *falciparum* malaria and the serious relapsing nature of *vivax* malaria stimulated more thorough investigations into the cause of death and the extent of disability caused by attacks of the disease.

In spite of the reinvestigation into the mechanism of death in acute malarial infections, controversy still exists. The sequence of events leading up to death in acute monkey infections with *P. knowlesi* has been studied in vivo by Knisely *et al.*[9] Explanation by these authors of "sludged blood" leading to stasis of the circulation, anoxia, and death in acute malaria is interpreted somewhat differently by Rigdon.[10] The rapid destruction of red cells by the parasites and by phagocytes is considered as the primary cause of anoxia and hence of the lesions found in cerebral and visceral involvement.

The investigations conducted by the Army and Navy [11, 12] to discover the organic and psychological causes for morbidity produced by *P. vivax* have added needed information about this infection and its sequelae. The chronic symptomatology which was found was attributed primarily to the malaria. On the other hand, many of the so-called effects ascribed to malaria arose from the personality of the individual and not from the disease.

These studies are of prime importance to the clinician and the pathologist. An accumulation and elaboration of biochemical and nutritional data about plasmodia should eventually lead to an understanding of the underlying ability of malarial parasites to produce disease and death.

Biochemistry. A substantial series of studies on the biochemistry and metabolism of malarial parasites has been made during the past decade. Rapid strides in biochemistry have provided essential methods and techniques applicable to studies of bacterial and parasitic metabolism and nutrition. Microorganisms are not simple in their structure and metabolism but highly complex in their cellular make-up and life processes.[13] The protozoa which cause malaria require diverse physical and chemical conditions for their existence and transmission from mosquito to man. Little is known about the requirements for development of plasmodia in anopheline mosquitoes. Furthermore, the invasion and growth of the asexual stages in erythrocytes further complicates studies of the requirements for growth, multiplication, and mechanism of disease production.

The first basic metabolic studies on plasmodia were made by Christophers and Fulton in 1938.[14] Prior to that time it was known that parasitized blood required glucose and that such blood became "blue" after standing in a test tube, owing to the removal of oxygen by the parasites. These British workers studied a number of carbohydrate nutrients and found that glucose was oxidized by the parasite *P. knowlesi*. In addition, they found that blood from infected animals consumed oxygen to produce carbon dioxide much more rapidly than normal blood.

In 1941, Maier and Coggershall[15] extended the experiments to demonstrate that various carbohydrates were capable of maintaining the oxygen uptake of *P. knowlesi, P. inui*, and *P. cynomolgi* of monkeys, and *P. cathemerium* and *P. lophurae* of birds. An attempt was made to correlate the inhibitory action of drugs on oxygen uptake to antimalarial activity, but the correlation was not clear-cut.[16] In the canary infection, *P. cathemerium,* Velick[17] correlated growth and nuclear division of the parasites with increased respiration, respiratory quotient, and cytochrome oxidase activity. Wendel[18] greatly extended our knowledge of the metabolism of the malarial parasite *P. knowlesi*. Respiratory activity with glucose and lactate substrates was equally good. Wendel confirmed the earlier work showing that glucose and oxygen are utilized with great rapidity by parasitized cells and that lactic acid accumulates because only about half the formed lactate is oxidized. Additional glucose is rapidly converted by the parasites to lactic acid, producing a rapid fall in pH and a subsequent drop in respiration.

In early morphological studies of plasmodia in red cells, it was observed that the host cell became increasingly pale as the parasite grew within. This observation suggested the utilization of hemoglobin by the parasites to form the pigment laid down in the cytoplasm. Brown[19] showed that hemoglobin of the host erythrocyte was split to form hematin, but the fate of the globin portion of the molecule was not known. More recently, the work of Moulder and Evans[20] and of Morrison and Jeskey[21] has shown that the protein of the red cell is hydrolyzed and about half the amino acids are utilized by the parasite for the synthesis of protein, the remainder of the amino acids dialyzing out of the cell.

This brief review indicates the basis for the biochemical and cultural studies begun by our group at the Harvard Medical School in 1943. The basic nature of the early work and the method of approach to unsolved problems were aptly stated by Wendel:

> . . . knowledge of the metabolic characteristics of malarial parasites may lead to an understanding of the lethal action of quinine, atebrin and plasmochin on *Plasmodia.* Such knowledge may, perhaps, simplify search for other antimalarial agents. Short of this, chemical studies should reveal the nutritional requirements and end-products of the metabolism of malarial parasites, in consequence of which *in vitro* cultivation may be feasible.

Extensive information had accumulated between the discovery of plasmodia by Laveran in 1880 and 1943, but fundamental biochemical data was meager and the requirements for in vitro cultivation were only partially defined.[22, 23]

Since the production of clinical malaria in man involved the asexual cycle of the parasite in the erythrocytes floating in the plasma, more chemical knowledge was needed about the erythrocyte and the plasma. *P. knowlesi* of monkeys was chosen for the basic studies, [24, 25, 26] and analysis of the major inorganic components of the blood (*Macaca mulatta*) was undertaken. A comparison of normal and parasitized blood gave similar results with the exception of an elevation of plasma potassium for a short time following rupture of parasitized red cells and a slightly lowered blood inorganic phosphorus, which is due to the conversion to organic phosphorus by the parasites.

Simultaneously with this attack, extended metabolic and chemical studies were made both on normal and infected erythrocytes. Many of the results of earlier workers with *P. knowlesi* were confirmed, namely, the maintenance of respiration with glucose or lactate, and the increased respiration with the increase in size of the parasite. In addition, glycerol was shown to be even more effective than glucose or lactate in the respiration of the parasites. Amino acids as a substrate produced a small uptake of oxygen, whereas succinate and acetate appeared to have no effect. Respiration was shown to be inhibited by cyanide, carbon monoxide, and high oxygen tensions. It was found that the parasitized erythrocyte converts glucose to lactate at a rate 25–75 times that of the normal monkey erythrocyte—increasing as the parasite grows. However, the lactate that is formed is oxidized at only one sixth to one third the rate at which it is produced. Chemical analyses of normal and parasitized cells for fatty acids and total and partitioned phosphorus showed that each parasitized cell contains 4 times the fatty acids, 3 times the total phosphorus, 2.4 times the 15-

minute hydrolyzable phosphorus (approximately two thirds of the adenosine triphosphate phosphorus), 3.8 times the phospholipid phosphorus, and 13.5 times the nucleic acid phosphorus found in a normal red cell.

This ability of parasitized erythrocytes to convert glucose to lactate and to oxidize lactate and pyruvate indicates that the malarial parasite synthesizes both glycolytic and oxidative enzymes. In view of this a study was made of the lactic dehydrogenase enzyme and of the flavine adenine dinucelotide content of both normal and parasitized red cells. Two to 2.5 times more of the dehydrogenase and 9 times more flavine adenine dinucleotide are present in parasitized than in normal cells.[27]

Speck, Moulder, and Evans,[28] studying *P. gallinaceum,* determined both the glycolytic cycle and the oxidative systems (tricarboxylic acid cycle) involved in the parasite. They also obtained evidence linking protein metabolism to the oxidative processes.

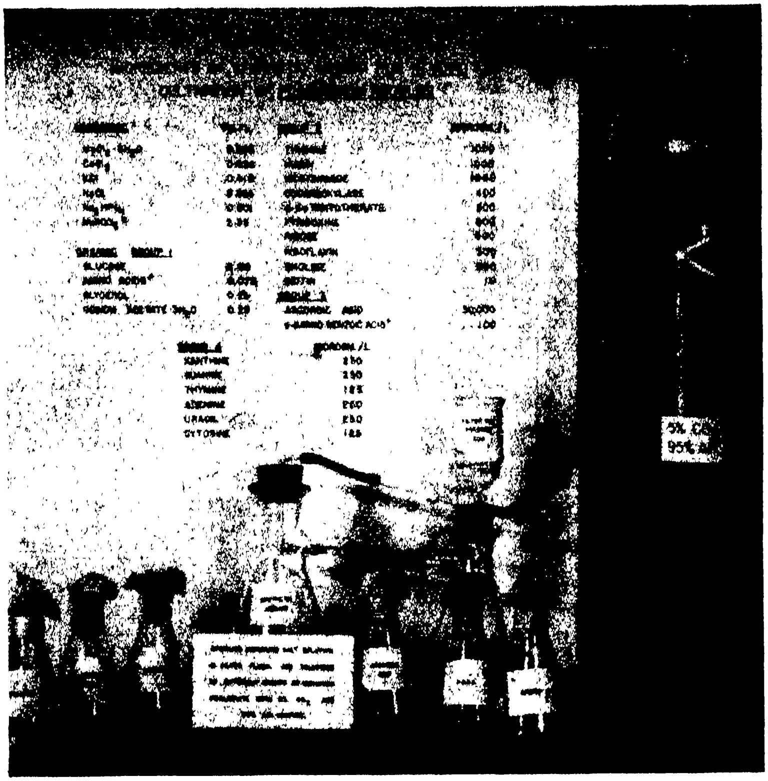


Fig. 1. To make the synthetic medium for the cultivation of mammalian malarial parasites, stock solutions of the different "blocks" of nutrients are combined.[26]

Bovarnick, Lindsay, and Hellerman,[29] studied the glycolytic and oxidative processes involved in *P. lophurae.* They have shown that succinate and fumarate, components of the tricarboxylic acid cycle, produce oxygen uptake which is inhibited by cyanide and by atabrine and quinine. These antimalarials also inhibit the phosphorylation of glucose.

As stated previously, glycerol is oxidized by the erythrocyte parasitized with *P. knowlesi* even more avidly than is either glucose or lactate. This is true also for *P. cynomolgi* and *P. vivax.*[30] This rapid oxidation of glycerol is of considerable interest because, first, it undoubtedly indicates the presence of an additional oxidative enzyme system; and, second, although glycerol is effective as a respiratory substrate, it will not act as the chief caloric source in maintaining the growth of parasites. The presence of glucose is absolutely essential for normal in vitro growth of parasites in all the systems tested thus far.

The great rapidity with which glucose is converted to lactic acid, with its consequent accumulation owing to the oxidation of only a fraction of the total lactate formed, causes a tremendous accumulation of acid. It is for this reason that either dilution or perfusion methods had to be utilized for the in vitro cultivation of the malarial parasites.[25] The dilution procedure involves both limiting the numbers of parasites and adding to the parasitized blood three volumes of nutrient fluid (Fig. 1) so that the accumulating acid is adequately diluted. The perfusion method allows for the outward diffusion of lactate, other waste products, and cell and plasma components not present in the nutrient medium, with the inward diffusion of substrates. Both these procedures (Fig. 2) have been utilized successfully for the nutritional and cultural studies—each having certain advantages and certain limitations. In vitro growth and multiplication have been obtained with five species of malarial parasites, *P. knowlesi, P. cynomolgi, P. lophurae, P. vivax* (Fig. 3), and *P. falciparum.*[30] To obtain quantitative data about the parasites before and after culture, films are made on cover glasses for staining, counting, and evaluation, and erythrocyte counts are obtained.[25] The amounts of glucose and lactate utilized are an index of the in vitro growth of the parasites.[24]

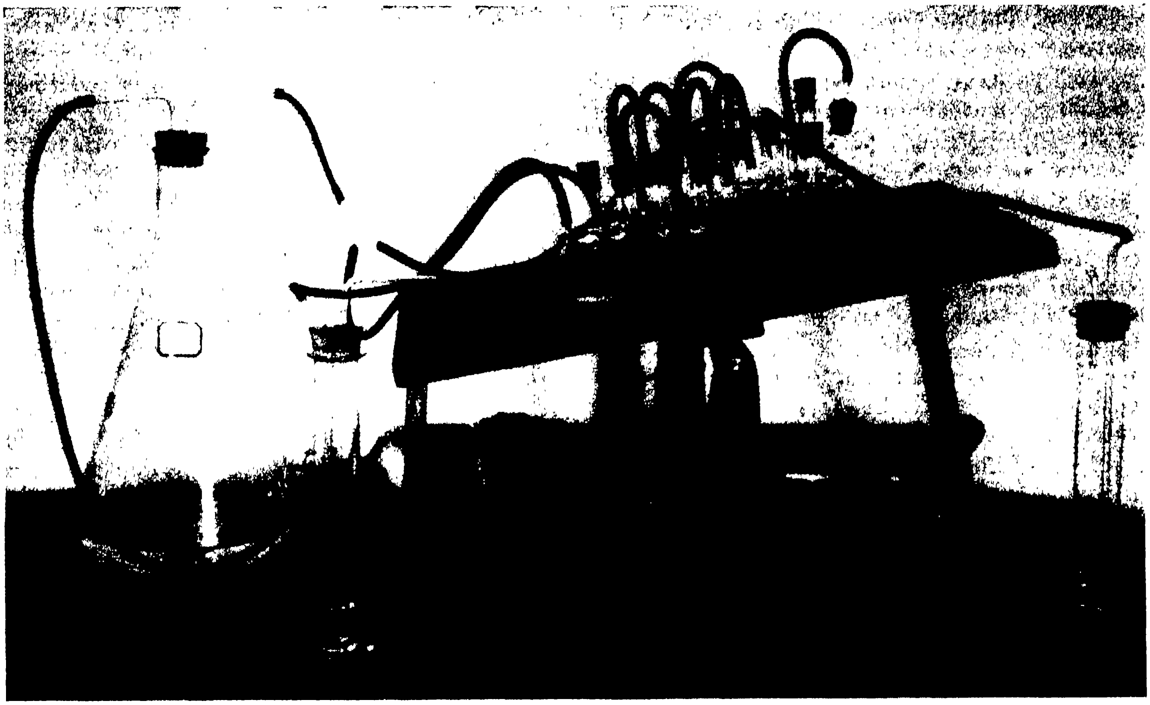


Fig. 2. Type of rocker used to agitate parasitized blood being cultivated in a warm room. The pH of the cultures is maintained by passage of a mixture of 5 percent carbon dioxide and 95 percent air through the vessels.[34]

Parasitological and biochemical data are combined to interpret the results of experiments.

The in vitro studies have shown that an isotonic inorganic medium, similar in composition to the host's plasma, is essential for the maintenance of red cells and for growth and multiplication of malarial parasites.[24] In addition, glucose is essential as a source of energy, *p*-aminobenzoic acid (PABA) as a growth accessory, and amino acids for protein synthesis. Vitamins, and purines and pyrimidines, both added as "blocks," are important in the nutrition of *P. knowlesi*.[26]

In attempting to evaluate further the essential or accessory nature of other nutrients in the medium for growth and multiplication with the dilution method, masking effects due to the presence of plasma prevented interpretation of results.[26] A dilution technique was then devised utilizing normal parasitized erythrocytes freed of plasma by washing and subsequently suspended in the isotonic culture medium.[30] With such a system, little if any growth and multiplication of parasites could be obtained unless a protein was added to the nutrient medium. Purified albumins and globulins from both human and bovine plasmas have been used with success. A system containing crystalline bovine albumin as a source of protein has been utilized in the re-evalution of the inorganic com-

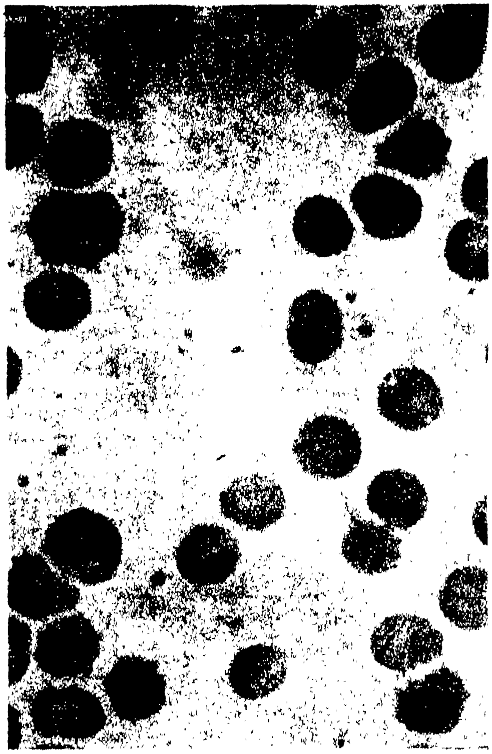


Fig. 3. Photomicrograph of a third in vitro generation of *P. vivax*. This blood film was made 68 hours after the start of the culture. Mag. × 1,100.

ponents of the medium, pH and tonicity, the individual amino acids, glycerol and sodium acetate, ascorbic acid, certain vitamins, purines and pyrimidines, and known plasma substances not present in the nutrient medium.[31]

The most striking effect observed in these recent in vitro studies was the essential need for methionine.[32, 33] If methionine is absent from the environment of the red cell, very poor growth and multiplication occur. The addition of albumin to the plasma-free medium had no influence on the amino acid requirement—methionine was still essential. This apparently means that proteolytic enzymes are not present outside the erythrocyte; otherwise, adequate methionine would be obtained from the hydrolysis of the albumin. The need for added methionine is not too surprising in view of the fact that hemoglobin contains only about 1 percent methionine, whereas most proteins—and possibly parasite protein—contain 3 or 4 times that quantity.

In spite of the dramatic effects produced by the additions of protein and methionine to the nutrient medium used in this washed-cell culture system, there still remain one or more factors needed for optimal growth and multiplication of the parasites, both simian and human. These unknown factors are present in plasma and are diffusible through cellophane, as shown by experiments using the perfusion culture technique. In an attempt to determine these unknown factors, a number of materials have been tested, none of which show beneficial results. In this regard, Trager[22] showed that a red cell extract was beneficial to the in vitro survival of *P. lophurae*.

The ultimate goal in nutritional and metabolic studies of malarial parasites is to obtain growth and multiplication of the organisms in a completely synthetic medium and devoid of the host red cell.

In Figure 4 an effort has been made to outline diagrammatically the growth and metabolism of the asexual stages of plasmodia. The chart was compiled from the results of metabolic and nutritional studies carried out in various laboratories

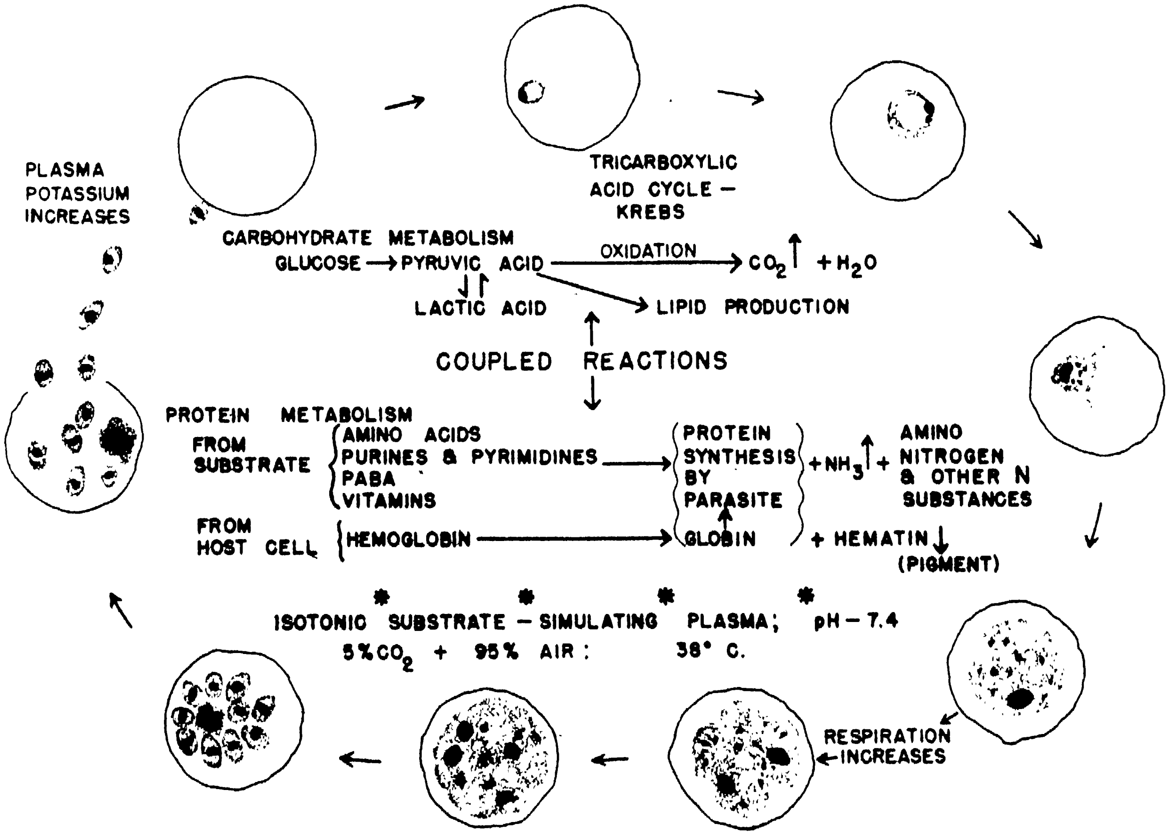


Fig. 4. Schematic diagram showing growth and metabolism of malarial parasites.[34] The sequence of events in the growth, segmentation, and reinvasion of normal red cells by the parasites is cyclic. The biochemical and metabolic processes are coupled, and they require body temperature and a physiological environment to reach maximum activity.

and discussed elsewhere.[34] It is sufficient to add, and to emphasize, that our knowledge is far from complete, and that more studies are needed to define further the nutrition and metabolism of the parasites and the properties of the red cell so necessary for the life of asexual stages of plasmodia.

Considering briefly the in vitro requirements of malarial parasites, we have found recently that the nutrition of the monkey (*Macaca mulatta*) is of great importance to the course of infection with *P. knowlesi.* Monkeys that were deficient in ascorbic acid, either naturally occurring or artificially produced, grew only a limited number of parasites.[35] The usual highly pathogenic course of infection was restored by the administration of pure ascorbic acid. A more recent finding, and one which we believe to be of even greater interest and importance, is the control of parasitemia by withholding food from infected animals (Fig. 5). This control of parasitemia can be reversed if a full diet or if only methionine or *p*-aminobenzoic acid is administered. However, the inhibition of parasite growth is not released if food is given about 8 or 9 days after inoculation with parasites. Immune mechanisms aid in controlling parasitemia at that stage of the infection even though the restored diet is adequate. In other words, adequate nutrients must be present in the animal to grow parasites. If the rate of parasite multiplication is retarded below a certain level, immune mechanisms are mobilized to control the further growth and multiplication of parasites. These observations have many applications to practical malarial problems and need to be explored further.

NUMEROUS free-living protozoa[36] and bacteria[37] have comparatively simple nutritional and cultural requirements. Tissue cells,[38] parasitic protozoa, and numerous species of pathogenic bacteria are more fastidious in their requirements. In most cases, media for cultivation were devised empirically and, when successful, answered laboratory requirements for study and propagation of the organisms. Similar attempts to devise methods and cultural procedures for asexual stages of malarial parasites were impeded by the complexity of conditions which had to be met, and the complicated problem of maintaining blood in vitro in a physiological state. The accumulation of biochemical and nutritional knowledge about plasmodia is necessarily dependent on, and limited by, the extent and advance of our knowledge about blood.

The primary environment of malaria parasites is blood. Blood plasma and red cells contain complex mixtures of inorganic and organic chemicals, with specific physical and chemical properties delicately balanced by enzyme- and hormone-regulating mechanisms according to the specific host. The blood components vary with the nutrition of the host and the function and secretion of the organs in the body. The specific biochemical differences between the blood and tissues of different hosts are providing new concepts in the study of host-parasite relationships.

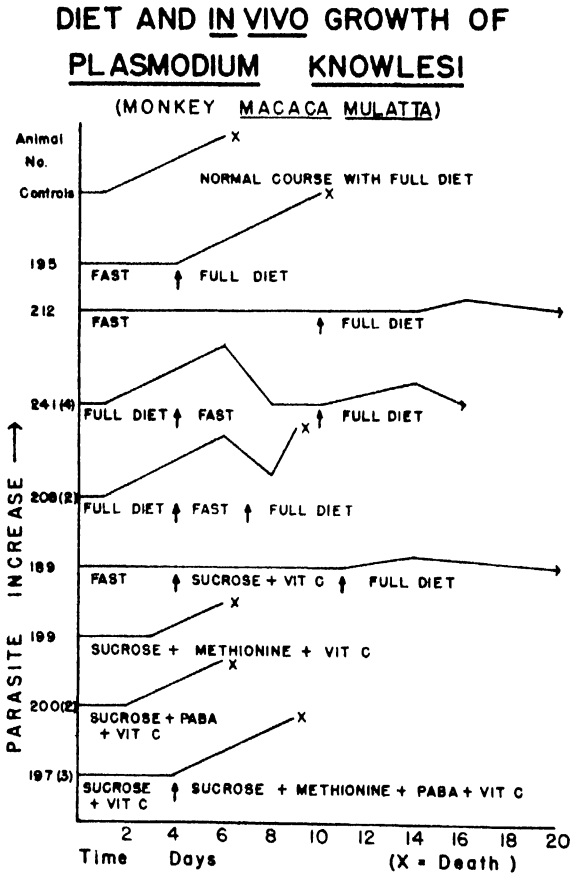


Fig. 5. Schematic representation of the control of parasitemia in monkeys as a result of the withholding of food, of the release of this control upon giving a full diet, or upon the administration of methionine and PABA. It also indicates the counteraction of antibody proteins in the plasma on parasite growth about 9 days after parasite inoculation.[33]

Since previous empirical efforts failed to produce satisfactory in vitro methods for the propagation of plasmodia, it became necessary to approach the problems systematically by analyzing host plasma and the properties of plasmodia in order to provide a foundation on which to build. Reference to previous papers and to the partial discussion here indicates the elaborate methods and techniques that have been found necessary to achieve growth and multiplication of malarial parasites. It is hoped, however, that the procedures can be simplified later.

Whether simplification of methods and the synthetic medium now in use will be possible is not known at this time. The requisite components and properties of the synthetic medium for cultivation are determined by the dual need of supporting the host red cell and its parasite in a physiological state for growth and multiplication. The nutrition of the parasites involves the splitting and utilization of hemoglobin, to form pigment (hematin) and to synthesize parasite protein, and the availability of diffusible substrates from the nutrient medium. Although the parasite contains enzyme systems, nothing is known about the supporting role which the host cell plays either in the nutrition of the parasite or in supplying a physical intracellular matrix needed for the existence of the parasite.

Unsuccessful attempts to cultivate the parasites free of the red cell in media simulating the intracellular components of the erythrocyte suggest the existence of unknown requirements for growth. Precise reasons for the immediate reinvasion of normal red cells by the progeny or merozoites of each asexual generation are not known. Perhaps the erythrocyte provides a more compatible inorganic and organic environment to carry on metabolic functions. When available, a comparison of the growth requirements of other obligate intracellular parasites, such as *Rickettsia* and *Toxoplasma*, with those of *Plasmodia* will be of great theoretical and practical value.

The competition of the malarial parasite with cells and tissues of the host for nutrients forms the basis for host susceptibility, virulence, intensity or levels of parasitemia, and stimuli for the initiation of immune reactions. The ability of the host to provide the specific metabolites for the parasites determines the initial rate of growth and multiplication. Natural or experimental interference with any of the key metabolites (Fig. 4) will affect one or all of the categories of host-parasite relationships. Chemotherapeutic interference to produce clinical or radial cures of the infection kills the parasites by direct action or selectively inhibits vital enzyme systems.

Thus, factors which influence host-parasite relationships and susceptibility to plasmodia can now be analyzed in vitro and in vivo. The results will aid in evaluating the mode of life of malarial parasites and when applied to other parasitic protozoa should provide needed information of value in combating important human infections.

◆

BIOLUMINESCENCE: A REACTION RATE TOOL

FRANK H. JOHNSON

Professor Johnson, Department of Biology, Princeton University, shared the Annual Thousand Dollar AAAS Prize in 1941 with Drs. D. E. S. Brown and D. A. Marsland.

The stars flame down . . . and who will say
This worm is not as great as they?
"Glowworm," by Daniel Smythe

THE problem of reaction rates in biological processes is the most complex and at the same time the most fascinating of all kinetic phenomena. How is it, for example, that among mammals a Blue whale, say, may be born with a weight of some 7,000 pounds whereas a human baby weighs only 6 or 8 pounds? Yet both begin their individual life as a single microscopic fertilized egg, and the period of gestation is approximately the same. In another three years, the whale has reached maturity, sometimes attaining a length of 100 feet and weighing more than 200,000 pounds. This is an impressive example of normal differences in the chemical reaction rates responsible for growth, at practically the same constant temperature, among related organisms. Such differences are, of course, under genetic control, but a recognition of this fact merely poses one aspect of the problem to be explained. Moreover, the influence of the genes may be modified, and to some extent overridden, by influences of the environment. Thus, an excess of a single protein, the growth hormone of the anterior pituitary, leads to abnormally large but symmetrical growth of a maturing individual, whereas a deficiency of the same substance results in a dwarf; a lack of vitamin A impairs vision; and insufficient oxidation of phenyl pyruvic acid in the body is always correlated with a certain type of idiocy.

Familiar examples of the significance of reaction rates in living organisms are legion. Cancer is one of the most important of the unsolved problems. In leukemia, a cancerous state of the white blood cells, the population of leucocytes becomes abnormally large. Normally, a subtle control of relative reaction rates maintains a delicate balance between the rate of formation and rate of destruction of these cells. The balance may be upset not only by cancer, but also by a bacterial invasion, whereupon the leucocytes increase to several times their normal population. Perhaps in both cases an inhibitory factor is removed. In any case, whether we deal with growth, reproduction, longevity, respiration, or other complex phenomena, including the thought process itself, inhibition as well as acceleration are fundamental to the normal rate control of the numerous reactions, simultaneous as well as consecutive, which lead to the observed result.

Any attempt to analyze the mechanism of biological reaction rate control must first take into account this fact—axiomatic among biologists—that the reactions are enormously complex. A second fact, paradoxical though it seems, is that, under given conditions, in a given genetic environment, a whole complex process may behave much in the manner of a single chemical reaction. The scientific literature is replete with instances where the rate increases over a considerable range of rise in temperature, in accordance with the Arrhenius equation, a mathematical relation that was originally discovered through studies on the rate of hydrolysis of sucrose by various acids. The usual interpretation is that in such cases the total rate is limited by the velocity of the slowest reaction involved, e.g., the activity of a single protein catalyst, or enzyme. Unlike most ordinary chemical reactions, however, the biological rate does not continue to increase more or less indefinitely as the temperature is raised. It always reaches a maximum, the so-called optimum, then decreases. Thus, at the higher temperatures, a different reaction becomes important in limiting the rate of the over-all process, and there are substantial grounds for believing that this reaction is the thermal inactivation of one or more of the enzymes concerned.

The activity of a critical enzyme with its substrate may determine the rate of a complex biological process. As an approach to the analysis of factors which influence the total rate, many processes, including growth, are too complicated, or the experiments too time-consuming, to provide favorable material for study. Under different conditions, different systems might assume control of the rate. Ideally, one would like to have some process whose rate is limited largely by the same enzyme system under a wide variety of conditions. Furthermore, one would like to have a means of measuring readily and accurately the activity of that system. Finally, if it is not asking too much, one would like to have some visible, instantaneous index to its activity, such as a little glow of light whose brightness would be proportional to the reaction velocity of the system. A system with these characteristics would be an ideal tool.

Nature has been kind to the investigator of biological reactions in providing just such a system: bioluminescence. Nature has been kind, again, in bestowing this fascinating property upon bacteria, which are widely distributed in the sea, easily isolated, and conveniently cultivated in the laboratory. Moreover, in bacteria the luminescent reaction, under favorable conditions, is a steady-state process, the emitted light glowing at constant intensity over considerable periods of time. Yet it responds at once to alterations in temperature and hydrostatic pressure, as well as to the addition of many chemical agents of physiological and pharmacological interest which react directly with the light-emitting system. Luminescence responds immediately by changing to a faster or slower steady state, indicated by a different, constant brightness. Or, if the environmental conditions are so drastic as to destroy the system, the rate of destruction may be accurately followed by the progressive decrease in brightness, even when this rate is so fast that it could scarcely be measured by any other means.

With the unique advantages found in luminescence, it has been possible to analyze to an unusual extent the mechanism of factors which influence the rate of a biological process within a living cell, and the results have been of some general significance. Since the quantitative aspects have been set forth at length in papers by Johnson and collaborators, Eyring, Brown, and others, the discussion in this article deals primarily with the basic ideas in a largely qualitative manner.

The luminescent reaction. First, with regard to bioluminescence itself, the work chiefly of Harvey and his associates has shown that the brightness of this "cold light" is proportional to the reaction velocity of the oxidative enzyme luciferase with its substrate, luciferin. From certain organisms such as the ostracod *Cypridina,* these components may be extracted and partially purified. When aqueous solutions of the two are mixed together in the presence of molecular oxygen, an emission of light takes place (Figs. 1, 2), but it rapidly diminishes as the substrate is destroyed through nonluminescent reactions. In luminous bacteria a similar system is evidently concerned in light production, but is more intimately bound to the living cell (no attempts to extract it have been successful so far) and, as already mentioned, may remain in a steady state for some time. Both materials are useful: evidence as to the site of action of various factors, such as drugs, can be obtained with purified extracts, and other kinetic aspects can be more easily studied with bacteria. Moreover, the latter are easier to obtain and prepare in abundance.

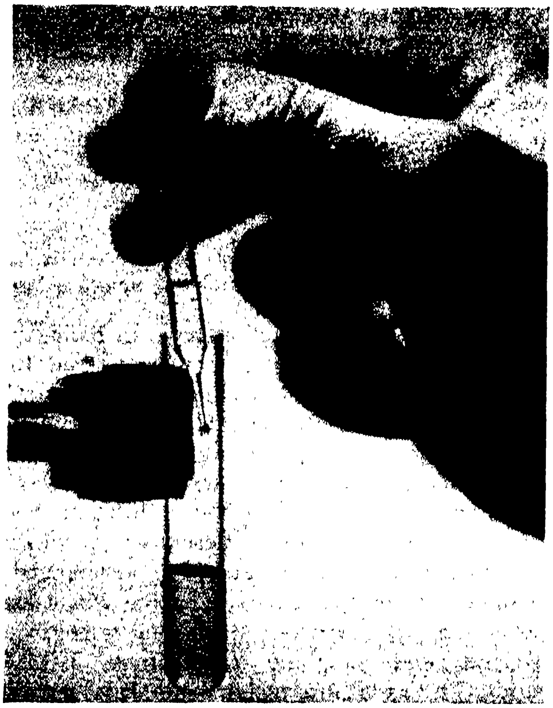


Fig. 1. Luciferin-luciferase reaction, with extracts of dried *Cypridina.* A partially purified solution of the enzyme luciferase is held in the small medicine dropper, just before adding to solution of luciferin in test tube.

Most specimens of marine fish in any market have a few cells of luminous species of bacteria on their bodies. If the fish is placed in a shallow pan containing a 3 percent salt solution and incubated at 10°–20° C, colonies of these bacteria develop in a day or two and may spread over a large part of the body surface before putrefaction sets in (Fig. 3). Pure cultures are isolated by ordinary bacteriological procedures, using a culture medium containing 3 percent salt. For experiments, the cells are usually grown on agar until maximum brightness is attained, and then emulsified in buffered salt solution containing an excess of glucose as hydrogen donator, followed by thorough aeration with air or oxygen. Under these conditions, the rate of the luminescent oxidation is limited primarily by the activity of the luciferase.

The action of temperature and pressure. With the advent of Eyring's theory of absolute reaction rates in 1935, the rational basis of chemical reaction rates became available in the expression

$$k' = \kappa \frac{kT}{h} K^{\ddagger} \qquad (1),$$

in which k' is the specific rate constant, and $K^{\ddagger}$ is the equilibrium constant between the reactants and the activated complex. Once formed, the activated complex decomposes with a universal frequency, $\frac{kT}{h}$, which is the same for all chemical reactions. In this term, k is the Boltzmann constant, T the absolute temperature, and h is Planck's constant. The transmission coefficient, κ, takes into account the probability that the activated complex, after breaking apart, will not immediately reconstitute itself. For most reactions the value of κ is very close to unity. All the methods of thermodynamics are applicable to the equilibrium constant $K^{\ddagger}$, exactly as they are to any ordinary equilibrium constant, the dagger superscript indicating "of activation" rather than "of reaction." Thus

$$-RT\ln K^{\ddagger} = \Delta F^{\ddagger} = \Delta H^{\ddagger} - T\Delta S^{\ddagger} = \Delta E^{\ddagger} + p\Delta V^{\ddagger} - T\Delta S^{\ddagger} \qquad (2),$$

in which $\Delta F^{\ddagger}$ is the free energy of activation, i.e., the difference between the free energy of the activated complex and reactants; $\Delta H^{\ddagger}$, the heat of activation (corresponding to the "μ" of the Arrhenius equation); $\Delta E^{\ddagger}$, the energy of activation; $\Delta V^{\ddagger}$, the volume change of activation; $\Delta S^{\ddagger}$, the entropy of activation; p, the pressure; R, the gas constant; and T, the absolute temperature. It follows that, other things being constant, the reaction rate will be determined by temperature and pressure, in accordance with the values of $\Delta H^{\ddagger}$ and $\Delta V^{\ddagger}$, respectively. Numerical values of these constants may be calculated from a few data relating the rate of the reaction to the variables, temperature and pressure, respectively, and the rate can then be predicted for other values of the same variables than those measured, provided the mechanism of the reaction does not change.

In biology, the same theory applies to each individual reaction among those which are involved together in bringing about a complex process. This theory for change, along with the classical thermodynamic theory for ordinary equilibria, constitutes the ultimate basis for interpreting the control of the rate. Both have been applied quantitatively to bacterial luminescence and the whole process, under the specified conditions, found to behave much in the manner of a simple reaction.

First, with respect to temperature, when a test tube containing a suspension of luminous bacteria is placed in ice water, luminescence becomes relatively dim. If the tube is gradually warmed, the brightness increases progressively with rise in temperature, finally reaches a maximum, and then decreases with further rise in temperature until it all but disappears (Figs. 4, 5). At this point, if the tube is placed at once in the ice bath again, luminescence grows brighter, reaches its former maximum (Fig. 6), and decreases as the temperature of the cells changes from warm to cool.

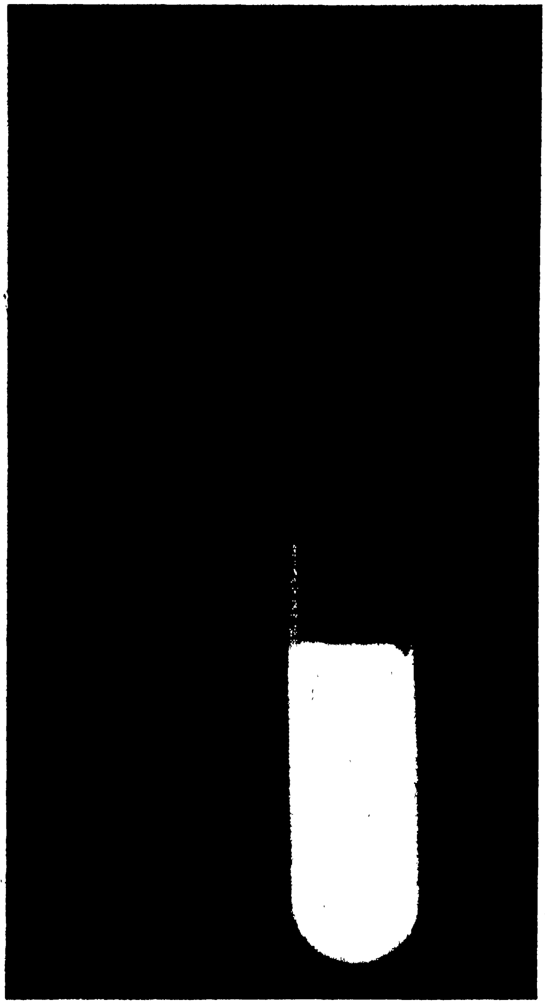


Fig. 2. Photograph of same tube taken by the cold light that was emitted as soon as the two solutions were mixed.

This is a critical experiment, revealing a fact of basic importance, namely, that the heat- as well as the cold-diminution of the observed reaction is readily reversible. Two limiting reactions are involved in the control of luminescence, one predominant in the low temperature range, the other in the high. Quantitative analyses leave little

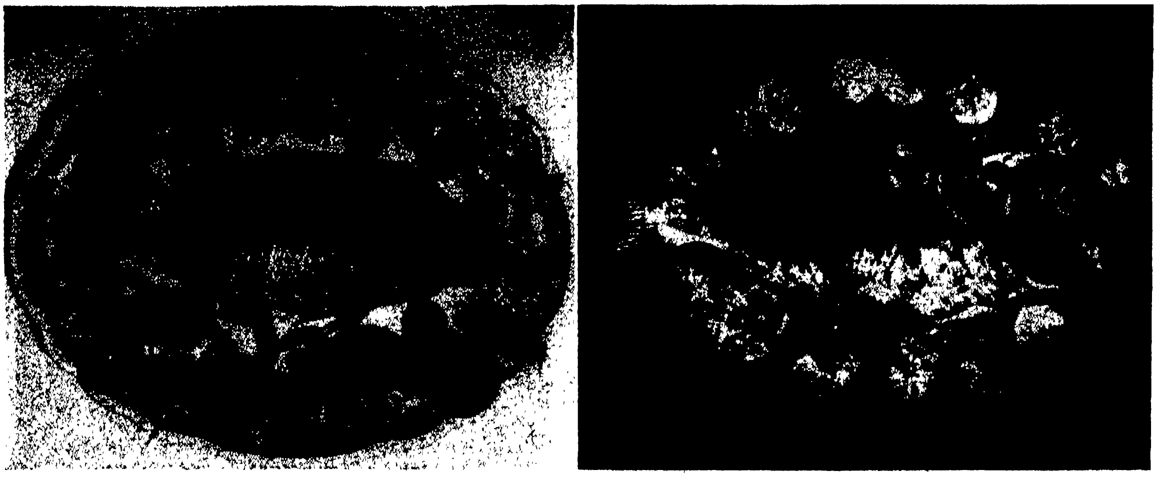


Fig. 3. *Left,* a fish dinner, with shrimp, appetizingly garnished with parsley and lemon; *right,* same specimens, as seen in the dark, both fish and shrimp glowing with the pale, blue-green light given off by nonputrefactive luminous bacteria growing on their surface. A few generations ago, before modern refrigeration, it was not unusual to find uncooked meat and sea food exhibiting such an appearance. It was well known to Robert Boyle and many others as long ago as the seventeenth century.

doubt as to the nature of these reactions: the first is the rate of the catalytic, enzyme reaction itself, i.e., the luciferin-luciferase reaction which leads to the emission of visible light; and the second is a mobile equilibrium through which the active enzyme is denatured to an inactive form. The former has an apparent activation energy ($\Delta H^{\ddagger}$) of 15,000–20,000 calories, typical of biological oxidations and many enzyme reactions as well as more complex processes, whereas the latter has a heat (ΔH) of 55,000 or more calories and entropy (ΔS) of some 200 E. U., indicative of protein denaturation.

A second observation of basic importance concerns the influence of hydrostatic pressure. At low temperatures, the application of a few hundred atmospheres of hydrostatic pressure immediately reduces the brightness of luminescence. At the temperature of maximum brightness, however, such pressures have scarcely any observable effect, and at still higher temperatures, in the range where heat diminishes the brightness, pressure will at once counteract this diminution (Fig. 7). The opposite effects of pressure show that different reactions are predominantly in control of luminescence brightness at the two extremes of the temperature range. There is evidence that these reactions, again, are the enzyme reaction itself and the reversible denaturation of the enzyme, respectively. The former proceeds with a volume increase of activation ($\Delta V^{\ddagger}$) of some 50 cc/mole, whereas the denaturation is accompanied by an even larger volume increase of reaction (ΔV), closer to 100 cc/mole. Pressure thus retards the reaction most noticeably under conditions where the enzyme is essentially all in the native state, as at low temperatures. When, however, a considerable fraction of the enzyme is in the reversibly denatured state, the application of pressure shifts the equilibrium in favor of the active form and the intensity of the light increases. This effect arises from the larger volume change in the equilibrium than in the rate process, the net result of pressure being an increase in the over-all rate. At the temperature of maximum luminescence, the so-called optimum, the influence of pressure on the two reactions balances out.

Although the theory based on these two limiting reactions is oversimplified, it is sufficient to calculate with remarkable success the brightness of luminescence, in relative units, over most of the temperature range in which it can be measured, and for pressures up to some 10,000 pounds per square inch. The theoretical equation is

$$I = \frac{k'\,(\text{luciferin})\,(\text{luciferase})}{1+K_1}\,.$$

$$= \frac{cTe^{-\frac{\Delta E^{\ddagger}}{RT}}\,e^{-\frac{p\,\Delta V^{\ddagger}}{RT}}}{1+e^{-\frac{\Delta E}{RT}}\,e^{-\frac{p\,\Delta V}{RT}}\,e^{\frac{\Delta S}{R}}} \qquad (3),$$

in which I represents brightness of luminescence in arbitrary units, and c is a constant containing some known constants as well as others, e.g., the

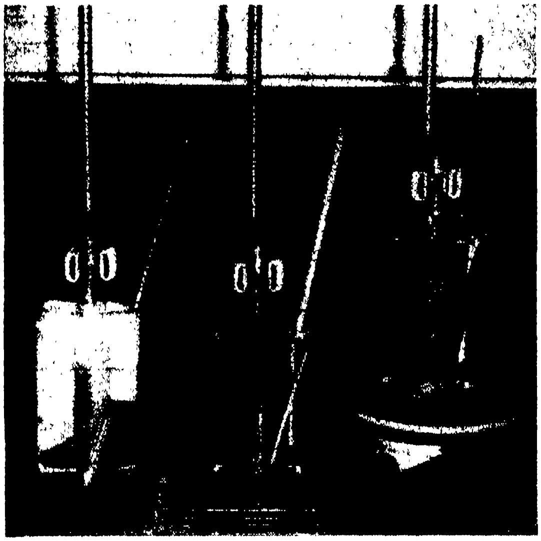


Fig. 4. Three tubes containing portions of the same suspension of luminous bacteria at (*left*) cold, (*middle*) optimum, and (*right*) unfavorably warm temperatures, respectively.

concentrations of luciferin and luciferase, which cannot be readily determined within a living cell. The other symbols have the usual meaning given above.

It is worthy of note that, first, equation (3) provides a rational basis for the interpretation of the "temperature-activity" curve; second, the reversible effects of hydrostatic pressure are accounted for on the same basis; and, third, this equation deals with the activity of a single enzyme system. Moreover, a number of chemical agents, including some of the familiar drugs, have been found to act upon the same system, introducing new reactions that limit its rate.

The action of chemical agents. From a study of the extracted and partially purified luciferin-luciferase system of *Cypridina,* it has been shown that the rate of light production may be slowed in the presence of drugs such as urethane, sulfanilamide, and others. Their inhibitory effects are removed by diluting the solution, and they evidently act by entering into an equilibrium combination with this system directly. In approximately the same respective concentrations, these drugs also inhibit the luminescence of living bacteria (Figs. 8, 9), likewise reversibly, since the inhibition disappears if the cells are centrifuged and resuspended in a drug-free medium. It appears safe to conclude, therefore, that the action in both the extracted system and in bacteria is fundamentally the same. Yet the mechanism of action of different drugs may be altogether different, as shown by the influence of temperature and hydrostatic pressure upon the amount of inhibition caused by a given concentration of the drug.

Qualitatively, the influence of temperature may be demonstrated by a simple experiment, as shown in Figure 10. A small amount of sulfanilamide solution is added to one tube (*left*), dilute alcohol to another (*right*), and pure salt solution is added to a third (*middle*) as a control for comparison. Equal portions of a bacterial suspension are added to each tube, and the luminescence of all three noted at different temperatures. In an ice bath (*A*) it is at once apparent that, although the alcohol causes no perceptible change in brightness, the sulfanilamide all but extinguishes it. As the three tubes are simultaneously warmed, all grow brighter until at the optimum temperature the sulfanilamide-containing suspension has increased in brightness manyfold, and is much more nearly equal to that of the control (*B*). The influence of alcohol, however, becomes increasingly manifest as the temperature rises, and it causes an obvious inhibition at the optimum temperature. At still higher temperatures, in a warm bath, the sulfanilamide tube remains luminous, whereas the one with alcohol is practically extinguished (*C*). All these effects are reversible on cooling; that is, the former relations obtain if the tubes are again placed in an ice bath.

The quantitative analysis of data from experiments of the type just described makes it evident that sulfanilamide combines in a manner that is independent of the denaturation equilibrium of the enzyme. It combines as if with some active group on the enzyme, involving chemical bonds that are not concerned in the thermal inactivation of the catalyst—perhaps with a prosthetic group. Conse-

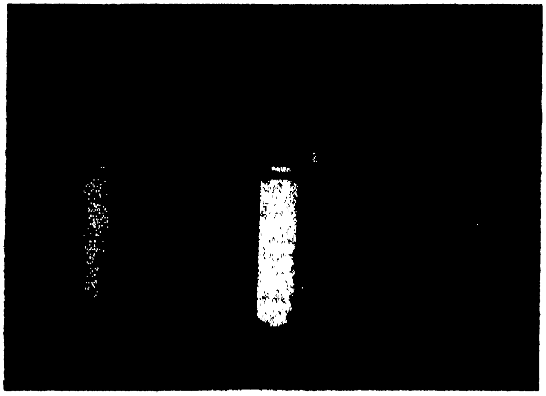


Fig. 5. Same tubes, as photographed by their own light, brightest at the optimum temperature, and dimmer when either too cold or too warm.

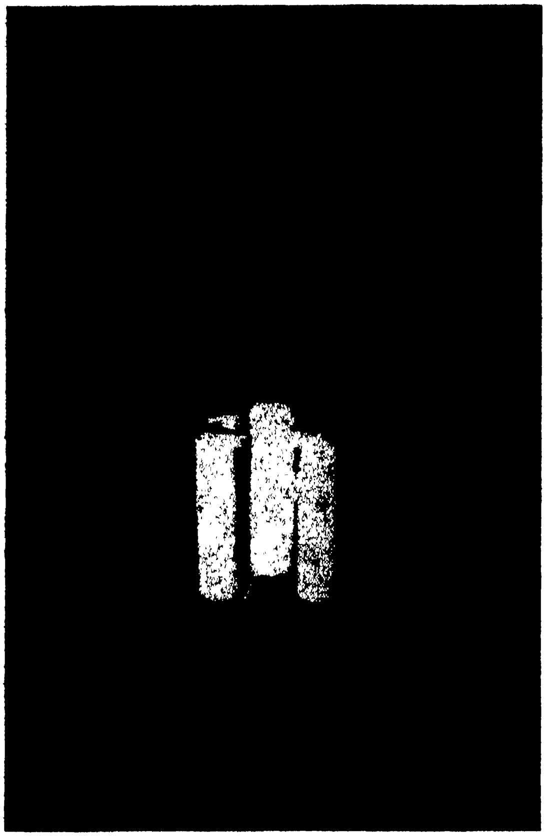


Fig. 6. Same tubes after placing all three in a water bath at optimum temperature. Both the heat- and the cold-diminution are reversible.

quently, as the temperature is raised, this combination becomes dissociated, and the percentage of inhibition is decreased. Alcohol, on the other hand, acts exactly as if it promoted the same equilibrium change from active to inactive states of the enzyme that is furthered by temperature in the absence of a drug; in other words, as if it catalyzed the denaturation of the protein. Or, viewed in another way, alcohol appears to combine with bonds on the protein that are made available through the denaturation. Thus, as the temperature is raised, the fraction of protein molecules with which alcohol may combine is increased, and the inhibition is augmented. In effect, the drug lowers the temperature of protein denaturation. A rise in temperature also tends to dissociate the protein-alcohol complex, however, and the net result depends upon the equilibrium constants for both these reactions. The heats of reaction are of opposite signs, and in the event that they are numerically the same the inhibition remains independent of temperature. Examples of this sort may be found in the literature with respect to other drugs and systems.

The influence of pressure on the action of these drugs might be anticipated from the above considerations. Combination with sulfanilamide evidently involves few bonds and leads to no pronounced change in the state of the protein catalyst itself. Consequently, pressure would be expected to have very little influence on the amount of the inhibition, since the volume change of the equilibrium would be small. On the other hand, because of its relation to the denaturation equilibrium which proceeds with quite a large volume change, the effects of alcohol might be expected to be sensitive to pressure, as, in fact, experiments have demonstrated. Thus, at the optimum temperature, where pressure ordinarily exerts little or no observable influence, the addition of alcohol causes an inhibition that, for moderate concentrations, can be completely eliminated by hydrostatic pressure.

The action of these drugs on luminescense, in relation to their concentration, to temperature and to hydrostatic pressure, is described with considerable accuracy by the following theoretical equation, in which K_1 represents, as previously, the equilibrium constant for the reversible denaturation of the enzyme in absence of drugs, K_2 the equilibrium constant for the combination of r molecules of sulfanilamide in molar concentration (X) with the enzyme, and K_3 the equilibrium constant for the combination of s molecules of alcohol in molar concentration (U) with the enzyme:

$$I = \frac{c\,T\,e^{-\frac{\Delta E^{\ddagger}}{RT}}\,c^{-\frac{p\,\Delta V^{\ddagger}}{RT}}}{1 + K_1 + K_2(X)^r + K_1K_2(X)^r + K_1K_3(U)^s} \qquad (4).$$

On dividing equation (3) by equation (4), the numerators on the right cancel out and simple formulations are arrived at for determining the equilibrium constants for the inhibitory reactions, provided that the inhibitors of the two types are not simultaneously present and entering into a possible combination with each other. The analyses indicate that one molecule of sulfanilamide combines with the enzyme molecule, whereas the ratio of alcohol molecules combining per protein molecule is from three to four. The heat of reaction in the sulfanilamide combination is around 15,000 calories, whereas that for alcohol is much higher and, unlike the former, is associated both with a large entropy and volume change.

Significance of the pressure effects. The foregoing discussion has dealt almost entirely with the process of luminescence as a type reaction, and

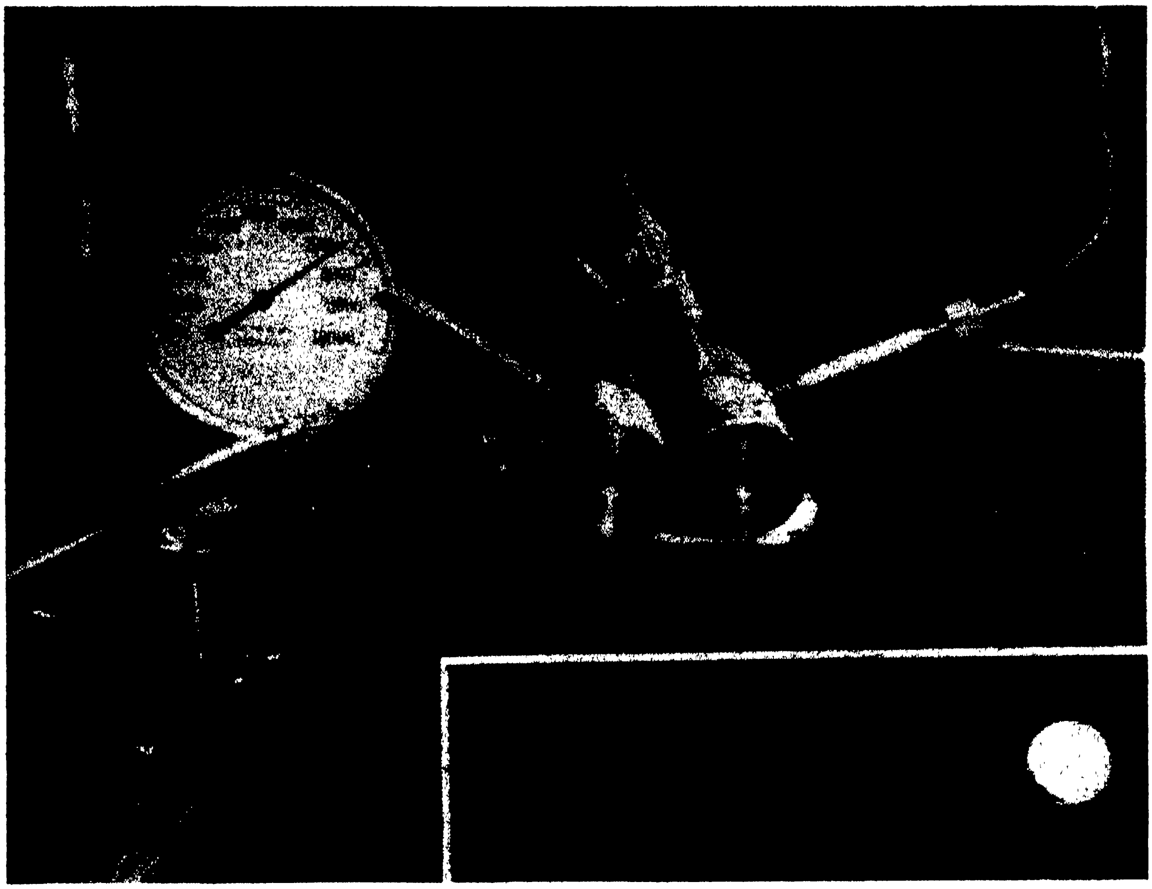


Fig. 7. Showing the influence of hydrostatic pressure on the brightness of bacterial luminescence. Two similar "bombs" are filled with equal portions of a bacterial suspension, both at a temperature high enough to cause a diminution in brightness. Bomb at right is then connected to the hydraulic press and the bacterial suspension placed under 7,400 lbs./in.^{2} pressure. *Inset:* Luminescence is visible through the plate glass window, .75 inch thick, in each bomb. Photographs show a close-up of the windows, taken by the light of the bacteria. The brightness of luminescence is much greater in bomb at right, showing that pressure counteracts unfavorable effects of the above-optimum temperature. A similar effect is noted at the optimum temperature if alcohol, urethane, or certain other drugs are first added to both suspensions. At cold temperatures, in the absence of drugs, effects are just the reverse; i. e., the window on right, showing the luminescence of the cells under pressure, would be dimmer than one on left, at normal pressure. In all these cases, the influence of pressure is in same direction as that of cooling.

the conclusions regarding certain mechanisms have been based very largely on kinetic evidence, i.e., the analysis of rate phenomena. Previous to the study of luminescence, the physiological effects of pressure, in particular, had not been interpreted on the basis of a rational, quantitative theory. There had been no evidence that hydrostatic pressure may retard the denaturation of proteins; on the contrary, a great deal of data had already accumulated showing that very high pressures, usually on the order of 10 times those which have been referred to, actually accelerate protein denaturation. Several questions, therefore, immediately present themselves. Is there, for example, any evidence more direct than purely kinetic evidence that protein denaturation can be retarded or reversed by pressure? Just what happens to a large molecule when it undergoes a volume change in a reaction? Why do moderate pressures, up to 1,000 atmospheres, retard protein denaturation, whereas very high pressures, of 10,000 atmospheres and above, accelerate it? Do the effects of the lesser pressures have any fairly general significance?

In answer to these questions, it should be pointed out first that, with regard to a rate process, kinetic evidence is not only the best but virtually the only kind that can be obtained for such volume changes of the molecules, and this is measured by the influence of pressure on the rate of reaction. This volume change takes place in the activated complex, which at room temperature

Fig. 8. The influence of drugs at the optimum temperature of luminescence in the species of bacteria concerned. Each flask contains equivalent portions of a bacterial suspension, to which has been added (*left to right*) small amounts of alcohol, aspirin, no drug (control), procaine, ether, and sulfanilamide, respectively.

has a lifetime usually of about 6 ten trillionths of a second, and only a fraction of the reactants are in the activated complex at any one time. With equilibrium reactions, on the other hand, it is possible to make direct measurements of volume changes between the initial and final state by means of a dilatometer. In this way it has been shown by Heymann that the denaturation of certain proteins—e.g., the coagulation of egg albumin by heat—is accompanied by volume increases of the same order as those revealed by the kinetic analysis of luminescence in bacteria. Furthermore, recent studies of the influence of pressure on the denaturation of highly purified human serum globulin and of tobacco mosaic virus, under the influence of heat as well as certain drugs, have shown that pressure retards the rate by a magnitude indicating about 100 cc/mole volume increase of activation. There is no doubt that in the cases investigated protein denaturation may be accompanied by a volume increase.

There is no basis in theory, however, for predicting that the volume change on denaturation will always be an increase, or that it will always be different in the final than the initial, or in the activated than in the normal, state. The volume change will depend upon the mechanism of the reaction, and, with molecules as complicated as

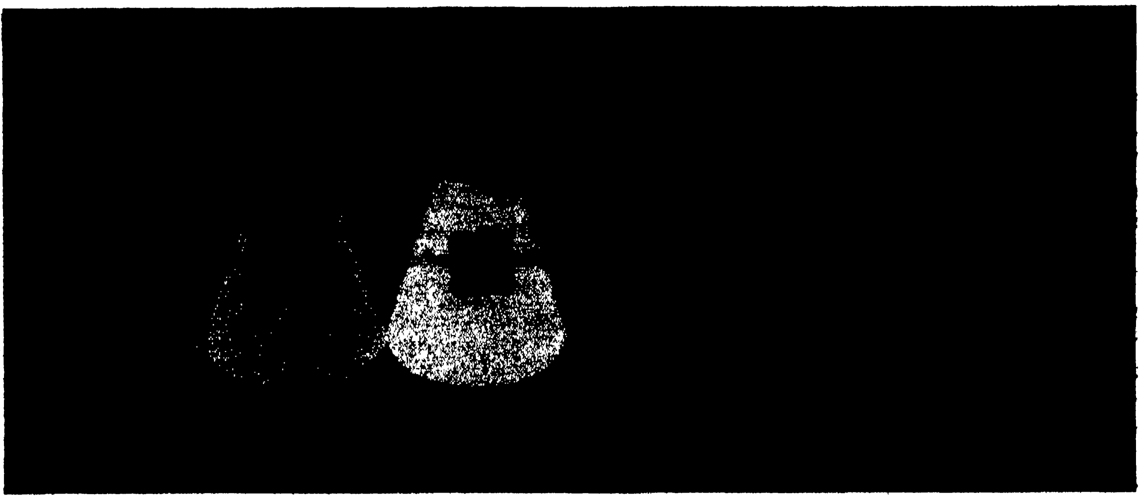


Fig. 9. Same flasks of bacteria, photographed by their own light, showing a partial (and reversible) inhibition of luminescent system through the action of the respective drugs.

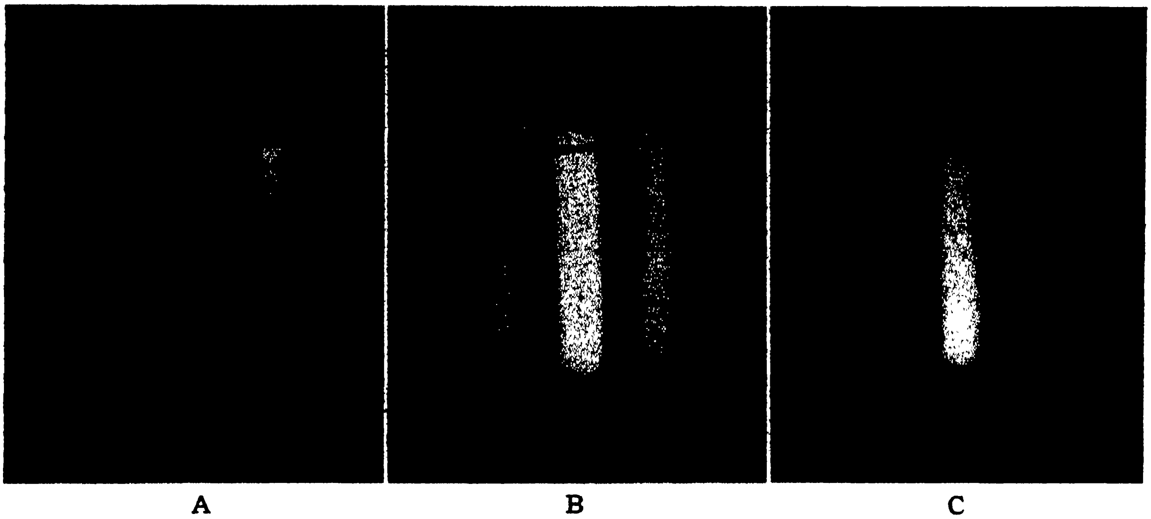


FIG. 10. The relation between temperature and amount of inhibition of luminescence caused by two agents, sulfanilamide and alcohol, which act by different mechanisms. To three test tubes containing equivalent portions of a bacterial suspension, a small amount of sulfanilamide solution was added to the first tube (*left*), pure salt solution to the second (*control*), and alcohol to the third (*right*). Three photographs corresponding in all respects, such as time exposure, were then made by the light emitted from these tubes, when they were placed in a water bath first at a cold temperature (*A*), second at optimum temperature (*B*), finally at an above-optimum temperature (*C*). The relative effectiveness of sulfanilamide is decreased by a rise in temperature, whereas that of alcohol is increased. These effects of heating are largely reversible on cooling, as discussed in the text.

those of proteins, the highly ordered native structure may be altered through many pathways. The opposite effects of very high pressures at ordinary temperatures, and of moderate pressures at high temperatures, are an illustration of this fact. The mechanism of denaturation of proteins under very high pressures has not been clearly elucidated, although it has been suggested (Eyring) that it results from a change in structure of the solvent. It is known, for example, that there are at least five different forms of ice, of which only one (ice *I*, ordinary ice) is less dense than water. Under high pressure other forms of ice become stable, and such a change in the structure of the solvent might be sufficient to lead to instability of the complicated protein structure.

At high temperatures, the volume increase accompanying denaturation has been interpreted as a loosening of the secondary bonds which normally hold the chains of amino acids in a highly specific configuration. With the loosening of these bonds the whole molecule unfolds from a somewhat globular to a more fibrous form, and it is reasonable to expect volume increases of the order of magnitude that have been observed to characterize such denaturation reactions. Pressure opposes this unfolding by opposing the volume increase and, in equilibrium reactions, results in a refolding of some of the molecules. Thus, in a reversibly denatured state, at high temperatures, or in the presence of drugs such as alcohol, urethane, etc., the application of pressure causes bacterial luciferase to be virtually squeezed back together, with a restoration of its catalytic activity. Perhaps an analogous unfolding of the protein is necessary in the combination of certain enzymes with their substrates in the activated state. This would account for the fact that there is a fairly large volume increase of activation in the catalytic reaction, as in bacterial luminescence at low temperatures under conditions where essentially all the enzyme is normally in the native form. The activity of some enzymes, however—for example, yeast invertase—is apparently not accompanied by a significant volume change, suggesting that the active groups of such an enzyme are at the surface of the molecule, so that it does not have to unfold in the reaction. The active groups of the other type in the normal state are presumably folded up within the molecule.

Reversible physiological effects of moderate increases in hydrostatic pressure are widespread, as shown by studies on the contraction of muscle, the characteristics of the nerve impulse, amoeboid motion, sol-gel changes in myosin and other protoplasmic gels, specific precipitation of antibodies, and many other phenomena. The volume increases that have been interpreted as accompanying an unfolding of the protein, however, are of especial interest in connection with the theory of biosyn-

Photograph of the author, taken by the light of some 10,000,000,000,000 cells of ***Photobacterium phosphoreum*** luminescing in 12 liters of oxygenated salt solution.

thesis and growth in general. For, in order to account for the synthesis of highly specific, optically active molecules, it seems necessary to postulate a templet mechanism, the first molecule acting as a pattern for the construction of a second one just like it. Moreover, in order for a native protein to act as a templet, it would seem equally necessary for it first to unfold into a one- or, at most, two-dimensional form. The evidence that has already been discussed leads one to expect that such unfolding will be accompanied by a volume increase of considerable magnitude. Pressure, therefore, should oppose the reproduction of proteins, genes, viruses, and other complex molecules in living cells. It has been recently shown to retard very markedly the rate of bacterial reproduction (*E. coli*). Current experiments (with C. E. ZoBell) indicate that this effect is of general applicability to pure cultures of numerous species of terrestrial bacteria, but that there are "barophilic" organisms, derived from the deep sea, that may grow as well (or better) under 600 atmospheres pressure as under 1 atmosphere, other things being equal. The nature of this adaptation to growth under pressure, as well as the whole relation of pressure to the metabolism of bacteria and other organisms in the ocean depths, offers an unusually challenging problem.

The quantitative theory for certain aspects of biological rate control that has been worked out in connection with luminescence has been extended, since 1942, to other processes. Some of them have already been referred to briefly in the above discussion of pressure effects. A few others are worthy of mention.

The relation between temperature and bacterial (*E. coli*) growth rate during the logarithmic phase at normal pressure is described with considerable accuracy by equation (3), from low temperatures to those that are high enough almost to prevent growth. The diminution in rate at high temperatures is immediately reversible on cooling, provided the cells are not maintained at such temperatures for more than a short time. Consequently, it appears that the rate of growth, like that of luminescence, is limited largely by the activity of a single enzyme system under the chosen conditions, and that the reversible denaturation of a single system again is the limiting mechanism at the above-optimum temperatures. Whether it is the same system concerned at both the low and high temperatures cannot be decided on the basis of the evidence at hand. Unlike luminescence, however, the rate of growth is not markedly increased by pressure at high temperatures, possibly for reasons (among others) that have been indicated in the discussion of volume changes that would be anticipated in connection with the biosynthesis of complex molecules. A pressure of 300–400 atmospheres retards growth at all temperatures in most ordinary bacteria and in all of several genera of yeasts that have been studied in pure culture, according to recent studies by ZoBell and Johnson. In some of these bacteria, however, growth will take place under pressure at high temperatures, where it apparently does not occur at normal pressure, in the same organisms whose reproduction is inhibited by the same pressure at lower temperatures. These results clearly represent the net effect of pressure and temperature on a number of possible limiting reactions, such as the catalytic process of enzymes, the reversible and irreversible denaturation of the proteins, synthesis, etc.

In contrast to the effects of slowing growth and reducing viability at ordinary temperatures, pressure aids in maintaining viability—i.e., slows disinfection—at higher temperatures. This is true even of bacterial spores at 95° C. Moreover, the destruction of spores at this temperature is accelerated by urethane in a manner that is opposed by pressure. In this case, also, the limiting reaction would appear to be one of protein denaturation.

The quantitative relation between inhibition of oxygen consumption and of methylene blue reduction in bacteria by urethane, both in relation to concentration of the drug and to temperature, is described with considerable accuracy by equation (4), as shown by the studies of Koffler, Wilson, and Johnson. Current studies in our laboratory have also shown that the thermal denaturation of tobacco mosaic virus is accelerated by urethane and may be opposed by pressure. The same is true of bacteriophage.

Although not all biological processes can be treated with the same simplicity as luminescence, as one would expect, the studies on luminescence have yielded a rational theory of much broader implications. They have anticipated results in diverse other processes. Numerous other phenomena, for which data adequate for analysis are not yet available, may be expected to disclose a quantitative interpretation on the basis of the same fundamental theory, appropriately extended to deal with more complicated situations. Thus, while the property of luminescence is of questionable advantage to many of the organisms possessing it, there is no doubt that it provides a unique and efficient tool to the investigator of basic mechanisms controlling biological reaction rates.

⁂

SCIENCE ON THE MARCH

PLANT PHYSIOLOGY AND RECENT PROGRESS IN AGRICULTURE

GREAT progress in agriculture often finds its origin in the plant physiological laboratory. Not so long ago manure was the only means the farmer had at his disposal for restoring to the land some of the fertility that cropping had removed. It was only after plant physiologists had learned to understand the fundamental principles of the mineral nutrition of plants that the chemical industry could make available effective fertilizers. Without modern chemical fertilizers large-scale farming would hardly be conceivable.

At the close of World War II plant physiology made another major contribution to agriculture: plant hormones. Today tons of these synthetic growth regulators are produced by the chemical industry, whereas before the war these substances could be found only in gram bottles on the shelves and worktables of research laboratories. These synthetic hormones are used as selective weed killers on crops such as sugar cane, corn, wheat, and barley; they are used in pineapple culture for the control of flower and fruit formation; in the apple industry to check the ruinous preharvest drop of fruit; and in the nursery for the propagation of plants by means of cuttings.

The introduction of synthetic plant hormones to agriculture has been so sudden, and the development of the field has been so fast, that the majority of people dealing with these compounds do not have the time to think of how all this originated. It is, of course, always difficult to find the exact starting point of any discovery. An idea grows slowly in the mind of several investigators, until suddenly one of them formulates it exceptionally well, or performs a critical experiment. With plant hormones, one could say that it all started when Charles Darwin, who was a capable plant physiologist in addition to being the famous student of evolution, studied the phenomenon of how plants orient themselves toward the light. In this study he used grass seedlings, and found that their tips were extraordinarily sensitive to light. An extremely accurate observer, he noticed that in order for a plant organ to curve toward the light it was not necessary for the entire organ to be exposed to the light. When the sensitive tip was exposed to unilateral light, the rest of the plant also curved toward the light source. This occurred even when the unexposed part of the plant was covered by black paper or hidden below the surface of the soil. In other words, Darwin found that the tip of the young plant had some way of communicating with the regions of the plant below it.

Darwin did not suggest a solution of this problem, but it aroused interest in academic circles, and, under the leadership of P. Boysen Jensen, of the University of Copenhagen, the problem was taken under investigation. Gradually it became more and more apparent that a chemical compound naturally occurring inside the young plant served as the messenger which told one part of the plant what was going on in another. At the University of Utrecht, nearly fifty years after Darwin's experiments, it was conclusively proved that a chemical was involved by F. W. Went, who in 1926 separated the active principle from the plant.

Even though proof now existed that different parts of a plant communicate with each other by means of a chemical principle, the nature of the compound still remained obscure. However, by 1934 this problem was also solved. At the University of Utrecht, Kögl and Haagen-Smit identified indoleacetic acid as one of the compounds involved. Since this substance could be synthesized cheaply, it was readily available for experimentation.

About this time plant hormone research in the United States began at the California Institute of Technology, eventually spreading to other laboratories in the country. Soon it was learned that applications of indoleacetic acid to plants will prevent development of dormant buds in the axils of leaves, that indoleacetic acid will promote root formation on cuttings, and that it will cause fruits to set without the benefit of pollination.

In the meantime, homologues and analogues of indoleacetic acid were developed—principally at the Boyce Thompson Institute for Plant Research—which served practical ends better than the naturally occurring indoleacetic acid. Among these new synthetic plant hormones was one—a chlorinated phenoxyacetic acid—which was extremely persistent in the plant and at the same time highly active. This compound, now known as 2,4-D, is by far the most widely used of all synthetic hormones. In low concentrations it could be termed a stimulant, in that it causes pineapple plants to flower, promotes root formation in a variety of plants, and prevents citrus fruit and apples from falling from the tree before harvesttime. In higher concentrations, however, 2,4-D turned out to be the most effective selective plant exterminator ever devised. It destroys the growing point of many broad-leaved weeds, thereby effectively preventing their recovery. Because it is relatively harmless to plants of the grass family, it is widely used on such economically important crops as sugar cane and the cereal grains, all of which are technically grasses.

This great effectiveness, coupled with the relatively low price at which the chemical industry made the material available, has changed the whole aspect of weed control. It can be said that, as a result of the impetus given to the problem of weed control by the discovery of 2,4-D, more progress has been made in this field during the past five years than in the entire previous history of agriculture.

Striking though this application of plant hormones may be, it is by no means all that has been achieved from a practical point of view. In Hawaii, synthetic plant hormones are being used on a large scale to force pineapple plants into flower. Another hormone is used to regulate the vegetative propagation of this plant, and still another synthetic hormone is used to increase the fruit size and step up the sugar content. To make the picture of crop control even more complete, synthetic hormones are also used to prevent pineapple plants from going prematurely into flower and fruit production.

Looking back upon their work, plant physiologists can be gratified with the contribution their research has made to agriculture. Looking ahead, they, and those supporting their work, may be confident that fundamental research will continue to uncover the ways in which plants function, and by applying this knowledge contribute further to progress in agriculture.

J. VAN OVERBEEK
Agricultural Laboratory
Shell Oil Company, Incorporated
Modesto, California

IDENTIFICATION OF THE BEST SOUTHERN NEGRO HIGH-SCHOOL SENIORS

THAT there are in some population groups a "normal" proportion of Negro subjects of very superior psychometric intelligence was shown in a recent article in THE SCIENTIFIC MONTHLY (May 1948) by Martin D. Jenkins, formerly professor of educational psychology at Howard University and now president of Morgan State College. He points out, however, that the populations producing these superior deviates are located almost exclusively in the Northern urban communities, and he adds: "Whether the fact that no children [in Southern states] with this development have been discovered is due to lack of environmental opportunity and stimulation, or merely to lack of identification, is not surely known."

Believing that Negro high-school seniors of superior intelligence do exist in each of the Southern states having segregated school systems and that these able students should be identified and their further education be made possible, the group of educators who make up the Pepsi-Cola Scholarship Board have developed a special program for Negroes in the South. This Board annually awards at least 119 four-year college scholarships and 550 college entrance awards to white and Negro students in the United States and territories. In planning the Southern phase of the program, it has been recognized that the environment of these students is inferior; that their schools, on the average, are substandard; and that these and other factors will depress the psychometric intelligence of such pupils.

The practical problems encountered in attempting to identify the most able Negro high-school seniors in the Southern states are many and varied. In the first place, the cooperation of the schools must be obtained for preliminary identification in the Pepsi-Cola Scholarship program. If a school refuses to enter the program for any of several reasons, able students may be overlooked. Although no fees, premiums, or pledges are required of the school or of the participants, there is a certain amount of suspicion among educators of any scholarship program financed by a commercial concern. (Actually, the Pepsi-Cola Scholarship Board is an independent corporation directed and completely managed by educators. The Pepsi-Cola Company, at the request of its president, Walter S. Mack, Jr., finances the project.) Also, many Negro school officials believe that no pupils of theirs have any possible chance of continuing their education on the college level and that participation is, therefore, pointless. Teachers in industrial types of high schools often fall into this category.

Second, since the preliminary test must be given in each participating school, able students may be at a disadvantage and less able ones aided if the test is not administered correctly.

With these problems in mind, the Board has set up this method of identification: Once a school has registered, candidates for the scholarships are chosen by a senior class election on the basis that they are the ones "most likely to make an important contribution to human progress." Any school, no matter how small the senior class, may have 2 representatives. In the larger schools, up to 5 percent of the senior class may be entered.

These candidates then take the preliminary selection test—prepared and scored especially for this program by the College Entrance Examination Board—in their own schools and administered by a member of the school staff. Approximately 8 of the highest-scoring pupils in each state on this first test then are asked to take the supervised test of the College Entrance Examination Board at its regular series. The Scholarship Board pays the fee required for this test. On the basis of this second test, the highest-scoring Negro student in the state, regardless of the level of his score, receives a four-year college scholarship. The scholarships pay full tuition and fees for the college of the winner's choice, a travel allowance, and $25 a month for thirty-six months of college. The next several in order, up to 5 percent of those participating or a maximum of 5 students in any one state, receive College Entrance Awards, which pay $50 upon the winner's entrance into college. Even low-scoring contestants, if they are the best in the state, receive these awards. In this way, it is believed that the faith of high-school officials in the program can be established, increasing participation, and thus making improved selection possible.

That this method of selection, to a degree at least, is effective is borne out by the results. In spite of all the practical difficulties, participation has increased from 561 Negro seniors, representing 179 segregated schools in 1945, to 1,875 students from 652 schools in 1948. Participation this year included 40 percent of the 1,600 Negro secondary schools in the 17 Southern states and the District of Columbia. A total of 28,140 seniors took part in the class election—or approximately 73 percent of the Negro seniors in the South's segregated schools. According to the 1945–46 figures of the U. S. Office of Education—the latest obtainable—there were 38,466 seniors in the segregated schools of the South.

By having the Negro finalists on the preliminary test given in the schools take the supervised test of the College Board for the final selection, it is possible to make a comparison of their scores with those of white students throughout the country taking the same test. The College Board Scholastic Aptitude Test, which is given only at supervised centers, is required of applicants to many of the Eastern institutions and is taken by what is probably the most highly selected group of comparable size seeking admission to college.

This Aptitude Test yields two scores, one on the verbal factor and one on mathematics. For the purposes of scholarship selection in the Pepsi-Cola competition this year, a final score was made by adding to the score on the verbal section one fifth of the score made on the mathematics section.

Table 1 lists the scores of the 18 winners of four-year college scholarships on both the verbal and mathematics sections of the Scholastic Aptitude Test of the College Board. The scores are reported on a scale ranging from 200 to 800 with a mean, or average, of 500 and a standard deviation of 100. One of the Negro winners scored 690 on the verbal section and 592 on the mathematics. A score of 690 ranks at about the 97th percentile of the College Board norms. This Negro winner from South Carolina is undoubtedly of superior intellect. Nine additional Negro winners scored above the average of this highly selected white college entrance population; 3 colored winners of the scholarships scored below 400 on the verbal section, thus falling into the lowest 16 percent of the College Board group.

TABLE 1

Scores Made by Negro Finalists, 1948 Pepsi-Cola Scholarship Program, on the College Board Scholastic Aptitude Test

Reported Score*	Number of Finalists Winning							
	Four-Year Scholarships		College Entrance Awards ($50)		No Awards		Total	
	V	M	V	M	V	M	V	M†
600–699	1	..	..	1	..	..	1	1
500–599	9	3	3	6	..	1	12	10
400–499	5	8	18	24	2	9	25	41
300–399	3	7	41	38	19	27	63	72
200–299	..	..	18	11	25	9	43	20
Total	18	18	80	80	46	46	144	144
Average Score	488	436	355	387	300	354	354	382

* The average score on the College Board norms is 500, and approximately two thirds of its group score between 400 and 600.

† *V* is the verbal section; *M*, the mathematics section.

For comparison, the 130 white winners of scholarships averaged 762 on this same test, and the 507 College Entrance Award winners (or runners-up) among the white group averaged 696.

This program has been a successful method for identifying superior Negro college material. In the past three years, 59 Negro high-school seniors have been awarded the four-year college scholarships. Although there has been some mortality, 4 of these 59 having had their scholarships canceled, some of these students are now succeeding under current competitive conditions at each of the following colleges and universities: Columbia, Illinois Institute of Technology, Northwestern, Oberlin, Radcliffe, and Western Reserve, as well as at Fisk, Howard, Tuskegee, and a number of other institutions.

There still remain many practical problems in locating Negro high-school seniors of superior scholastic aptitude in the Southern states, but there is no reason to believe that there is a low upper limit of intellectual ability among this group. In the light of results achieved after four years, the Pepsi-Cola Scholarship Board believes that progress is being made.

JOHN M. STALNAKER
Department of Psychology, Stanford University, and *Director, Pepsi-Cola Scholarship Board*

COLUMBIA UNIVERSITY RESEARCH IN CONTEMPORARY CULTURES

A VERY common question with which the intelligent layman greets the anthropologist just returning from a field trip is "What were the people *like*?" The question is a legitimate one, though extremely difficult to answer, and it concerns a scientific problem which, curiously, has been one of the last to be taken up by anthropologists. Usually the anthropologist will attempt to answer the question by describing various facets of the tribe's culture, only to be asked insistently, "Yes, I know. But what are the *people* like?" Anyone who travels, whatever his scientific or other purposes, is of course aware of significant differences in national character; indeed, the people of various countries are well equipped with stereotypes concerning foreigners of different nationalities. These stereotypes, however, from the point of view of the anthropologist, are more an aspect of the culture of the people holding them than they are verified scientific knowledge.

Anthropologists traditionally have been so concerned with preserving some record of fast-disappearing preliterate cultures that one is apt to forget that there is no reason why their well-tested field methods should not be applied to the study of contemporary literate cultures. Great as the theoretical significance and scientific interest of such studies would be, these considerations are far outweighed by the very great practical importance of such knowledge. International relations are constantly impeded by inexplicit, unconscious differences of assumption and cultural expectation; and certainly a major (and irrational) cause of wars is just these cultural incommensurabilities that are inaccessible in most cases to conscious reasoning and judgment. Every successful diplomat has as his chief duty "understanding" the people of the nation to which he has been assigned, and he must acquire a sizable body of experience and information which will enable him to assess, to judge, and to predict the behavior of this people. But his knowledge is often unformulated or inarticulate and usually lost to others scientifically interested; the proved methods of field ethnography might gather much the same kind of data more efficiently, voluminously, and systematically. In peacetime such an understanding of the predictable regularities of national character would tend to preserve world peace; in wartime the military uses of such knowledge for "white" and "black" propaganda, for strategic planning, and for actual military operations are of almost incalculable importance. A notable instance in World War II was our failure to understand and to predict Japanese diplomatic and military behavior in terms of their character structure—when the data, indeed, are quite accessible to a scientific approach.

Whereas most studies of national character structure have suffered from an impressionism inevitable from an unsystematic gathering of relevant data, and from improperly weighted judgments based on too-few instances of the phenomenon, the Columbia University project for Research in Contemporary Cultures is notable for its systematic approach, voluminousness of data, and methodological rigor. Not only are the formal categories of field ethnography used, including extensive and repeated interviewing, but in addition the checks and tests of psychology and the insights of psychiatry are also used. These include test materials (the Story Test, new forms of the Horn-Hellersberg Test, the Thematic Apperception Test, and the Rorschach Test), as well as analyses of novels, cartoons, and motion pictures produced by representatives of the nation concerned. Full verbatim interviews are obtained and kept on permanent file for later reference; of these a keyed abstract is made, so that all the group working on a given nationality may be familiar with all the materials thus far gathered. Seminars are held periodically, preliminary hypotheses advanced, discussed, and later explored in further research. This method avoids the limitations and the biases of the single research worker, since all materials must be examined in a forum of one's fellow-experts.

The researchers themselves are required to complete a Professional Data Sheet, giving residence, schooling, professional work experience in foreign countries, linguistic skills, and culturally relevant personal relationships and religious experience.

The project began officially on April 1, 1947, and at the end of 1947 was employing 62 persons, including anthropologists, psychologists, sociologists, psychiatrists, social workers, and specialists in labor, film analysis, and folklore, all under the directorship of Ruth Benedict. Dr. Benedict has long been famed, even beyond her profession, for her classic work on the *Patterns of Culture,* and for a discerning study of the Japanese in *The Chrysanthemum and the Sword.* The countries studied during 1947 were France, Czechoslovakia, Russia, and China, and some 700 interviews had been obtained by the end of the year. In 1948 the study of Russia and China has been continued, with the addition of Spain, a Mediterranean Arabic culture (Syria), and a continued research on Jewish cultures, which last has important methodological usefulness, because of the usual biculturalism, frequent multilingualism, and cultural articulateness of many European displaced Jewish minorities. The locale of the study is New York City, which is particularly advantaged in the accessibility of large numbers of appropriate, often newly arrived, subjects. The research is sponsored by the Psychological Branch of the Medical Sciences Division of the Office of Naval Research. A sizable body of data has already been gathered, and the work is being continued. Under these auspices, several staff workers have already arranged field trips to the countries concerned.

One interesting scientific result has already emerged: that the *psychological culture areas,* so to speak, of a continent like Europe by no means coincide either inclusively or exclusively with national political boundaries. Although the present research has already demolished many of our stereotypes about foreign nations as a whole, on the other hand, as systematic data accumulate, materials can be refined and corrected by region, occupation, class, sex, and age groups, as well as by period. As a result, in place of untested stereotypes which do not bear scientific scrutiny, we shall have carefully modulated, qualified, and weighted statements for delimited culture areas and for their constituent subcultures. It is possible that the study of character structure in contemporary cultures is among the most important of the researches currently going on in the social sciences —important in a very practical sense for every citizen of the world.

WESTON LA BARRE
Department of Sociology and Anthropology
Duke University

TREES, SHOES, AND PERON

IT HAS recently been announced that Argentina has raised by 50 percent the price of quebracho tannin extract. Since nearly 70 percent of our tannin is imported, and a considerable amount of it comes from the quebracho tree, found mostly in northern Argentina, this announcement focuses our attention upon the importance of a forest product used by every leather tannery in the United States.

Tannins are substances which change raw skins to leather. Untreated animal skins when dry are stiff and unusable; when wet, they putrefy. Tannins combine with the skin proteins, and the result is leather, a product which is strong, flexible, and resistant to water, decay, and wear.

Quebracho, meaning "axe-breaker" (from the two Spanish words *quebrar* and *hacha*), is one of the heaviest and hardest woods in the world. A perfectly dry piece sinks in water, and has a specific gravity of 1.2 to 1.4. In a country where termites make a honeycomb of most woods, quebracho wood is immune to attack. Its great value to leather producers lies in the fact that with proper extraction methods 30 percent of the dry weight of the wood may be taken out as tannin. The green logs are put through a chipper, the chips are cooked, and, when the water has evaporated, the brown, pasty residue is discharged by gravity into jute bags for shipment.

The sharp increase in the price of quebracho, coupled with the growing scarcity or inaccessibility of the trees themselves, forces us to ask how our colonial tanners fared, and also what may be our own native sources of tannin. In the early days of our country, oak and hemlock barks were the chief producers of tannin. Since that from the hemlock gave a reddish color to the leather, a certain amount of oak tannin was mixed in to counteract this effect. The discovery of quebracho, and the difference in the standard of wages between the United States and South America, led to an ever-increasing use of the South American product and the neglect of our own resources. Whether or not the current increase in price will make it economical to utilize native materials, it is at least of interest to survey them and to see what they have to offer. It should be mentioned that there are other foreign sources of tannin now being exploited, but that together they occupy a secondary position to quebracho. Three of these are wattle extract from Africa, sumac from Italy, and eucalyptus from Australia.

The leaves and bark of a great many kinds of woody plants, both native and foreign, contain tannins; in addition, the heartwood of some of them also yields these materials. The important questions, other than those relating to labor, are: How high a percentage of tannin will the plant yield? How numerous and how large are the plants themselves? Are they readily accessible?

At the present time, about 90 percent of the tannin actually produced in this country comes from native chestnut wood, with the remainder derived from oak and hemlock barks (Bandekow, R. J. *J. For.*, 1947, **45**, 729–34). Thousands of chestnut trees were killed by the blight introduced from Asia about 1906. When these standing dead trees have been cut, there will presumably be no others to take their place, since the disease has practically wiped out this valuable species. The tannin content of the heartwood is not particularly high (5–10 percent), but it is economical to extract it because the chips can then be used for pulp. Bandekow lists a considerable number of native woody plants, together with the tannin content of their bark or wood, and selects several which seem to show special promise.

The sumacs are ubiquitous shrubs or small trees, growing as "weeds" even on poor soils, and are characterized by their milky sap and compound leaves, which, at least in the North, turn a brilliant red in autumn. The dried leaves and stems contain from 25 to nearly 30 percent tannin.

Considerable experimental work has been done on the canaigre plant, native to northern Mexico and our Southwestern states. A valuable tannin extract can be made from it, and, furthermore, the plant can be grown and harvested annually.

The bark of the giant Sitka spruce of the Pacific Northwest contains 12–25 percent tannin, and, since the bark is a waste product, extraction of its tannin may prove economical.

Western hemlock bark appears to be available in sufficient quantities to make tannin production worth while, but the same cannot be said for the Eastern hemlock on account of the relative scarcity of large trees. Among the Eastern oaks, only the chestnut oak, a relatively rare tree, produces tannin extracts of outstanding excellence. The other oaks yield material of less than top quality.

A great many other native plants will yield tannin, including the tanoak tree and bitterbrush shrub of the West and the mangrove of tropical Florida, but the present supply of such plants would need to be increased greatly to mitigate the present tannin shortage.

The waste liquor from the spruce wood sulfite-pulping process contains several substances of value in tanning, but prices have been prohibitive.

It can be said, therefore, that research or investigation is needed in learning how to add to the present supply of tannin, and particularly in producing high-grade materials more cheaply.

Wm. M. Harlow
New York State College of Forestry
Syracuse

◆

THE AAAS ENTERS ITS SECOND CENTURY

IN STATEMENTS on this page, the President and the President Elect of the American Association for the Advancement of Science emphasize the coordination and integration of science that has become a most necessary and effective function of our organization.

As Retiring President of the Association, I venture to suggest that, in this age of growing power and shrinking distances, the Association has an increasing responsibility in the international field. Many of our Centennial lectures will emphasize the One World-ness of Science. There is no truly American astronomy. The stars and planets disregard national boundaries. I shall therefore speak at the inaugural meeting on "The One World of Stars," showing how much our theories and interpretations depend on our colleagues in all nations. The capture of atomic energy was a contribution from scientists of many countries; modern genetics grew up under a dozen flags; the most recent and most accurate measurement of the distance to the sun involved the coordinating work of scientists from a score of nations.

For the new era, the Association's planners should work out specific programs for spreading our work and influence abroad. Among other ways, it may be accomplished through an arrangement for technical conferences, through assisting and providing travel fellowships, through participating in international explorations, through directly collaborating with similar organizations and with UNESCO.

The advancement of science is the major role of our Association, as indicated by its name. Building our strength during this coming year for the New Advancement, we must prepare to go forward on a world-wide front. This should be done by the central organization as well as through the affiliated societies. And it is not for America alone we must advance—ours must be a world-wide service.

HARLOW SHAPLEY
Retiring President, AAAS

THE year 1848 is remembered for many notable events. Not least among them was the formation of the American Association for the Advancement of Science. Ours is the only scientific society open to all scientists of this country that brings together their varied disciplines into one organization. It has had a notable history and contributed much to scientific progress. The time is past, however, when one group can serve all the needs of biology, chemistry, physics, and the host of other specialized interests that constitute modern science. Each discipline must have its own professional society. In these days of specialization it is particularly important, therefore, to have one inclusive organization that will make it possible for the practitioner of one science to know enough about the others to work intelligently in his own field. It is the peculiar function of the AAAS to bring together men from all fields of science, either through its meetings or through the pages of its journals, for mutual stimulation and for the development of that breadth of vision so necessary for sound scientific work. As it now enters upon the second century of its history, the Association looks forward to the continuation and the extension of its unique service to science.

EDMUND W. SINNOTT
President, AAAS

IN THE homely philosophy of the whimsical optimists, "the first hundred years are the hardest." Whether or not this applies to the American Association for the Advancement of Science, there can at least be a firm determination that the second century will be even more fruitful than the first. But the attainment of the ambition will not be easy, because the tremendous expansion of science and technology has brought in its wake certain centrifugal tendencies.

The Centennial naturally provokes retrospection, introspection, and prospection. And it is fitting that the past and present should be studied as guides for the future. After the inevitable presentation of ponderous profundities at the meeting, the basic philosophies of the Association should be securely anchored and future procedures clearly charted. But works must follow the words.

If science is to function effectively in emancipating from ignorance, fear, and prejudice and in illuminating and enriching life—if science and technology are to help alleviate want and suffering and make living easier and more secure—then scientists have far deeper and wider responsibilities than ever before. The complexities are greater than ever before; hence the need for crystallizing guiding principles out of the complex mass of knowledge is greater than ever before. There must be some unity of purpose in the diversity of scientific and technologic activities.

E. C. STAKMAN
President Elect, AAAS

THE

SCIENTIFIC MONTHLY

OCTOBER 1948

TIME AND CHANGE IN THE METAGALAXY

HARLOW SHAPLEY

Dr. Shapley (Ph.D., Princeton, 1913) was astronomer at Mount Wilson Observatory during 1914–21, and since that time has been director of Harvard College Observatory. He has received numerous awards for his astronomical work, among them the Janssen Prix, Société Astronomique de France, 1933; Gold Medal, Royal Astronomical Society, 1934; and Pope Pius XI Prize, 1941. Dr. Shapley is the retiring president of the American Association for the Advancement of Science.

HOW MUCH of primeval chaos remains in the sidereal universe, and how much of order, is partly a question of definition. We are inclined, for working purposes, to define stars and their attendant planets, comets, and meteors as orderly parts of the cosmos, and leave in the chaotic category the interstellar and intergalactic dust and gas and the swirls of nebulous matter that are impelled through space by radiation pressure and other dissipative forces. But this definition is quite artificial. No true chaos exists for one who knows all the laws, and all the positions, motions, and masses of all material. In the microcosmic world, the Heisenberg principle would suggest that no such knower could exist. But we are speaking macrocosmically. We are not yet blocked by unknowableness. Our progress toward understanding the nature and the past and future history of the material universe will come through the formulation of partial laws, applying them to incompletely observed phenomena, testing the predictions that evolve from the assumptions and analyses, and thus amplifying the laws.

The Time here considered extends over a hundred million years; the Change cannot be directly observed. It must be inferred from contemporary evidence on objects near and far, and therefore on objects old and young, thanks to the perspective furnished by the finite velocity of light.

Fortunately for our progress, there are observable changes in the stars and even in the galaxies. We measure many kinds of variations: for example, the changes in position on the surface of the sky (very slight outside the solar system); and changes in motion, in light, surface temperature, and spectrum—but scarcely yet in age. From these measures, however, we try to infer the ages of various sidereal bodies, and also their chemical composition, dynamical history, direction of evolution, and destiny. The problems are large and our resources are small. The examination of the ratio of recognized order to apparent chaos, and the relation of change to time, are fields of inquiry that are discouraging to those who would avoid philosophical speculation and would stay near the observables. There seem to be too many vague puzzles. Still, when we note how great has been the progress in our understanding of the stellar universe during the past forty years, we infer that continued observation, and theoretical attacks on the problems we know enough to formulate, will probably provide during the next generation the grounds for demonstrating how primitive our present concepts are.

To help in making obsolete as many of our current views as possible, the Harvard Observatory has specialized in the study of the southern sky, which of necessity is somewhat neglected, because

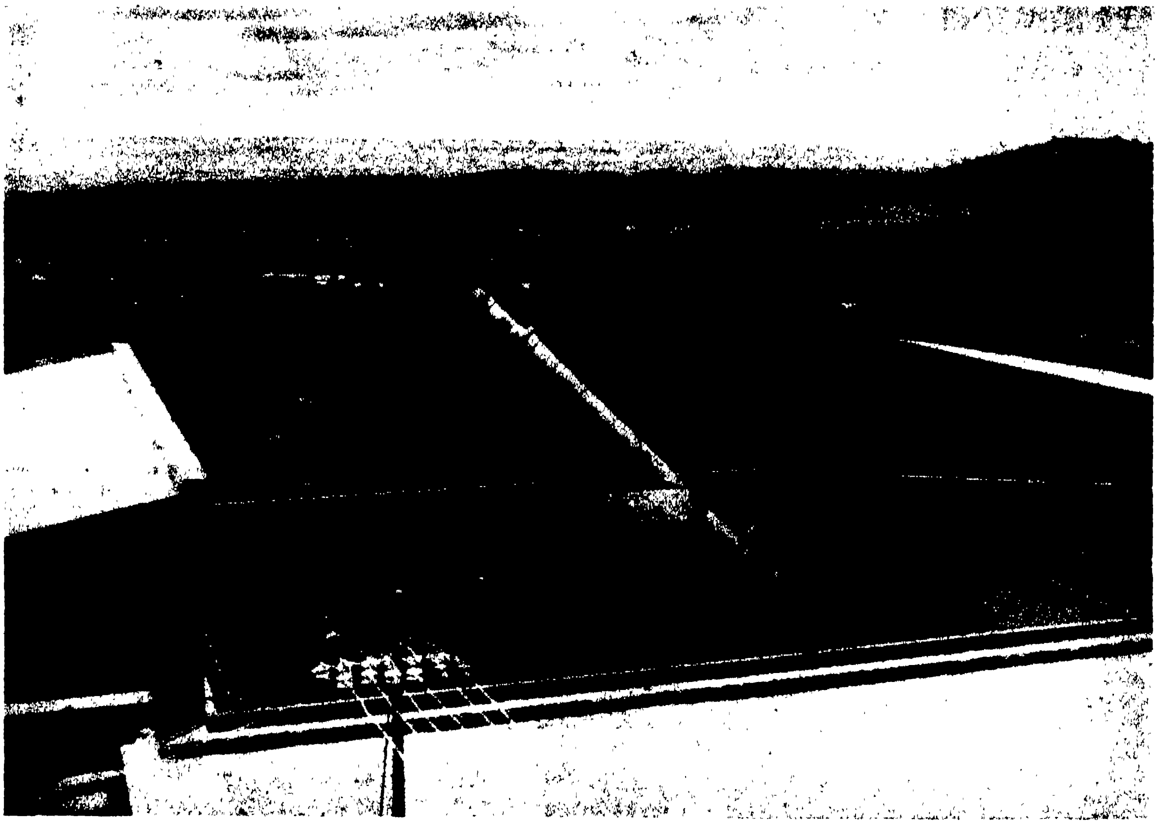


Fig. 1. 24-inch Photographic Refractor on Harvard Kopje, Bloemfontein, Orange Free State.

of the concentration of man power and mind power in the northern latitudes. Harvard's large southern station in the Orange Free State, South Affica, is dedicated in considerable part to studies of the metagalaxy—the over-all system of galactic systems. But the several researches reported in the present communication on "Time and Change in the Metagalaxy" will deal both with problems that could not have been handled from the northern observatories and with others that require work in the north only.

Concerning the metagalaxy. Although the metagalaxy was not in our science thirty years ago, we have now acquired a simply described picture of it. In a vast but perhaps not limitless expanse of space and time we find that the common luminous units are galaxies, of which the principal constituents are stars. The number of stars in a galaxy seems to be of the order of 10,000 million. But there is a wide dispersion in population. A supergiant galaxy, like our own Milky Way system, may contain more than 200,000 million stars, whereas some dwarf galaxies have scarcely more than 10,000,000. Indeed, there may be subdwarf galaxies of still smaller population; and already there is convincing evidence that some of the external star clouds, that we would now call galaxies, are probably no more massive, or luminous, or populous than the greatest of the near-by globular star clusters. Thus, the bright globular cluster 47 Tucanae, at a distance of only 25,000 light-years, is approximately as luminous intrinsically as the faint irregular galaxy IC 1613, at a distance of about 750,000 light-years.

Involved in the star fields of the individual galaxies, especially in those of spiral and irregular form, are both bright and dark nebulae, and groups of stars in all degrees of organization, from the compact million-starred globular systems and the clusters of the open Pleiades type to the multiple stars and simple binaries. Not so easily recognized as stars and nebulae is the widespread interstellar and perhaps intergalactic medium that is composed of electrons, atoms, molecules, and particles—the material (mostly nonluminous) that causes the absorption, blocking, and scattering of light.

This universal background stuff may be the most significant feature of the physical universe, with the stars only a phase that has monopolized human attention because they generate a radiation, as a result of concentration of mass, that happens to affect strongly one human sense organ. Analogously, we have until recently considerable overrated the little segment of the spectrum from violet to red which our eyes recognize. To the "short" of violet and the "long" of red are the energies that empower and reveal the universe.

All these material entities, from interstellar atoms through stars to supergiant galaxies, are of interest in the study of the structure of the metagalaxy; but our attention will be given here mostly to the galaxies themselves, since they permit us to explore the depths of space more extensively than do the individual stars or the interstellar gases.

as-yet-unclassifiable, which is of course an enormously large group containing more than 90 percent of all now known. Distance, with consequent faintness and small dimensions on our photographic plates, hides from us the structures of most of the galaxies that appear on the long-exposure photographs made with the telescopes best suited to the recording of galaxies. The remote objects can be described only on the basis of shape and central concentration. Fortunately, however, a few thousand of the galaxies are near enough to permit somewhat detailed classification and analysis. We like to think that they are a fair sample of all the galaxies within 1,000 million light-years.

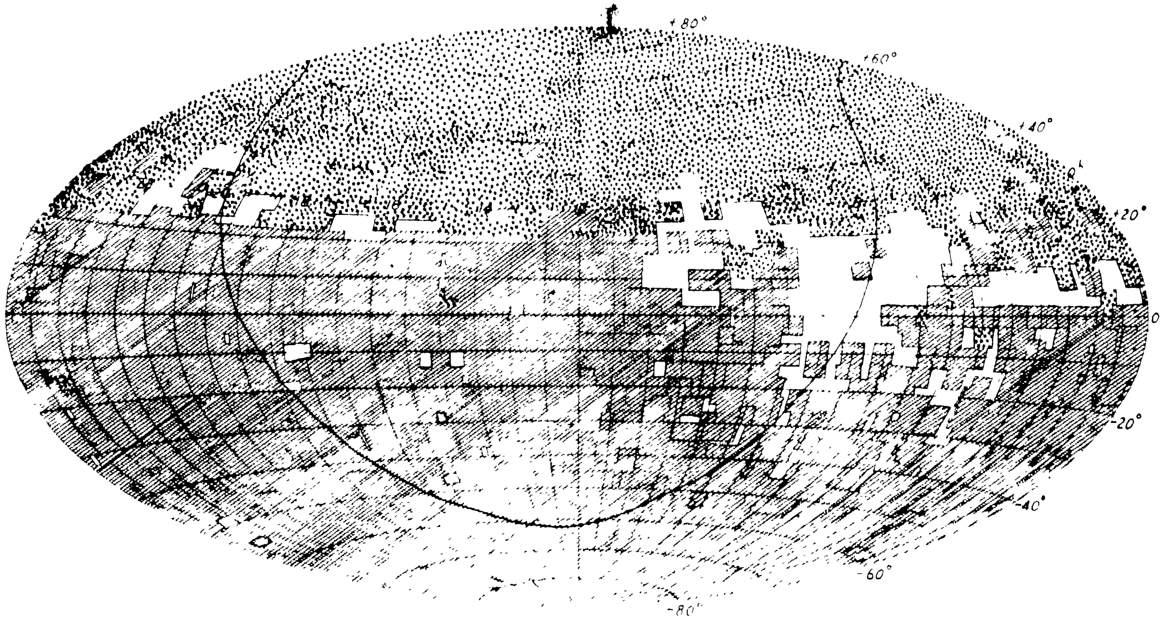


Fig. 2. Coverage of the sky in the eighteenth-magnitude metagalactic survey. Dotted areas were photographed with the Metcalf telescope.

The average separations of stars one from another are much greater, compared with their linear diameters, than the relative separations of galaxies. In ordinary regions of the metagalaxy there are less than 100 galaxy-diameters from one system to the next, and in some rich regions there is an average separation of not over 10 diameters; whereas from star to star in the neighborhood of the sun there is an average separating distance of more than 10,000,000 sun-diameters.

We find a considerable variety among the galaxies, especially in size and structural organization. But the inaccessibility of most galaxies, and our scanty knowledge of those within range, have led us to classify them into relatively few types: the spheroidal; the spiral; the irregular; and the

Our sketchy metagalactic picture can be concluded with a reference to the red shift in the spectra of external galaxies—a shift highly correlated with apparent brightness and therefore with distance, which has thus led to the hypothesis of the expanding universe. That the hypothesis is correct, and refers to all the space we survey, is probable, but not conclusively demonstrated. If we accept the hypothesis, we can refer to the age of the expanding universe. We find evidence that a few thousand million years ago there was an important epoch in cosmic history—an epoch that saw the beginning of the rapid expansion, the beginning also of the earth's crust (and of the earth itself), and a primitive arrangement of the star clusters in our own galactic system. How much of the evolution of stars and galaxies has occurred since that hypothetical zero point in time, we are not yet ready to say. Observations such as those to be summarized here should eventually contribute to our knowledge of the interrelation of Time and Change in the Metagalaxy.

Progress in a metagalactic survey. For a score of years one Harvard telescope in the Southern Hemisphere, the Bruce 24-inch refractor (Fig. 1), has been accumulating photographs, each of three hours' exposure on large fast-emulsion plates, as a part of a comprehensive survey designed to advance knowledge of the external galaxies in the southern and equatorial sky. Fifteen years ago a similar photographic doublet, the 16-inch Metcalf telescope, was moved from the Cambridge station to the new Oak Ridge station of the Harvard Observatory, and there we undertook to cover systematically the northern sky as a part of the same metagalactic survey. In Figure 2 we reproduce a map of the whole sky on an Aitoff projection—a projection which for most of the sky gives an equal number of square degrees for each square inch on the map. The curved line is the projection of the galactic circle.

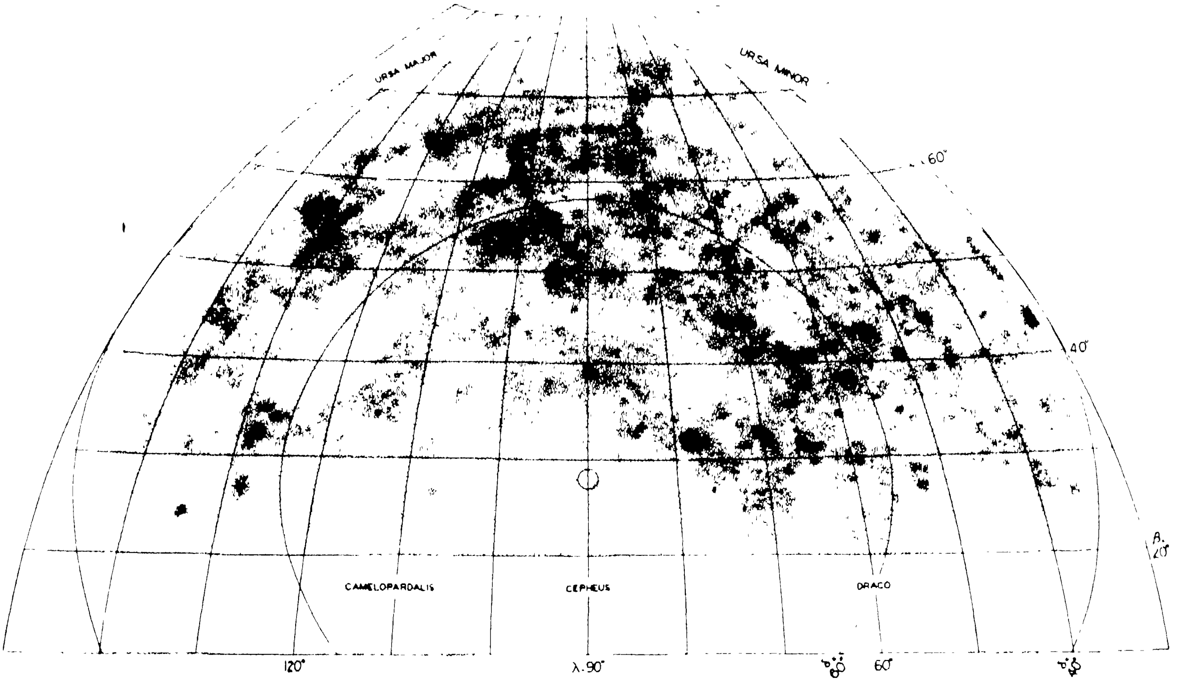


Fig. 3. The distribution of 80,000 galaxies in 3,600 square degrees north of Virgo.

The survey, as indicated by the figure, is more than seven eighths completed. The power of the plates varies somewhat over the sky because of atmospheric and seasonal variations, and because of inequalities from year to year in the speeds of photographic emulsions. But since plates of low quality are systematically retaken, we can claim a fair degree of uniformity for the survey. The aim is to photograph the stars of the whole sky to magnitude 18.0 ± 0.2; such photographs give in general a complete picture of the distribution of external galaxies to magnitude 17.5 ± 0.2. The corresponding distance in parsecs to which the survey reaches for an average galaxy in high galactic latitude, where space absorption is small, is computed from

$$\log d = 0.2(17.5 + 15.2) + 1$$

when, in a survey limited as this one is by apparent brightness, we take −15.2 as the average absolute photographic magnitude of a galaxy. The distance is 35 megaparsecs, or approximately 115,000,000 light-years.

The calculation refers to average systems only. The giant and supergiant galaxies, of course, appear in our census even when they are much more remote than 35 megaparsecs. Some as distant as 200,000,000 light-years are recorded. But occasionally dwarf galaxies at a distance of only 50,000,000 light-years are too faint to appear in our census. Most of the galaxies have luminosities distributed so closely around the mean that we can fairly say that our survey covers metagalactic space to a distance of 100,000,000 light-years. In low latitudes, however, where the brightness has been dimmed by interstellar space absorption, the survey does not reach so far, and near the galactic circle it reaches nowhere at all because of the complete blocking by the clouds of obscuration along the Milky Way.

The long-exposure photographs already available in the eighteenth-magnitude survey show approximately half a million galaxies, more than nine tenths of which are recorded for the first time on these two series of plates. The completed survey, which will include many repeats of earlier photographs, should put on record, and into our statistical analyses, something more than 700,000 galaxies, of which perhaps 20 percent lie beyond the limit of 100,000,000 light-years. If it were not for low-latitude space absorption, the number within this distance could be nearly doubled. In other words, we find that at least a million galaxies are within 100,000,000 light-years of the earth.

If the mass of an average galaxy, including the interstellar material of its domain, is 10^{10} times that of the sun, the mean density in space is $\bar{\rho} = 5 \times 10^{-30}$ *g/cc* throughout this surrounding volume of 4×10^{24} cubic light-years. That value is a lower limit. We have probably underestimated the number and mass contribution of the dwarf galaxies, which because of faintness will not be recorded in the Harvard survey; and possibly also we have underestimated the mass of interstellar material. Probably a better value is $\rho = 10^{-28}$ *g/cc*.

Out to the remotest distance at which an average galaxy could still be photographed with the most powerful reflecting telescopes, there should be at least 200,000,000 galaxies. That distance is approximately 600,000,000 light-years if space is effectively transparent in these outer regions. A supergiant galaxy like our own system could, however, be photographed with many of the telescopes now existing at double that distance. No doubt we have with the Bruce telescope already recorded objects a billion light-years away, but we have not identified the specific images of such galaxies among the thousands at the magnitude limits of our photographic plates. We deduce their presence statistically.

An atlas of northern galaxies. In the northern part of the constellation Virgo is a well-known cluster of some 200 bright galaxies, with magnitudes ranging mostly between 10.5 and 14.5. The diameter of the main body of the group, which is centered on R.A. $12^h\ 30^m$, Dec. +12°, is some 10°, but there are many outlying systems that, on the evidence of comparable brightness, could reasonably be assigned to this large cluster or cloud of galaxies. The most conspicuous extension of the cloud is to the south, through Virgo to the constellation Centaurus, at Dec. −30°. It makes the total extent about 5,000,000 light-years.

To the north of the Virgo cloud is a wide scattering of bright galaxies, covering the north galactic pole in Coma, and spreading over the constellations of Ursa Major and Canes Venatici. In our general study of this important northern region, we have photographed not only the bright galaxies, but also the background upon which they are superimposed. It is possible, therefore, to present, in the accompanying diagram (Fig. 3), a plot on an equal-area projection of the distribution of faint galaxies over all that area lying north of declination +40° in higher galactic latitude than +20°. The survey plates were made with the Metcalf telescope at Oak Ridge, with three-hour exposures on fast emulsions.

The total area of approximately 3,600 square degrees covered by this survey contains about 80,000 galaxies, to the magnitude limit of 17.6, and many thousands fainter. Before making the diagram showing the distribution of the faint galaxies, all the plates have been reduced to a common magnitude limit. Appropriate allowance has been made for overlapping of plates, but no attempt to plot accurately the positions of individual galaxies within each square degree. Although in plotting we have somewhat exaggerated the smoothness of the distribution, the many real clusterings, deficiencies, and other irregularities are

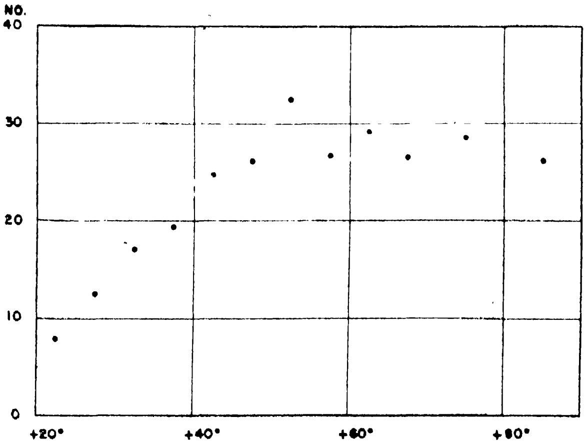


Fig. 4. Illustrating the effect of space absorption on average number of galaxies per square degree. Abscissae are galactic latitudes.

clearly shown. In particular, it is easy to see the effect of space absorption on the number of photographable galaxies in galactic latitudes from + 20° to + 30°. The conspicuous clusterings are now under special investigation with the aid of the Schmidt reflector at Oak Ridge.

A preliminary examination of the dependence of the number of galaxies in this part of the northern sky on the galactic latitude is assisted by Figure 4, where the average number of galaxies per square degree is plotted for 5-degree intervals of galactic latitude. (For latitudes greater than + 75° the material is supplemented with observations not represented in Figure 3.) There appears to be no certain dependence on latitude above + 40°. Below + 40°, the number of galaxies per square degree falls off rapidly—more rapidly, in fact, than in most other parts of the sky. The strong space absorption shown here for latitudes between + 20° and + 40° in the northern sky is chiefly the "flare" of absorption in Cepheus, previously pointed out by Hubble,[1] and later confirmed by researches on the North Polar Cap by Shapley and Jones.[2] This Cepheid flare extends over the North Pole of the sky, and produces a considerable absorption and some reddening in the important field of the North Polar Magnitude Standards near Polaris.[2] The flare is limited in galactic longitude, as can be seen in Figure 3.

It is of passing interest to note that we have photographed 1,760 galaxies through the bowl of the Big Dipper. The galactic coordinates of its center are $\lambda = 105°$, $\beta = +58°$.*

The galactic anticenter. Diametrically opposite the galactic nucleus, which lies chiefly in Ophiuchus, Scorpio, and Sagittarius, is a less significant but nevertheless important region of the sky. In the anticenter region we can hope to discover most easily the extent of the Milky Way in its own plane and effectively explore the outer regions of a spiral galaxy.

Obscuring matter along the galactic circle in Taurus and Auriga, the anticenter constellations, blocks measurements in low latitudes. Any useful distance indicator requires transparency, or quantitative knowledge of the absorption. By making a careful simultaneous study of Cepheid variable stars and external galaxies in latitudes higher than 15 or 20 degrees, on both sides of the Milky Way, we can, however, get at least an approximate measure of galactic extent in the direction opposite the center. The periods and brightness of the Cepheids along the borders of the Milky Way give us the first estimates of distances. But they give maximum values. The number of faint galaxies provide a measure of the total amount of space absorption in each field photographed for variables and, therefore, indicate how much correction must be made to the photometrically determined distances of the Cepheid variable stars in order to get reliable values.

The systematic study of the anticenter variables has been in progress for five years. The region under survey is shown, in galactic coordinates, in Figure 5. Several hundred plates have been made for the discovery and measurement of the faint Cepheids. Various photographic telescopes are being used, chiefly those located at the Oak Ridge station; the most effective for the work, because of large field and deep reach, are the 16-inch Metcalf refractor and the 24-inch Jewett Schmidt-type re-

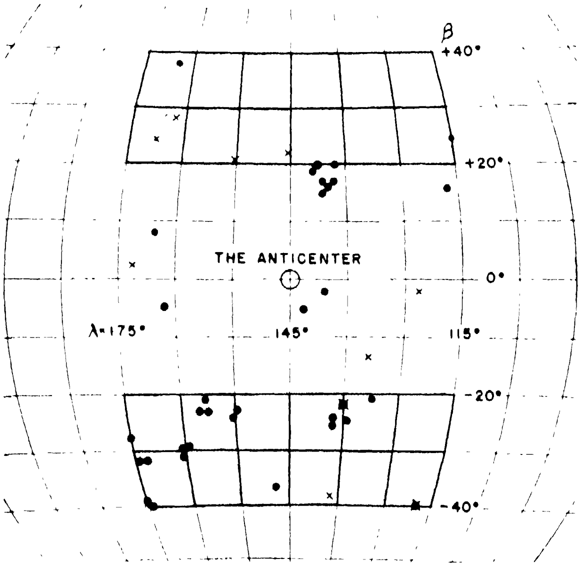


Fig. 5. Diagram showing the two anticenter regions under special exploration for Cepheid variable stars. The cluster variables in this part of the sky that were known at the time work was begun are shown as dots when brighter than photographic magnitude 12, as crosses when fainter. Those close to the galactic circle, $\beta = 0°$, are generally too much troubled by space absorption to be useful. More than 100 new variables have now been discovered in the two regions, and many are cluster-type Cepheids.

---

* The examination of the photographs for the work described in this section has been chiefly in the hands of Rebecca Jones, with assistance from Frances Wright and others. The photographs with the Metcalf refractor were nearly all made by Henry Sawyer at the Oak Ridge station. (Detailed description of the work will be published elsewhere.) The dependence of frequency of galaxies on galactic latitude can be better analyzed after the photographic magnitudes of the individual objects have been determined.

flector. The variables previously known and catalogued in the anticenter region are in general too bright and too near, or too much involved in the lower-latitude absorbing clouds, to be useful in this work. The 150 variables, newly discovered for this study by Martha Dowse, are mostly fainter than the fourteenth magnitude, and many are located in the regions where the population of galaxies is sufficient to indicate high space transparency.

Of the new variables, many are of the cluster type, and at this stage of the research, we can report that several are as far from us in the anticenter direction as we are distant from the galactic center. Presumably these faint variables are members of the galactic system, not intergalactic; but some of them may lie in the "star haze" surrounding the discoid of the galaxy,[3] rather than in the main body of our flattened spiral system.

Eventually the research on the anticenter region will involve star-counting and the analysis of the colors and spectra of faint stars, as well as the present exploration by way of galaxies and Cepheid variables.

Variable stars in the Clouds of Magellan. The Magellanic Clouds (Fig. 6), as the nearest of external galaxies, are important in our study of the metagalaxy because of their resolvability, their irregular forms, and their significance in the local cluster of galaxies. Their Cepheids are of wide cosmic usefulness. They provide much information valuable to the study of Cepheids in our own galaxy. Recent investigations in the Clouds have brought out some new facts about Cepheids and have reinforced others already surmised. A few may be summarized as follows:

a) Continued study of the photographic plates has increased in the two Clouds the number of recognized classical Cepheids (periods greater than a day) to more than 2,500. Some scores of irregular variables and eclipsing stars, found and studied on these photographs, are mostly giant and supergiant members of the Clouds, rather than intervening galactic variables. Many of them are a thousand times the luminosity of the sun.[4]

b) About a dozen cluster-type Cepheid variables (periods less than a day) are found on the photographs of the Small Magellanic Cloud, but probably without exception these objects are all superposed. No cluster variables that are definitely members of either Magellanic Cloud have been found, notwithstanding the use of a special series of plates made with the 60-inch reflector at the South African station. Certainly such stars are very rare in the Clouds, in striking contrast with the situation in the galactic system where the cluster-type Cepheids are much more abundant than the classical Cepheids.

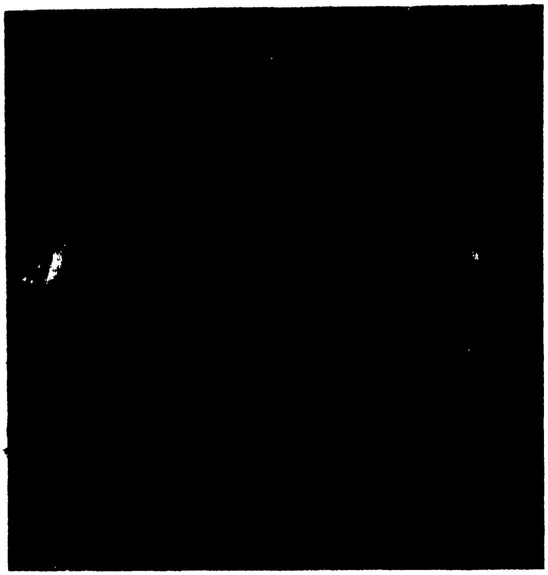


Fig. 6. Magellanic Clouds, the nearest external galaxies. The globular cluster 47 Tucanae (Fig. 10) appears at the edge of figure to right of the Small Cloud.

c) In the core of the Small Magellanic Cloud are many classical Cepheids with periods abnormally long—10–20 days—with scarcely any with the dominant period length, that is, of 2–5 days. A similar scarcity is shown in our accumulating data on the globular star clusters, and it is particularly conspicuous for classical Cepheids in the direction of the galactic center, where the "16-day Cepheid" is especially dominant. In all three regions[5] the star density is high. This peculiar distribution of the periods is doubtless a matter of evolutionary significance in the history of rich stellar systems.

d) Certain regions of the Small Cloud have been thoroughly studied for the magnitudes and periods of the variables. The danger of preferential selection, on the basis of period or brightness, is thus eliminated. The distribution of the lengths of the periods in these thoroughly explored regions is shown in Figure 7. The maximum frequency appears for periods of 2.5 days, whereas, for the regions in the solar neighborhood, the maximum is between 4 and 5 days. The contrast is in small part a matter of incompleteness of the surveys in our own galaxy; but clearly it indicates a basic difference in Cepheid phenomena in adjacent galaxies.

c) As a further result of the study of the periods and light curves of more than 500 of the Cepheid variables distributed all over the Small Cloud, we have confirmed, as noted above, the fact that Cepheids of longest periods show a preference for the dense central regions, and that the shortest periods predominate in the intermediate and boundary regions. This peculiar phenomenon has not yet been established for the Large Magellanic Cloud where, so far, much less material is available and where, very surprisingly, scarcely a variable with period less than 2.5 days has been found.

f) A detailed analysis of the dispersion of magnitudes about the period-luminosity curve shows that half a dozen factors contribute to the spread in brightness. The four most important are the intrinsic differences in luminosity for stars of a given period; photometric measuring errors, including the doubling effect in the crowded star fields; thickness of the Cloud in the line of sight; and patches of absorbing material within the Clouds.

g) An apparent extension, or wing, of the Small Magellanic Cloud was recently discovered. Its distance has now been determined by means of Cepheid variable stars, and it is shown to be a physical part of the Small Cloud—not an independent system at some other distance. The wing extends toward the Large Cloud.[†]

Distribution of luminosity in galaxies. With the assistance of Dr. Patterson, I have begun systematic studies of the distribution of luminosity over the surface of the nearer spheroidal and spiral galaxies. The photographic work has been done at the Oak Ridge station with a flat-field refractor, and the plates have been carefully standardized throughout the processes of exposing, developing, calibrating with the sensitometer, and analyzing with the microdensitometer. Several workable galaxies generally appear on each plate. About half the photographs have been reduced and discussed, and a report on one phase of the work has already been published. Another discussion will appear in

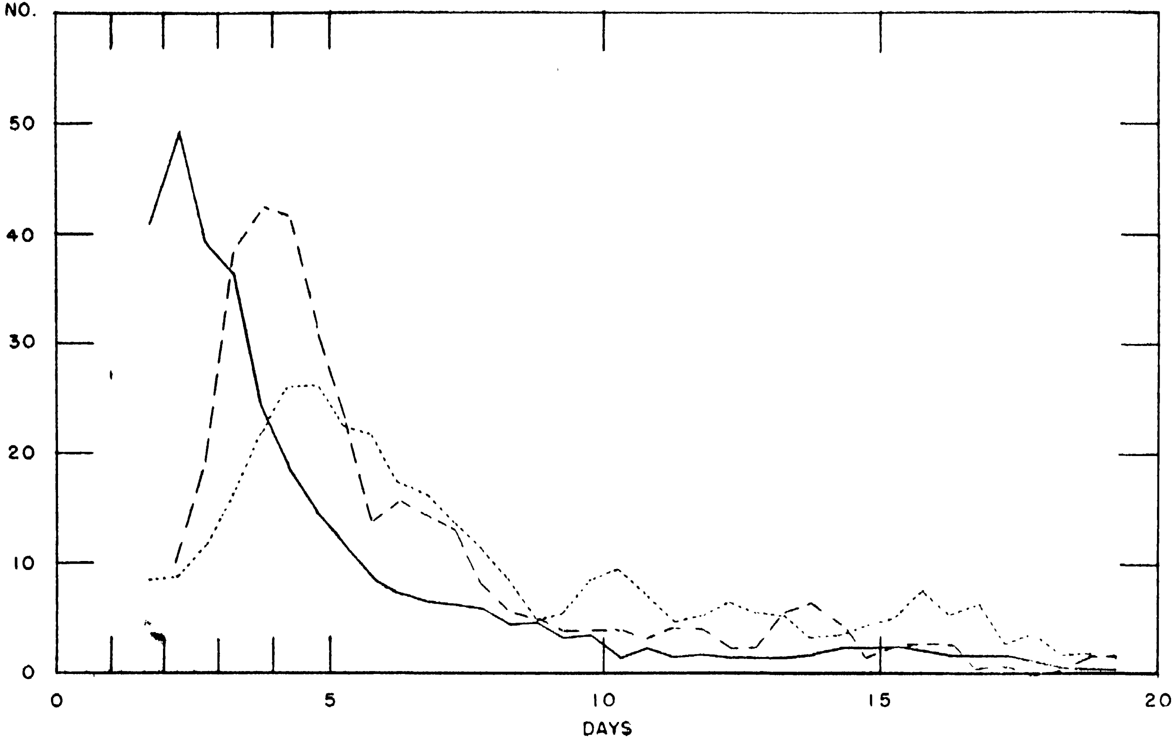


Fig. 7. The frequency of periods of Cepheids. *Full line*: 531 Small Cloud variables; *broken line*: 144 Large Cloud variables; *dotted line*: 288 galactic variables. For convenience of comparison, the curves have been reduced to a common total of 250 by multiplying the ordinate for each plotted point by 0.47, 1.74, and 0.87, respectively.

[†] In the investigations of the Cepheid variable stars in the Magellanic Clouds, reported in the foregoing summarizing paragraphs, I have been assisted by Virginia McKibben Nail, Richard A. Craig, Frances W. Wright, and Martha Dowse. The photographic work in the Southern Hemisphere has been under the direction of John S. Paraskevopoulos, superintendent of Harvard's South African station.

a Harvard Observatory publication. Two of the principal results may be summarized:

a) The measurements of a number of spiral galaxies, whose galactic planes are highly inclined to the line of sight, indicate that only about 20 percent of the light, sometimes much less, of a typical spiral is located in the spiral arms themselves. Some of it is in the nuclei, of course, but most of it is in the little-noticed background of the galaxy. The evolutionary implications are obvious.

b) The over-all dimensions of galaxies of the various types appear to be much alike. Earlier studies had seemed to indicate that the open spirals were several times more extended than the spheroidal galaxies. But it now appears from the microdensitometry that the spheroidal structureless systems can be traced to great distances from their nuclei. The arms of a spiral galaxy (and three fourths of the galaxies show these spiral arms) appear, therefore, to be concentrations, or perhaps condensations, within the systems rather than ejections from a central nucleus. In Figure 8 the distribution of the diameters of the various types of galaxies, as shown by the new series of measures, is illustrated, with the abscissae expressed in kiloparsecs.[6] The measured diameters of the smallest objects are about 6,000 light-years, the median value is twice as great, and the largest systems are more than 30,000 light-years in diameter and therefore approach comparability with our own giant galaxy.

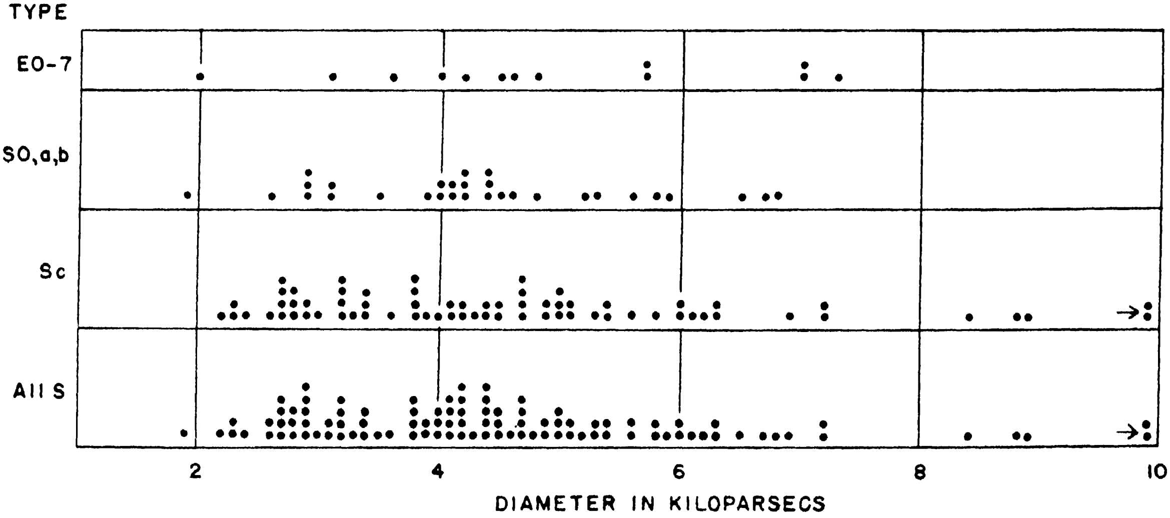


Fig. 8. Distribution of dimensions of galaxies. One kiloparsec equals 2×10^{16} kilometers.

On the evolution of galaxies. From the foregoing new observations on the dimensions of galaxies and the significance of spiral arms, one might provisionally conclude that the open spirals do not necessarily represent the older or more developed state of a galactic system. If there is development along the well-known sequence of galaxy forms, it may be in the direction opposed to that usually assumed; that is, in the direction from the most open spiral (Sc, Fig. 9) to the less open spirals (Sa) to highly flattened spheroids (E7), toward the spherical galaxy (E0). In such a scheme globular clusters like the giant 47 Tucanae (Fig. 10), or the still more gigantic globular Messier 87, which is a typical spheroidal galaxy, might be considered the end products of galactic evolution. One rather colorless argument for this hypothesis is the above-mentioned suggestion that spiral arms may not represent rotational ejection from the nucleus of a developing galaxy, but represent instead merely an inner organization of the nebulosity, star clusters, giant and supergiant stars. But such an interpretation of the arms does not necessarily define the direction of development. Positive arguments for a spiral-to-spheroidal trend in galactic evolution are the following:

a) Because of the shearing action in the rotation of a galaxy of Class Sb or Sc (our own galactic system, for instance), the star clusters and star clouds which characterize these open galaxies will be gradually dismembered and the whole system must proceed toward structural uniformity. Thus, as Bok and others have shown, the trend of loose star clusters is toward dissolution. We have visualized no reasonable mechanism for constructing star clouds or loose clusters of giant and supergiant stars out of the uniform star fields that characterize the structureless spheroidal galaxy. If evolution were in the direction of spheroidal to spiral, what could be the mechanism that develops irregular clustering of giant and supergiant stars?

b) The radiant life of supergiant stars is relatively brief, according to current ideas on the atomic generation of stellar energy. Spitzer and Whipple have discussed a process by which the red supergiant stars can develop out of dust clouds. But such stars apparently do not and cannot evolve in a spheroidal galaxy; my observations on the giant globular clusters indicate that interstellar dust does not exist profusely if at all in a globular cluster or in a spheroidal galaxy, which is therefore a sterile place for the production of giant young stars. Open spirals and irregular galaxies, on the other hand, are characteristically full of dark and bright nebulosity. There the material is available for the birth of giant stars of low density.

The time required for the transition from a very open spiral, with its star clusters, nebulosity, dust clouds, and supergiant stars, to a globular galaxy from which all this population, as well as the flatness and spiral structure, has disappeared, would undoubtedly be so long as to strain severely the short time scale of about 10^{10} years. The short time scale, however, should not be held as conclusively demonstrated, notwithstanding strong evidence favoring it, such as the age of the earth's crust, the speed of the expansion of the universe, and the present existence of open star clusters in the Milky Way. But even within the framework of this limited time schedule one could look at the development of galaxies from the standpoint of different speeds in two different phases; the first being the rapid and explosive adjustment, when the universe was "young"—an adjustment which resulted in sidereal matter aggregating into unit galaxies of

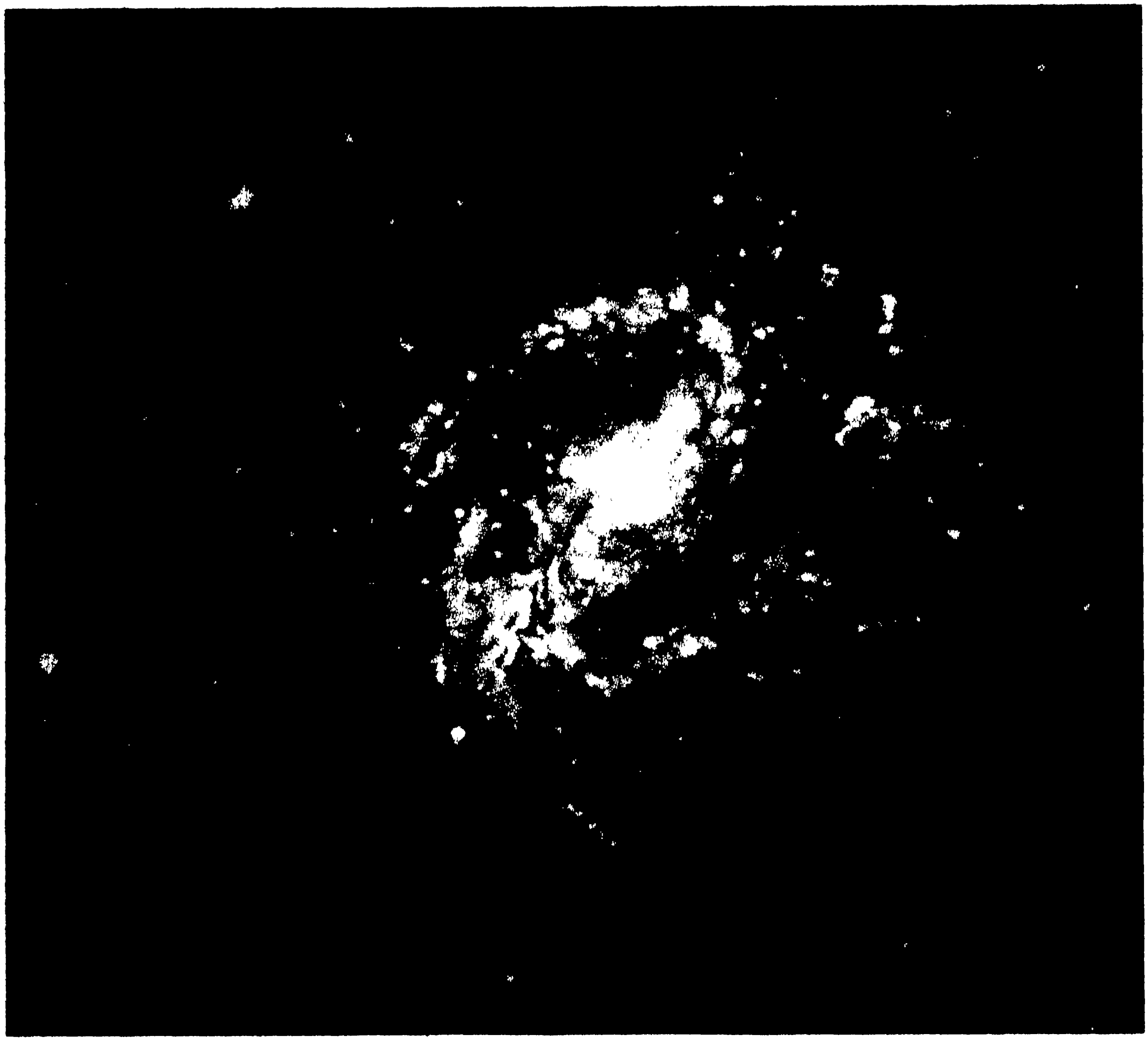


Fig. 9. Messier 83, a bright southern spiral of class Sc.

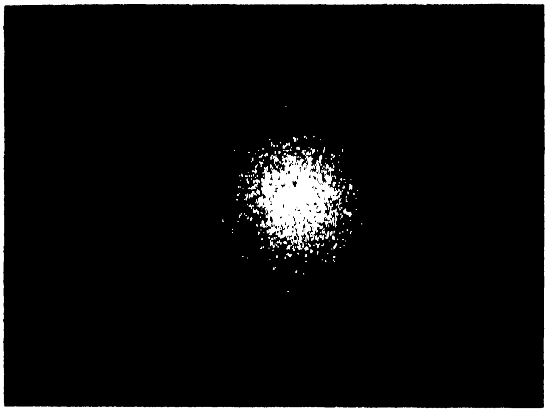


Fig. 10. The globular cluster 47 Tucanae.

many sizes and forms, much as we see them today; and the other phase being that more deliberate dynamical and radiational process that is now going on, which in the long run must tend to smooth out and perhaps round up both the irregular galaxies and the considerably nucleated open spirals. There is much room here for theoretical analysis.

Of the five brightest systems in our local group of galaxies, two are irregular (the Magellanic Clouds); of the total group of a dozen, half are irregular—a much higher proportion of chaos-touched systems than we observe elsewhere in metagalactic space. If these irregular star clouds were isolated from perturbing giant galaxies like the Andromeda Nebula and our own system, they might eventually become much smoother. The Andromeda Nebula and our own galaxy both have faint approaches to sphericity in the apparently spheroidal haze of distant high-latitude stars, which for our system includes the high-velocity "escape" stars of the cluster-variable type. But are these spheroidal hazes, which surround the massive discoids, incipient or vestigial?

REFERENCES

1. Hubble, E. The Distribution of Extra-Galactic Nebulae. *Astrophys. J.*, 1934, **79**, 35; *Mt. Wilson Observatory Contribution 485*, 1934.
2. Shapley, H., and Jones, R. B. Survey of 16,639 Galaxies North of Declination +70°. *Harvard Ann.*, 1938, **106** (1), 1–361.
3. *Harvard Reprint 173; Proc. Nat. Acad. Sci.*, 1939, **25**, 423–8.
4. *Harvard Reprint 306*, 1948; *Proc. Nat. Acad. Sci.* 1948, **34**, 178.
5. *Harvard Reprint 192*, 1940; *Proc. Nat. Acad. Sci.*, 1940, **26**, 105; *Harvard Reprint 241*, 1942; *Proc. Nat. Acad. Sci.*, 1942, **28**, 200.
6. *Harvard Reprint 238*, 1942; *Proc. Nat. Acad. Sci.*, 1940, **28**, 189.

ASTRONOMER

All night he watches roving worlds go by
Through tempered glass, his window on the sky,
Feels in his own the beat
Of some far mightier heart, and hears
The mystic concert of the spheres.

Scanning infinitude, that dim vault kissed
By light, he centers on one star long missed.
For this unstable ray,
This bright but strangely prodigal beam,
The torch within his mind will gleam.

Roland Ryder-Smith

THE SEVEN DEGREES OF ADVENTURE

MARSTON BATES

Dr. Bates, an entomologist (Ph.D., Harvard, 1934), has been associated with the International Health Division of the Rockefeller Foundation since 1935, working in Albania until 1939, and then spending a year in Egypt. In 1940 he moved to Villavicencio, Colombia, where he is working on jungle yellow fever.

SCIENTISTS frequently become involved in the problem of writing books about their travels in the tropics. The question of adventures, in such circumstances, must be taken very seriously. It may be possible to explore the rain forest without encountering adventures, but the moment you use the word "jungle," the reader starts looking for an adventure, and he will be happier if adventures occur at fairly regular intervals throughout the text. Keeping the reader happy is an important consideration; if he gets bored, he may shut up the book and go off to a movie, or even remember that he is supposed to help his wife wipe the dishes.

I started to write a book about the tropics the other day, and these factors led me, logically, to undertake an analysis of the phenomena of adventures. Clearly, adventures are natural phenomena and, as such, subject to attack by the ordinary methods of scientific analysis. Preliminary study of this sort should enable any scientist to handle his adventure problems in a reasonable way, and the results of my study may be helpful to colleagues who find themselves in a similar situation.

I have always entered adventures in my journals, along with the more routine natural-history observations; in addition, I have had the foresight to make notes on the adventures of friends and casual acquaintances. These made a rather unwieldy accumulation that plainly would have to be reduced to some sort of order before it would be possible to compose any coherent narrative. After various preliminary tests, I decided that a punched-card system would provide the best method of analysis.

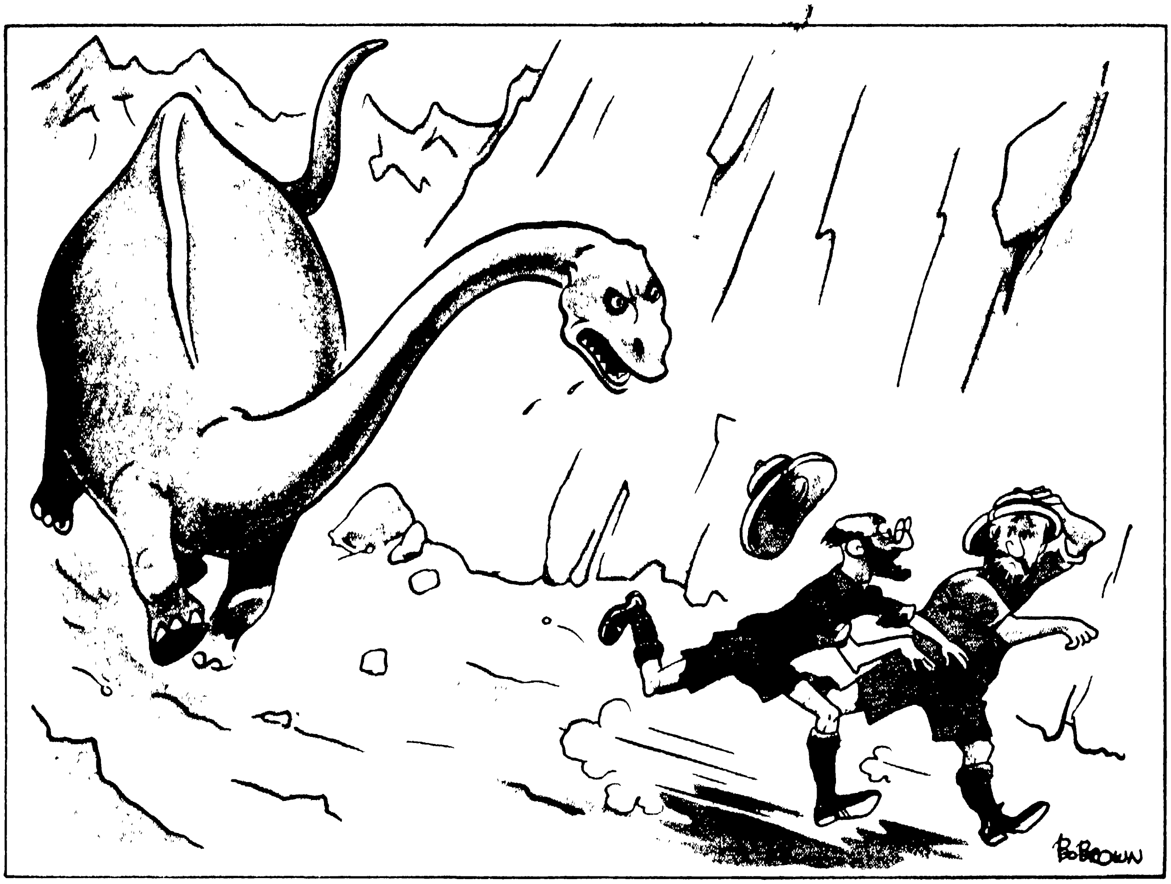
Punched cards are an indispensable tool in scientific work. I don't mean the punched cards used by the statistical Johnnies with big budgets: cards with orderly patterns of holes punched by electric machines, those superhuman monsters that sort the cards, count them, digest them, and, finally, with perspicacious efficiency, flush them down the drain. The common, nonstatistical, scientist uses another kind of punched card. It has rows of holes all around the margin and ample space for writing in the middle. You write out the data (adventure, in this case) on the card, and then study its characteristics. You cut notches into different holes along the margin, according to these characteristics, and then file the cards.

If, for instance, the adventure includes a poisonous snake, you convert the third hole from the left on the top of the card into a notch; if the adventure concerns a drunken Indian trying to stab someone in the back, the fourth hole from the left; and so on. Thus, if you want to compare all the adventures involving drunken Indians with knives, you simply borrow your wife's knitting needle, run it down the pile of cards through the fourth hole from the left on top, shake the cards, and all those with drunken Indians fall out.

I must acknowledge that my secretary, Miss Carmichael, did most of the work. I went through my journals carefully and marked my adventures with a red pencil; she then copied each one out on a punched card. We worked out the analyses together. We soon found that there weren't enough adventures in my journals for statistical significance, so Miss Carmichael undertook a general review of books on travel in the tropics, making cards for all the adventures. This kept her busy for a long time and helped me a great deal, since I didn't have to think up things for her to do in the mornings.

We found that adventures were most satisfactorily classified according to a scale of degree of intensity, and that they fell into seven categories. Adventures of the First Degree are those in which the author faces death. Unfortunately, I never knowingly faced death in the tropical forest, so there were no examples for study in my journals. We found many fine ones, however, in the travel literature: the subconscious "freeze" just as the intrepid explorer is about to tread on a bushmaster (*Lachesis mutus*); the drunken native lunging in the dark with his machete; the rifle that misfires as the cornered lion springs forward in fury.

Miss Carmichael collected two hundred and six-



Reprinted by permission from *The New York Times Book Review*

"What a footnote this'll make to Chapter Five!"

teen specimens of Adventure of the First Degree from her meticulous study of seventy-six volumes of travel and exploration in tropical regions. From these we defined certain peculiarities of the circumstances under which the authors of these volumes faced death. There were poisonous snakes in 39.2 percent of the adventures; in 5.1 percent of these, however, we were able to determine, from indirect evidence inadvertently included by the author, that the snake was really of a harmless kind. Authors should be more careful about these things. If they expect to face death from a poisonous snake in the water, they should first make sure that there are poisonous water snakes in the region they are exploring.

Hostile Indians, drunken savages, belligerent natives, or other varieties of *Homo sapiens* were involved in 13.5 percent of the adventures. The author always gave details from his point of view, but in no instance was there any adequate statement of what provocation he might have given these Indians, savages, or natives.

Mammals other than man constituted the peril in 26.3 percent of the cases. There was a nice geographical correlation here, since the majority of these adventures occurred in the Old World tropics. Even a seasoned explorer has trouble achieving First Degree Adventure with the tropical American mammals.

It is remarkable that the author was not actually killed in any of the two hundred and sixteen situations. (Of course, if he had been killed, he wouldn't have been able to finish the book, but you would think that maybe his wife could have taken over or something.) We got interested in this and checked on the longevity statistics for tropical explorers. It turned out that on the average they live to a very ripe old age, generally dying of pneumonia in England or in an automobile accident in the United States. I feel strongly about this because, although I never experienced any First Degree Adventures in the tropics, I have frequently faced death in Harvard Square or at

Broadway and Forty-second Street. Perhaps I could work that into my book by including a final chapter on Facing Death in Harvard Square (although it looked for a while this morning as though my wife might have to write it as an epilogue, for posthumous publication).

Next there are Adventures of the Second Degree. These have to do with women. Miss Carmichael looked carefully through my journals, but was unable to find any. (My wife also has access to my journals, so this is perhaps not strange.) It is queer, though, that she found no examples in the seventy-six volumes of travel and exploration, despite the fact that slightly more than half these books were written by bachelors. Surely they must have been exposed to Adventure of the Second Degree at times on their travels: a willowy blond from Texas in the cocktail lounge of the steamer; a shy, dusky maiden bathing in a lonely stream in the forest; some unforgettable figure in the wild abandon of a savage dance propitiating some god of fertility. By all logic of human nature, these authors must have encountered Adventure of the Second Degree, but they are remarkably reticent about it.

Adventures of the Third Degree involve sublethal danger; of the Fourth Degree, pests; and of the Fifth Degree, officials. These are all very interesting categories. Sublethal danger is a particularly protean phenomenon: it includes disease (fatal diseases, of course, are never listed among adventures); hunger; being lost in the woods; bodily injuries and mutilations; and major inconvenience like missing airplane connections.

Pests (Fourth Degree Adventures) include perhaps the commonest and most-written-about incidents of life in the tropics. The catalogue is endless: scorpions, mosquitoes, black flies, fleas, bedbugs, centipedes, redbugs, ants, and my wife's friends, the cockroaches. It is perhaps arbitrary to separate officials (Fifth Degree Adventures) from other kinds of pests, but the separation has convenience, since adventures with officials and with insects generally take place in different settings. Official adventures in themselves may have considerable variety: customs officers puzzled by pickled snakes; police incensed by the photographs of strategic defenses such as colonial fortresses; poker games with *El Presidente* (travelers always seem able to contrive the most friendly relations with presidents and kings); getting a visa when you have forgotten to bring along three photostatic copies of your marriage certificate.

There remain only the Absurd Adventures (Sixth Degree) and the Unforgettable Thrills (Seventh Degree). In classifying absurd adventures Miss Carmichael always tactfully took the author at his face value. If he reported ordering a glass of milk and getting a fried chicken instead, that was an absurd adventure, introduced to add comic relief. If he called to his gunbearer for the high-powered rifle to shoot a rhinoceros, and was given a shotgun instead, that was sublethal danger (he could always club the rhinoceros with the butt of the shotgun if it attacked). The first example, of course, illustrates the author's struggles to learn the outlandish native language; the second illustrates native stupidity and unreliability in time of stress.

Unforgettable Thrills (Seventh Degree) are always easily recognized: "a breath-taking panorama was suddenly spread before my eyes;" "no white man had ever before heard the clear, lilting song of this beautiful bird;" "there, quivering in the palm of my hand, lay a new butterfly, unknown to the catalogues of science;" "the sheer beauty of that flaming orchid will forever be etched in my memory." It may be significant that Miss Carmichael found a slightly higher frequency of unforgettable thrills among male than among female authors.

When we started to write my book, "Wanderings in the Jungles of South America," we found that Miss Carmichael had mixed up my adventures with those culled from the seventy-six previous volumes of travel and had, unaccountably, thrown them all away. There was enough Whole Cloth around the office, however, so that we were able to proceed with the preparation of the manuscript.

◆

TWENTIETH-CENTURY NAVIGATION

N. C. GERSON

After graduating from the University of Puerto Rico in 1943, Mr. Gerson, whose special field is meteorology, entered the Technical Investigations Section of the U. S. Weather Bureau. He later became associated with the Low Frequency Loran Experimental Project in southwestern Canada. At present he is chief of the Electromagnetic Wave Propagation Section, Watson Laboratories, Red Bank, New Jersey.

> *Now there be some that are very inquisitive to have a way to get the longitude but that is too tedious for seamen since it requireth the deep knowledge of astronomy, wherefore I would not have any man think that the longitude is to be found at sea by any instrument; so let no seamen trouble themselves with any such rule, but (according to their accustomed manner) let them keep a perfect account and reckoning of the way of their ship.*—Nautical works of seventeenth century.

IN ITS general sense, navigation may be defined as the science of location, i.e., the science by means of which one determines his position within a given coordinate system from observations made upon objects either within or without the system. From a knowledge of his position, the navigator of a mobile unit may then set a course to any other object whose coordinates are known. Thus, upon our planet, the science of navigation affords the perception necessary to conduct a vessel or other surface craft from point to point on the earth's surface, and enables the navigator to determine his position at any time within some minimum degree of accuracy.

Although in present usage the connotation of navigation has been expanded to include avigation, nevertheless some important differences between the two sciences should be recognized. Navigation is restricted to surface location; i.e., it pertains basically to a surface craft plowing through the water or moving over land at a low velocity—known to within a fairly close limit. With a vessel, ocean currents, winds, tidal action, etc. may force the ship from its course. However, because most of these deviating factors may be estimated accurately, corrective action which practically eliminates their effect may be taken. Contrast this somewhat simple practice with that normally found in avigation. Avigation, restricted to location above the surface of the earth but in the atmosphere, deals fundamentally with aircraft traveling at a high rate of speed. The velocity, known only approximately, takes place in a supporting medium which usually is in a highly variable and complicated state of motion. As atmospheric winds cannot be plotted in the manner of hydrospheric currents, since the operation occurs in three dimensions instead of two and because aircraft travel rapidly rather than slowly, avigation is generally more difficult and more hazardous than navigation.

Notwithstanding the finer distinctions between navigation and avigation, the present discussion will conform with popular usage and allow the former term to connote also the latter. Thus, the original definition of navigation may be expanded to include location either on or above a set of coordinates from observations made on objects, whose coordinates are known, residing either inside or outside the system or both.

It will be interesting to note the various subdivisions of navigation as the science is applied on the earth's surface. All navigation by observations made upon objects within the coordinate frame of meridians and parallels is lumped together under "geonavigation," whereas when the objects are extraterrestrial, the science is termed "celestial navigation." When it is necessary to resort to extrapolation procedures, the method is termed "dead reckoning." A convenient breakdown is shown in Table 1.

TABLE 1

NAVIGATION

- Geonavigation
  - Pilotage
  - Sonic methods
  - Radiolocation
- Celestial navigation
- Dead reckoning

Pilotage is analogous to motoring or perhaps walking. Both pilot and pedestrian move from origin to destination by noting visually the location of prominent landmarks along the route. Mountains, towns, and rivers are significant in the case of the pilot, and buildings, street signs, and bridges in the case of the stroller. Except under special circumstances, pilotage is invariably employed when possible, even though other navigational aids are present. Obviously, pilotage is impossible in fog, clouds, over oceans, or whenever visible landmarks are absent.

Sonic types of navigation depend upon the reflection of compressional waves traveling either in air or in water. The time required for a whistled signal or other sound to travel from a vessel to a near-by cliff and return is measured. From the known speed of the wave, the distance between the ship and cliff is quickly determined. Similarly, reflections of sonic waves from the ocean floor may be utilized. Directional receivers may be employed similar to electronic DF systems. Present sonic methods are confined to vessels, not aircraft. With the depth contours now appearing on the nautical charts published by our Federal mapping agencies, the navigator, equipped with an echo-sounding device, can use the many submarine features delineated on the charts for position location.

Radiolocation, the third division of geonavigation, is coming into widespread usage. In the early future it may become the principal mode of terrestrial navigation, especially if combined with some form of an automatic dead-reckoning device. Radiolocation employs the radio wave region of the electromagnetic spectrum as an aid in position determination. Although its prewar usage was mainly confined to such devices and systems as radio compasses, radio range stations, absolute altimeters, etc., today it embraces a wide variety of competing electronic orders, most of them hidden under odd or fanciful names. Some of these systems are known as Gee, Shoran, Racon, Consol, Decca, Raydist, Loran, etc. (It should be noted that if instead of employing radiolocation as a category under geonavigation, *electromagnetic location methods* had been used, then pilotage, which employs the visible region of the spectrum with the eye as a receiver, also would have been included within this classification.)

Celestial navigation is self-explanatory. Its practice consists of establishing the position of at least two heavenly bodies upon the celestial sphere from the observer's site. By a transformation of coordinates the observer's position upon the earth is then determined. Celestial navigation is the most complex of the various forms of navigation and is usually employed over the oceans, over cloud banks, over isolated tracts of land, etc., when pilotage or radiolocation is not available or when an independent position check is desired.

Dead reckoning has been the major form of navigation thus far utilized. Essentially it consists of maintaining a record of the velocities and times which have elapsed since leaving the last positively identified position. Dead reckoning, imperative when pilotage is impossible, suffers one great disadvantage. Since errors in it are cumulative, the use of dead reckoning is limited by the unknown factors. An unknown wind velocity effective on a long course, for example, may carry a pilot far from his final destination. Between position determinations obtained by means of either geonavigation or celestial navigation dead reckoning, of course, is used.

With the exception of radiolocation, one major interfering factor is common to all methods of navigation employed below an altitude of about 25 km—weather. Rain, snow, fog, dust, or other meteorological conditions attended by poor horizontal and/or vertical visibility may prohibit the use of pilotage or celestial navigation. Further, whenever unknown or inaccurately prognosticated winds are present, the extrapolation procedure employed in dead reckoning is subject to serious and sometimes fatal error.

Radiolocation methods generally are unique in that they operate, for the most part, independently of the weather. Sonic systems are also independent of weather, but their use is confined to very small distances of the order of miles. In a relatively few instances, however, the utilization of radiolocation methods is curtailed by the elements. These cases are caused by meteorological situations which favor precipitation static or corona discharge because of snow or dust particles; mechanical failure of radio equipment by violent buffeting of the aircraft in severe turbulence or pounding of the vessel in heavy seas; lack of signals through destruction of ground stations by hurricanes, typhoons, simooms, etc. Other cases not strictly terrestrial in origin are radio fadeouts caused by ionospheric disturbances, which in turn are brought about by sunspot activity. However, the proportion of the time during which radiolocation procedures are nullified by vagaries of the weather is but a tiny fraction of that when other navigational methods are rendered inoperative for the same reason.

Before leaving the field of general navigation, it would be well to emphasize one requirement basic to all positional or navigational methods. This basic requirement stems from geometry: one observation establishes a given constant coordinate which the navigator terms a line of position. A second observation defines a second line of position. Obviously, the intersection of two lines of positions fixes the navigator's position precisely. In some instances each line of position may be a great circle; however, no ambiguity is caused by the second nadir intersection since the navigator in general knows the area in which he is located. Position may be specified by latitude and longitude (x, y), by distance and bearing (r, θ), or, in the case of dead reckoning, by velocity and time (v, t).

Under the tremendous impetus given electronics by World War II, several original electronic navigational systems have been evolved during the past few years. Some of the systems thus developed utilized radar techniques such as pulsing and high-precision timing, and others employed only simple continuous wave transmissions. Although it is not the intent to describe fully all radiolocation systems, it will undoubtedly prove illuminating to note briefly the distinction between the major radiolocation classes. A radiolocation system may be classified generally as one of three fundamental types: range, azimuthal, or hyperbolic. A fourth type, composite, consists of any twofold combination of the previous three (Figs. 1, 2, 3).

The range type of electronic navigational system is one of the simplest types proposed. It relies on the same fundamental principle as that underlying radar: if the velocity of electromagnetic propagation is constant, the distance or range between two points is proportional to the time required for a radio wave to make a round trip between these points. Even if the coordinates of either the probing station or the remote point were known, this procedure establishes only the distance between them. In general, the navigator then knows that he is located somewhere on the surface of a sphere whose radius, measurably changing in a high-speed aircraft, is known. The intersection of this sphere with the surface of the earth provides somewhat concentric lines of position, but when these are drawn upon a map, their actual shape depends upon the map projection employed. The surface navigator, however, by maintaining a constant time delay on his receiving instrument could navigate along any desired "circular" line of position.

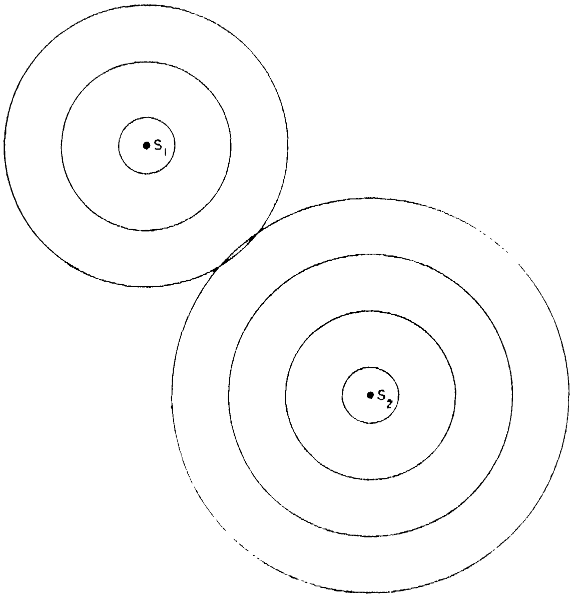


Fig. 1. Concentric circles about each range station, S_1 and S_2, indicate isopleths of constant distance between the observer and the range station. (Not all isopleths have been drawn.)

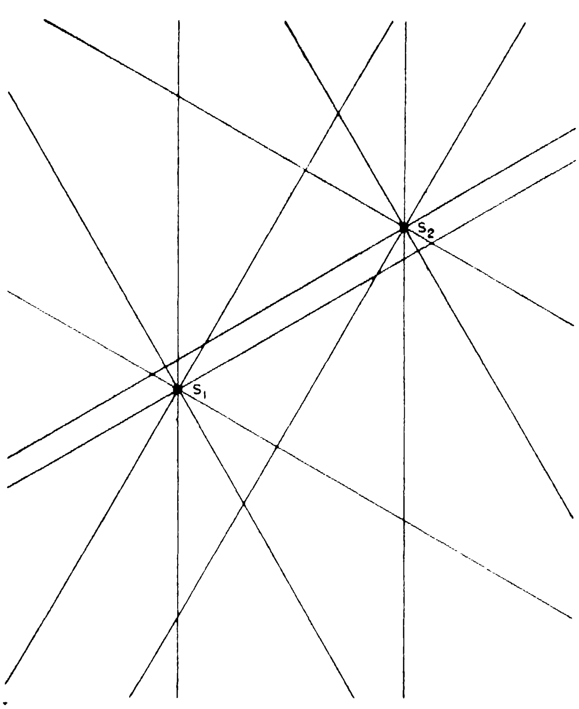


Fig. 2. Radial lines emanating from the azimuthal type navigational transmitters indicate isopleths of constant bearing (on a plane earth). Although the stations S_1 and S_2 are shown as a point, each may include several antennas usually separated by a small number of wave lengths.

In some systems, such as Shoran and Oboe, the mobile craft emits the pulse and receives its reflection. An improved method would allow some means for distinguishing, from among the multitude of reflections produced by prominent objects in the path of the radiation, one particular echo from a reflecting point whose coordinates are known. This method, employing interrogators, solves the problem by installing responder-beacons, or Racons, at desired specific ground locations. Usually the range type of navigational system is restricted to a relatively small area on the surface of the earth by the limited power of the mobile transmitter.

Azimuthal navigational systems are among the oldest radiolocation aids known and are quite familiar through the widespread use of one of the methods, the typical direction-finding system. DF systems are based on the fact that the electromagnetic energy received in certain classes of antennas, square or circular loop antennas for ex-

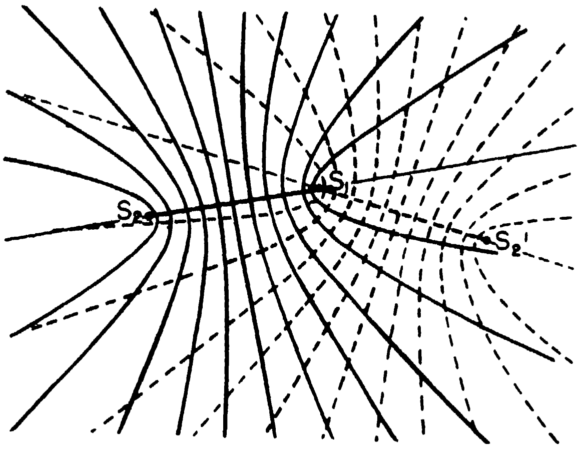


Fig. 3. "Hyperbolas" indicate isopleths of constant time difference between the time of arrival of signals from station pairs S_1 and S_2 and S_1 and S_2'.

ample, is a function of the mutual orientation of the radio wave front and the antenna. By rotating the antenna, the received signal strength varies correspondingly and thus provides a means of determining the angle between the incoming wave from the distant transmitter and the loop antenna. When a radio compass is available the preceding information may be manipulated to give the angle between the incoming wave and geographical north (after corrections have been applied).

Navigation by pure DF methods, however, may be deceptious, particularly at great distances from the transmitter. The deceptiveness arises from two facts: the meridians are not parallel but convergent, especially at high latitudes; and the great circle path—followed by the radiated signal from the transmitter—is not coincident with the line of position for a given DF bearing. At distances within about 150 nautical miles the great circle path and the line of position for a given bearing are, for practical purposes, closely identical (Fig. 4). For utmost accuracy, corrections must be applied for the convergence of the meridians and for the difference between the magnetic variation at the craft and the radio station. In following a given DF bearing, a navigator will describe a loxodromic curve rather than a great circle path.

Under the generic term of azimuthal systems, there may be classed a subheading called directional azimuthal systems. These more recent navigational networks require an electrical phase or amplitude measurement, resolved by visual or aural or possibly even metering methods, which is essentially converted into a bearing, or radial line of position. When the transmitter is a fixed ground station, the lines of position in the ideal case appear as great circles crossing at the station. With long wave lengths, the directional effects are obtained by the use of two or more antennas, which from great distances appear as a point, separated from a quarter to several wave lengths. At these low frequencies the resulting radiation patterns indicating relative signal strength may appear as lemniscates, cardioids, or 3- or 4-leaved roses, depending upon the antenna spacing.

In azimuthal systems, directional patterns at low frequencies are not produced by physically moving or rotating large cumbersome antennas, but by electronically switching to different pairs of fixed antennas, or by changing their electrical excitation. The transmitter usually emits at least two types of signals, which correspond to two different azimuthal directional patterns. Thus, regardless of the absolute intensity of the two signals at a given time and place, their relative strength is constant and is dependent solely upon the bearing of the observer with respect to the transmitting stations.

Perhaps the most familiar example of an azimuthal system of this type is the common *A* (•–) *N* (–•) network (Fig. 5) covering the airways of the United States, Canada, and other countries. This system is received aurally by means of a radio receiver. The installation suffers the disadvantage of being limited to four directions. (Under special circumstances sporadic, unplotted, or undesired courses arise which may result in disaster.)

Another example, which, however, is not restricted to only four directions, is the Consol system. This and similar directional azimuthal systems do not provide complete 360° horizontal coverage about the antenna array, inasmuch as in limited sectors (usually in the vicinity of the base-

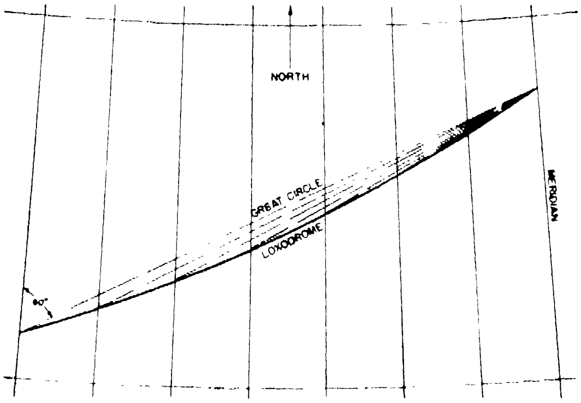


Fig. 4. The spacing between meridians is 2° of longitude. Even at the comparatively small distance portrayed above, appreciable deviations between the DF bearing and the rhumb line are evident. At greater distances from the DF station, deviations increase rapidly.

line extension) the error in position determination becomes excessive. At the transmitter a pattern of dots and dashes is radiated through the combined means of special antenna arrangement, sequential transmission of distinct radiation patterns, and uniform phase shifts of the antenna current. To the navigator receiving them, these signals appear as a characteristic pattern of dots, dashes, and equisignal zone (hum), the pattern being a function of the bearing of the navigator with respect to the transmitter. Using as an argument the number of dots and dashes received, the navigator refers to special charts and quickly determines a line of position.

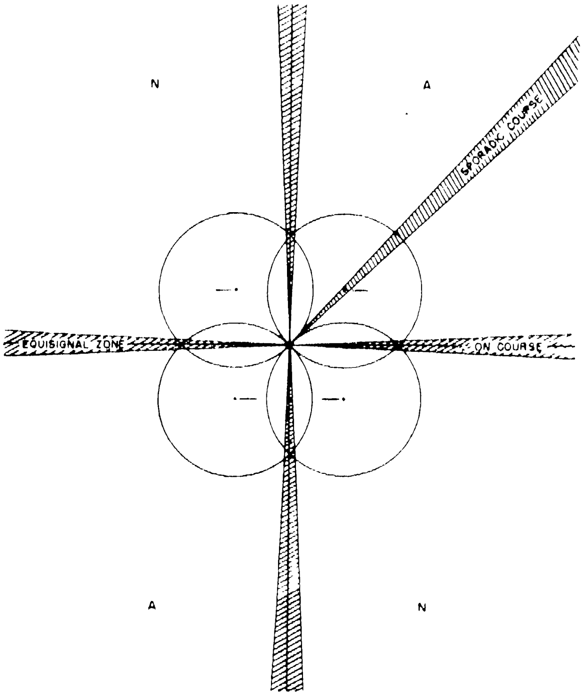


Fig. 5. The four perpendicular courses are typical of those found on *A–N* airways ranges. Sporadic courses may arise because of mountains, peculiarities of the weather, or discontinuities in ground conductivity.

Still other systems provide omnidirectional (360°) coverage about the transmitting station. Two such types are Navaglobe and CAA Omnirange. In the former the bearing indicated at the mobile craft is the bearing of the observer from the ground transmitting station.

In a hyperbolic navigational system the difference in time of arrival of two radio signals is measured. The definition of a line of position is similar to the definition of a plane or spherical hyperbola. A plane hyperbola is the locus of a point which moves in such a fashion that the absolute value of its distance from two fixed points equals a positive constant whose value is always less than the distance between the foci. If the velocity of electromagnetic propagation is assumed constant, a hyperbolic line of position in a plane surface would be defined as an isopleth of constant difference in arrival time of two synchronized signals which had been emitted one at each focus, respectively. In actuality, since the earth is a spheroid and since the radio waves propagate in three dimensions, a line of position in a hyperbolic navigational system lies on the intersection of a hyperbaloid of two nappes with the geoid.

It is thus evident that geometry and the measurement of time difference are the qualifying factors for a hyperbolic navigational system. Either a pulse or phase technique may be employed. With a pulse system, such as Gee or Loran, the difference in arrival time of two pulses provides a line of position directly. In the phase method (Decca or Raydist), a comparison of the relative phase of the two continuous synchronized wave signals emitted, one at each focus, must be obtained. One great advantage of hyperbolic and azimuthal over range system types is that in the former methods no signal need be radiated from the aircraft. A simplified hyperbolic system is portrayed in Figure 6.

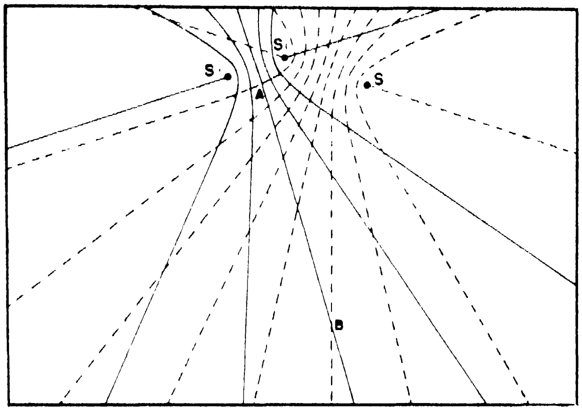


Fig. 6. Absolute accuracy in position determination is greater near point *A*, where the isopleths intersect almost perpendicularly, than near *B*, where the isopleths intersect at a relatively small (acute) angle.

In a composite electronic navigational system, each of the two stations is of a different type. For example, one station may be of the range, the second of the azimuthal, type. The resulting pattern of lines of position may be seen by superposing the appropriate diagrams (Figs. 1, 2, 3). Thus far, however, only the simplest type, range and azimuthal, has been inaugurated.

It is appropriate at this time to mention that an inherent accuracy factor existing in each of the various radiolocation or other types of navigational systems is predetermined by the angle at which two lines of position intersect. Obviously, any system which could provide an orthogonal relationship between the lines of position determined by the navigator would contain the greatest accuracy factor in this respect. Conversely, if the system provided lines of position which crossed at small angles, the accuracy would be materially decreased (Fig. 7). Orthogonal radiolocation systems might be devised by combining range and azimuthal types of stations which are mutually perpendicular. The hyperbolic type would require a family of orthogonal "ellipses"—a requirement difficult of fulfillment. As it has a comparatively small service area, a range type of station has limited usage in a long-range navigational system. Nevertheless, without such stations in a long-range navigational system, the principle, or orthogonality, in the intersection of lines of position is sacrificed.

A brief summary of the various systems and some typical examples of each type are given in Table 2. (The list is not intended to be exhaustive.)

TABLE 2

ELECTRONIC NAVIGATIONAL SYSTEMS

Range type	Oboe Shoran Racon
Azimuthal type	*A–N* Direction finding Sonne Consol Popi Navaglobe Omnirange
Hyperbolic type	Decca Raydist Gee Loran
Composite type	Range and azimuthal (1 station of each type) Range and "hyperbolic" (1 station of each type) Azimuthal and "hyperbolic" (1 station of each type)

Various frequencies could, of course, be employed for any of the types listed.

From the partial register given in Table 2, it is obvious that a fair choice is allowed in the selection of station types and systems needed to fulfill any particular navigational requirement. For each station type employed, however, the navigator must have aboard his craft, and must know how to operate, the proper receiving equipment. (Some systems, such as *A–N* or Consol, require little other than a receiver and earphones; others, such as Loran, require highly specialized apparatus and skill in their use.) In addition, each type of system is usually accompanied by its own series of maps printed with lines of position pertaining to the system. If various adjacent countries installed and operated diverse navigational systems,

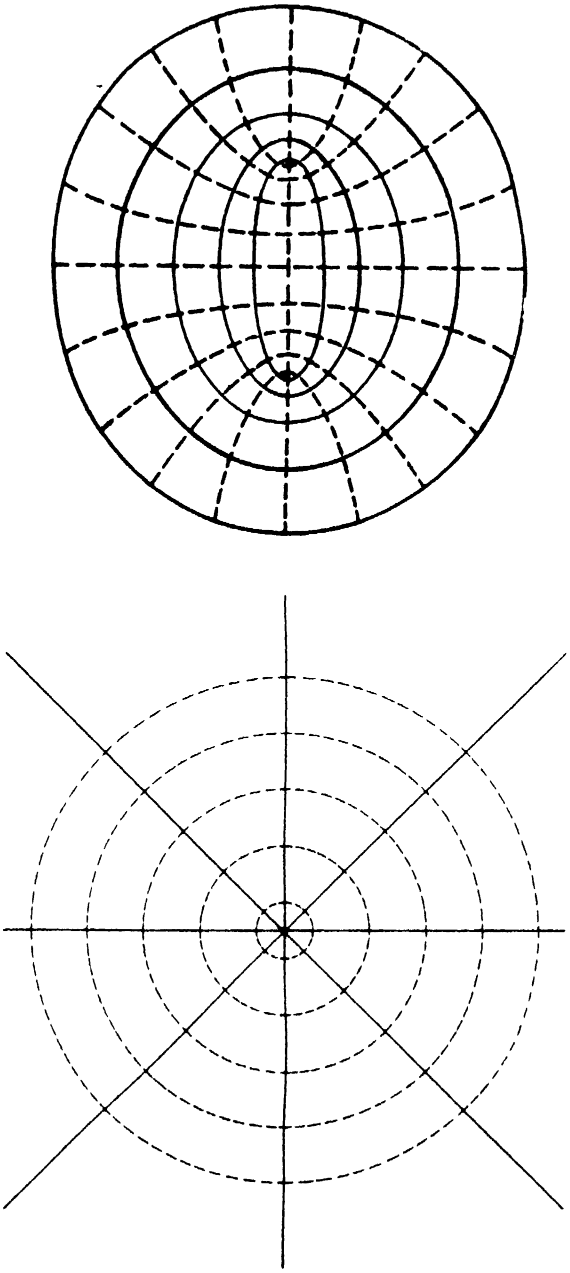


Fig. 7. Ideal orthogonal networks portrayed as objectives of electronic navigational systems. Progress has been made in establishing the range and azimuthal type of system shown in bottom drawing.

long-range vessels or aircraft would be required to invest in excessive installations of equipment, to keep on hand large numbers of charts, maps, and books of instruction, and to train their navigators in the use of a wide variety of equipment, all to accomplish a similar purpose. Obviously, a standardization of navigational systems was necessary to avoid confusion, large investments, and displacement of pay loads.

To effect such a standardization, delegates from nearly two thirds of the member countries of the then Provisional International Civil Aviation Organization (PICAO), the society now known as the International Civil Aviation Organization, (ICAO), met in Montreal during October and November 1946. In addition to other recommendations, the PICAO Conference recognized the value of standard- or medium-frequency Loran as a long-range navigational system. Standard Loran already was providing adequate coverage over several important oceanic regions, and it was deemed of sufficient worth to warrant further installations as the demands arose. However, notwithstanding its many favorable aspects, standard Loran had two serious deficiencies: it would not provide a coverage area over land because of the large signal attenuation at its frequency, and it would not provide complete transoceanic coverage within its accurate but somewhat limited ground-wave service area. The same system operated on a frequency about one tenth that employed in standard Loran seemed to overcome both objections. Low-frequency Loran thus appeared to offer the greatest promise of meeting the desiderata of a long-distance navigational system. It also met the problem of instrumentation by not requiring completely new equipment, but merely a small frequency converter, which allowed LF Loran signals to be received on standard Loran equipment. Thus, with the converter, both standard and LF Loran signals could be utilized with, in many cases, already existing apparatus. The conferees were sufficiently impressed with the range and accuracy characteristics of LF Loran to recommend tentative installation dates (Table 3). This step is the first one toward a global standardization of long-range electronic navigational systems.

In view of the importance of standard and LF Loran as geonavigational methods of the immediate future, these systems will be described in some detail. Operationally, both are very similar except for the additional advantages of LF Loran caused by the enhanced propagation characteristics at the lower frequencies.

"Loran" is a word coined from the initial letters of its definitive phrase: LOng RAnge Navigation. The basic proposal for Loran was devised in 1940, and in 1942 the first fixed stations were placed in operation along various coastal regions. (Simultaneously with the development of Loran, the Telecommunications Research Establishment, of England, developed a similar system, Gee. In 1942 information between Radiation Laboratory and Telecommunications Research Establishment was exchanged, and research on both systems continued. Gee was primarily intended to be employed by high-altitude aircraft for a comparatively short range.)

The operational frequency of standard Loran lies in the vicinity of 2,000 kc/s, and it may be employed by both aircraft and ocean-going vessels. At these frequencies, radio waves propagate around the curvature of the earth for fairly large distances. Although the general principles of "hyperbolic" navigation have already been presented, a Loran system in more detail is shown in Figure 8. At some point *P* a navigator measures the difference between the arrival time of two pulses, one radiated from the system master station S_1 and the second radiated from the system slave station S_2. A similar time difference is established for the second system pairs S_1' and S_2'. Each time difference corresponds to a line of position for the particular station pair involved. In practice, master stations for adjacent Loran transmitter pairs are

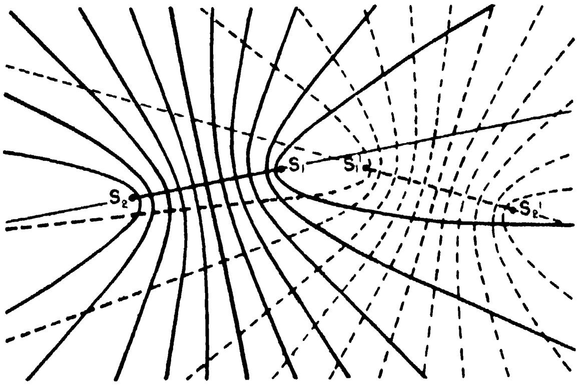


Fig. 8. The master station transmitters are indicated by S_1 and S_1', and their associated slave transmitters by S_2 and S_2', respectively. (Simplified plane Loran system.)

TABLE 3

Region	Tentative Installation of LF Loran
North Atlantic Ocean	January 1949
South Atlantic Ocean	January 1951
Southeast Asia	January 1951
African continent	January 1951
Australasia	January 1951
Pacific Ocean	January 1951

usually coincident; thus, S_1 and S_1' would be combined and the station termed a double master.

Instead of spending considerable time in making stellar or solar observations, and performing intricate calculations, the navigator now makes two simple measurements of time difference. By noting the intersection of two isopleths bearing these values on a map, the navigator locates himself. Some means of distinguishing the signals from the above two station pairs must be given the navigator. The identification may be easily accomplished by allowing one pair of stations to transmit pulses at a rate of perhaps 25 pulses per second, whereas the other pair transmits at a slightly different rate, such as 25 and a fraction pulses per second. A means is also provided to warn the navigator when difficulties at either transmitting station prevent the accurate transmission of pulses.

If pulses were emitted by each station simultaneously, the system would contain an ambiguity because of the symmetrical spacing of equal-valued lines of position on either side of the baseline perpendicular bisector. To remove this uncertainty, the slave station radiates its radio pulse, identical in wave form with the master pulse, at a constant delay period after receiving the master station pulse (Fig. 9). The constant delay is equal to 22,000 microseconds, plus the time of transit of a radio wave between the two stations in microseconds. By an electronic arrangement in the receiving equipment, 20,000 microseconds are automatically subtracted from the navigator's observation. The remaining 2,000 microseconds and the transit time between the two stations are absorbed in the method of labeling the isopleths of constant time difference on the Loran navigational chart. This chart has, in addition to ordinary latitudes and longitude lines, topographic features, lakes, rivers, etc., Loran lines of position printed in different colors for every station pair.

At the receiver, the pulses from each station are rectified and displayed upon the screen of a cathode-ray oscilloscope. By introducing a time difference equal in magnitude to that existing between the pulses when they impinge upon the antenna, both master and slave pulses may be superposed. The introduced time difference may be precisely determined by radar timing techniques incorporated in the equipment.

The accuracy inherent in any navigational system is a major consideration in its ultimate adoption. In Loran, the accuracy is not constant, but varies over the service area. In the most favorable regions, along the base line where the lines of positions converge, an accuracy to within a few tenths of a nautical mile or less is possible. As distance perpendicular to the base line is increased, the lines of position become increasingly divergent, thus lessening accuracy. Also, in the region of the "degenerate hyperbola," i.e., along the baseline extensions, accuracy is so poor, because of the wide separation between lines of position, that normally this region is not used for position determination.

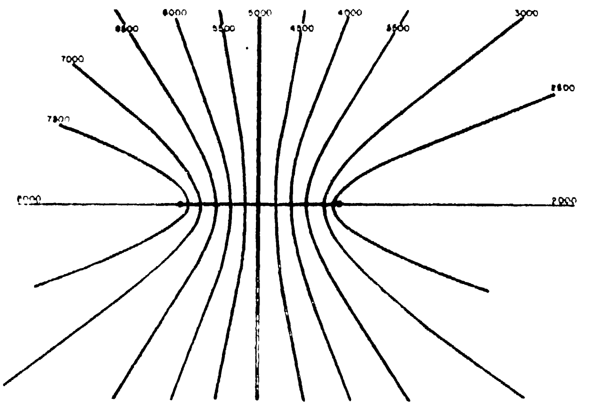


Fig. 9. By inserting a residual coding delay of 2,000 microseconds in the transmissions from the slave station, the ambiguity in position determination that would otherwise exist is entirely eliminated. (Plane Loran.)

A second factor affecting system accuracy is the distance between the master and slave transmitting stations. If the base line is doubled, the error of position determination at any particular point under identical conditions is approximately halved. Still a third factor is the principle of orthogonality, which with Loran chains cannot be maintained. By a judicious choice of station location, it is possible to have the lines of position from two adjacent Loran systems closely perpendicular in some small, particular area. In general, however, especially at outlying distances from the stations, the angle of intersection of the lines of position will be small. Thus it is evident that the accuracy within a Loran service area is intimately associated with the navigator's position with respect to both transmitters. A rough estimate of accuracy is contained in the general empirical rule that the accuracy of position determination is about 1 percent of the mean distance from the transmitters. If the navigator is 1,000 nautical miles from the stations, he is located within about 10 nautical miles of his determined location.

The coverage of standard Loran over sea is about 600 nautical miles during daylight and about 500 nautical miles (but variable) during darkness. The lower coverage area at night is principally caused by the increased noise, which is of a greater relative intensity because of the enhanced propagation of electromagnetic waves. The decreased signal-to-noise ratio correspondingly decreases or eliminates the possibility of obtaining an accurate Loran observation. The smaller coverage area at night may be almost quadrupled by employing pulses which have been refracted from the ionosphere. In this procedure, a sky wave is employed which leaves the earth at a low angle at the transmitter site, strikes the E layer, and is refracted back to earth. This pulse is known as the first-hop E. By employing first-hop E pulses the coverage area during night is increased to about 1,300 nautical miles from the transmitter. The additional transit time required by the signal is taken into consideration by a series of correction tables. Generally, little confusion exists in distinguishing between first-hop E and ground-wave pulses. Since the height of the E layer is not constant, the resultant accuracy is decreased somewhat. The final results are still satisfactory, however, and during the war some blind bombing was effected by using sky waves. When master and slave stations employ sky waves for purposes of synchronizing their transmissions, the system is known as Sky-Wave Synchronized, or SS Loran.

The simplicity and many advantages of standard Loran led to its installation over large coastal regions of the globe. Its use, however, is distinctly restricted to oceanic or other water areas. It does not provide satisfactory navigational service over land masses. In this respect it is inferior to the Gee system. Another disadvantage, or perhaps disappointment, in standard Loran is its inability to provide a range of at least 1,000 nautical miles under all conditions.

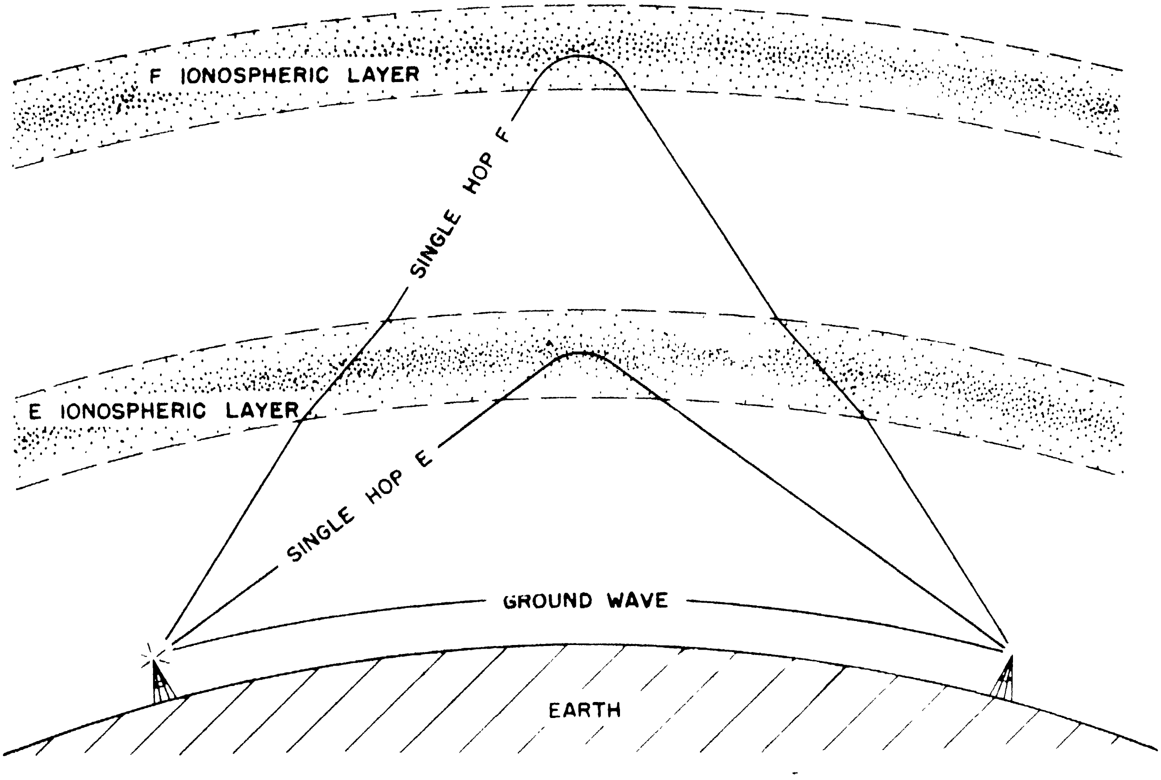


Fig. 10. Because the ground wave attenuates much more rapidly than the sky wave, the latter may still be employed at distances where the former is of such small intensity that it cannot be utilized. Either E- or F-layer reflections may be obtained, depending upon the radio frequencies transmitted.

The greatest factor preventing increased coverage for long distances over both land and sea was the carrier frequency being employed. Since absorption and reflection phenomena, ground-wave range, etc. are frequency-dependent, frequency considerations were re-examined and a lower one recommended. When a carrier frequency about one tenth that utilized in standard Loran was used, both overland coverage and increased ground-wave range were obtained. In addition, several other advantages were apparent. Because of the increased range, the ground transmitters could be more widely separated. Thus increased accuracy over the service area was obtained. Also a not-inconsiderable monetary saving resulted because of the fewer number of ground stations required to cover a given area. Because of instrumental difficulties, accuracies with LF Loran are not as good as those obtained in standard Loran; nevertheless, they are comparable. One result of the theoretical investigations was that if four LF Loran transmitters were located at the corners of a square 1,000 nautical miles on a side, a root mean square distance error of 5 nautical miles or less was obtainable over a service area of close to 3 and 1.7 million square nautical miles by day and night, respectively. Basically, the operational principles and techniques of LF and standard Loran are similar. With LF Loran many difficulties of technical character made their appearance, however. One such minor difficulty may be mentioned—a quarter wave-length antenna is about 1,300 feet in height. At the present time this antenna is a thin copper-clad wire supported by a barrage balloon.

It should be emphasized that Loran and other radiolocation methods are confined to terrestrial navigation. With the advent of rockets and, in the near future, interplanetary flights, these systems would be of negligible value in comparison to celestial navigation methods. As long as speeds of less than 150 nautical miles per second occur, relativistic effects in time of transmission may be neglected. One disadvantage of all Loran systems is their unsuitability as homing systems unless a line of position passes through both the navigator's position and his objective.

* * *

TIME

If time were but a constant pendulum
Swinging between the boundaries of day
And night, its iron regularity
Would drive fear's midnight messengers away.

But time's a seesaw. With a masked unknown
For fellow-passenger, who can foresee
Whether he'll tip the balance toward the sun
Or the cold earth's rock-strewn topography?

FRANCIS BARRY

THE EVOLUTION OF TERRESTRIAL LIFE AS SEEN BY A BIOCHEMIST

ADDISON GULICK

Born in Osaka, Japan, of missionary parents, Dr. Gulick received his Ph.D. in 1910 at Wurzburg. He taught briefly at the Universities of Toronto and Minnesota; since 1912 he has had charge of physiological chemistry at the medical school of the University of Missouri, where he is at present chairman of the Department of Biochemistry.

TO THE naive, terrestrial life may seem the least involved and the most natural way in which to live, but from the physiological and biochemical viewpoint, such life is a highly specialized evolutionary adjustment to a difficult situation. Historically, the difficulties have proved to be so severe that sometimes tens of millions of years have been involved in the process of gradually overcoming them.

It is universally agreed that life was originally aquatic, and that the first great expansion of life forms was in the sea or possibly in shallow waters bordering the sea. Nearly all the mineral nutrients of living organisms are extremely water-soluble. That places them among the substances that leach out from the eroding rocks and are thus carried in a dissolved form by the rivers into the ocean. We do not know how long this process has been going on. It must have begun just as soon as the earth stabilized at a temperature that permitted rainfall. Geologically, the evidence would be the formation of sedimentary rocks. The date of the very earliest sedimentary rocks has not been determined and may never be. In one place and another among the very ancient sediments there are scattered conditions that permit an approximate dating of some particular formation. The most ancient sediments that have been determined by way of the so-called radioactive clock technique have been found to date back about 1,750 million years. That does not reveal what older sediments might exist or may once have existed and the records perished.

It was undoubtedly a long time from the first formation of rain water and seas upon the earth before the exceedingly difficult combination of coincidences that brought forth life may finally have occurred. Nearly all the very ancient sediments are by now so heavily altered that there is little in them to show whether life existed at the time of their deposition. The likeliest manner in which such evidence may survive would be through the existence of streaks of graphite caused by the accumulation of some sort of vegetable material. Such evidence can be traced with a fair degree of assurance some two and a half times as far back as the earliest classifiable fossils; that is, to about 1,200 or 1,400 million years ago, when the graphite-rich Grenville sediments of pre-Laurentian age started forming. But that fails to tell us what primitive, slowly evolving forms may have existed at earlier dates. Such deposits of carbon are usually a result of concentrations of plant life in shallow water.

By the time the leaching of the continental soils had been going on for hundreds of millions of years, it would seem obvious that these surface soils, despite many submergences and re-elevations, must have become very seriously depleted. We must further suppose that the river waters originating upon these land masses must also have ceased to contain much soluble material, so that there was already a sharp contrast between the nutrient-poor lands and fresh waters, on the one hand, and, on the other, the perhaps already over-strong brine of the ocean. Under these circumstances probably the earliest parting of the roads into unlike habitats was the division into marine and fresh-water organisms.

We cannot say positively, but only with great probability, that the first colonization of land came from fresh water. This would be the less difficult of the two alternatives because the mineral content of ground water and of fresh surface water are quite similar. Plants must necessarily have preceded animals as colonists on the land, because otherwise animals, completely dependent upon organic foodstuffs, could not have nourished themselves.

Vascular plants. It is not unlikely that the earliest organisms to establish themselves on land may have been microscopic green algae. This has to remain a surmise because plants of this sort have very little power to leave geological evidences of their existence. Presumably following them, but still at a relatively early date, would have been the mosses, an essentially fresh-water stock which became the starting point for the development of types having strands of vascular or woody tissue, thereby forming the first links of the evolutionary chain that consists of higher mosses, ferns, and flowering plants. The earliest good fossils of vascular plants known thus far are credited to the Silurian of Australia—age about 330 million years—but the stock they represent must at that time have been long acclimated to dry land.

This great block of plant life, beginning with mosses and culminating in flowering plants, has considerable solidarity of type and is subject on land to a rather characteristic group of special limitations and potentialities. Their answer to the problem of water shortage was to run rootlets down into the earth. This method of nourishment establishes an essentially one-way flow of water. From the rootlets in contact with ground moisture to the green structures exposed to sunlight and to evaporation, living on a soil with low sodium and low chlorine content, their whole physiology shows adjustment to shortage of these more soluble elements. In fact, most of them have by now evolved to where more than a trace of sodium chloride amounts to a mineral poison. Regulation, through one means or another, of water and mineral intake was necessary, first, because many of the essential elements have been leached out until they are scarce, and, second, because if any form of mineral becomes absorbed copiously the excess can never be disposed of, and so can hardly fail to leave troublesome deposits in the leaf regions, where they are left behind when the water evaporates.

A rooted plant is unable to migrate, so for survival some form of dry dissemination is necessary. Primarily, we observe in mosses and ferns a system of dispersal by dry spores. Much later comes the dissemination of seeds by animal agency or wind, and of pollen by visiting insects.

When dissemination has come to be a dry process it has to be dissociated from fertilization, which in its earlier evolutionary forms is an aquatic process. Mosses and the principal fern forms are still aquatic with reference to reproduction, to the extent that they require at least a drop of water in which the spermatozoid can find the egg cell. It is only in flowering plants that the mechanism known as a pollen tube completely supersedes the original aquatic pattern of their life cycle.

Another adaptation in plants meets the difficulty of dry air and the resulting high rate of evaporation. From the liverwort mosses upward, nearly all plants provide themselves with minute air spaces, enclosed within their tissues, whereby the actual individual living cell is removed from contact with external air and allowed to function in the environment of a water-saturated atmosphere.

Excretion also is a special problem coming from terrestrial life. Aquatic forms, particularly when simply organized, can get along with relatively primitive methods of disposal of the excretion products. On land the nonvolatile excretion materials must usually be especially provided for. The green plants are less troubled by this problem than are the other land organisms, because they are well supplied through sunshine with the wherewithal for reactions that absorb energy. This gives them the power to re-employ almost any form of waste material in biological syntheses. Plants have a notable ability to deposit byproducts of their activity in the vacant cellular cavities or physiologically dead portions of the plant structure. It is hard to tell just what the explanation of some of these deposits may be, whether they are excreted end products from biological processes that have hit some chemical blind alley, or whether such deposits may actually be fulfilling some highly valuable purpose. The pitch in the nonliving portions of a pitch pine tree, for example, may conceivably represent such a "blind alley," but at least equally well it may be a provision that secures against the rotting of the tree trunk. Perhaps the most difficult material of excretory nature for the plants to dispose of would be the mineral material that arrives absorbed along with the necessary water intake. Apparently all adjusted land plants have learned to hold such unuseful mineral intake down to a very low minimum.

One might look upon the higher plants as representing the most perfect and most complete case of biological adjustment to nonaquatic life. One of the evidences in favor of this statement is that no other group of land organisms begins to compare with such plants for ability to live in the absence of sodium chloride, the salt which more than all other salts has been effectively removed from the land areas of the world.

Insects and arachnids. Although it is impossible today to decide what group has the distinction of being the earliest animal type to have made its way out of water onto the land—whether the arachnids, comprised of scorpions, spiders, and their relatives, or the centipede tribe, or the insects, or the latter two groups together by way of a common early land ancestor—the best-documented proof of antiquity among land animals goes to the arachnids. This is because the earliest of them were scorpions, a form that is relatively well adapted to leaving fossil remains. Spiders were a later development, arising by way of a slender-tailed transition form. Scorpions are found on land in the Silurian strata, approximately 330 million years ago. They show obvious relationship to their contemporaries and predecessors, the marine Eurypterids, which were an ancient stock related to the crustaceans, and ancestral to the present Limulus, or horseshoe crab.

When they became terrestrial, the scorpions enclosed the eurypterid book gills behind spiracles, and so produced "book lungs," from which in due course the spiders developed a tracheal system, superficially similar to those of insects. But it is unlikely that this resemblance strikes any deeper, as there is no indication that insect ancestors ever had book gills.

The origin of insects is much more obscure. The winged forms, which can be traced back 250 million years into the time of the coal measures, were clearly descended from wingless terrestrial predecessors, which are thus far first discoverable in the Devonian, aged about 300 million years. The ancient wingless insects were very small, almost microscopic in fact, and of extremely lowly organization. Fortunately, their rather near counterparts are still to be observed in the ground litter of present forests, and the comparative anatomy of their soft parts can be studied. In this way it is possible to establish a probable relationship between them and certain small, primitive kindred of the centipedes. Hence comes the hypothesis that insects and the centipede tribe had a common origin from a very primitive arthropod type that was somewhat collateral to the crustacea. They may even have emerged onto land before becoming bulky enough to require special respiratory organs in either the aquatic or the aerial environment. Respiration by spiracles and tracheae gives the impression of being a joint inheritance of insects and centipedes from a very early common terrestrial ancestor.

Insects, centipedes, and arachnids all have exoskeletons, inherited from their aquatic ancestors. They all have their physiology founded on similar solutions of the problem of air breathing, and of reproduction by eggs under dry conditions. Thus the different groups exemplify in several respects a remarkably parallel or convergent evolution.

Life within an exoskeleton has no great disadvantages for a crustacean that can float quietly in the water, uninfluenced by the distorting effect of gravity during the periodically repeated occasions when for a few hours after its molt it simply has no skeleton. On land it is a different story; an insect cannot afford to grow so large and heavy that its weight will ruin its shape while it is soft. As competition on land increases, natural selection will enforce the limitation on size more rigorously. Thus, all told, modern insects are on the average small organisms. Even in ancient times their maximum observed size was far below the corresponding maxima for vertebrates—a roachlike form with about a 2-foot wing spread and a dragonfly with a 29-inch spread being the largest.

The laws of physics that affect a creature as small as the average modern insect are quite different from those that rule our own daily lives. The muscular power to jump or pull is measurable by the cross-sectional area of the muscle involved —a two-dimensional quantity—whereas body weight and weights of load carried are three-dimensional. Because of this, an insect with linear dimensions 1/100 of a man may have, in these terms, a muscle strength 1/10,000 of the man's and a body weight 1/1,000,000 that of the man—roughly, a one hundredfold relative mechanical advantage over the man in the ratio of strength to weight. On the other hand, some physical forces that are completely negligible for us may have ominous consequences for the insect. Any small insect's very life is threatened if it has the misfortune to suffer a moistening contact with a good-sized dewdrop, since surface tension is a force that far outclasses his muscle power. Hence their need for various devices to combat moistening with water—special waxy substances on their surfaces, water-repellent hairs, wing-scales, etc.

As natural selection pushes organisms gradually to higher and higher levels of muscular activity, the oxygen needs of their tissues increase in similar proportions. In many modern insects the branching of the tracheal system which supplies them their oxygen has been carried to astonishing extremes, and the ultimate ramifications become tubules only 2–3 μ (thousandths mm) in diameter, which actually penetrate some of the cells that they serve for gas-exchange purposes. These fine-bored air tubes bring up a new and unexpected complication. Their walls are semipermeable membranes, and but for the aid of the osmotic pressure of the surrounding body fluid, the capillary force, which grows progressively stronger as the tubes grow narrower, would suck enough water into the tracheal tubes to waterlog them and completely destroy their function. The smaller the tubes, the higher is the salt content required in the body fluids to prevent this waterlogging. Thus we find widespread among insects a salt content sufficient to give osmotic pressures 50–100 percent above that in mammalian blood. Usually potassium is the predominant kation; among anions, HPO_4 is more prevalent and Cl less prevalent than in the vertebrates. The comparatively smaller proportions of the ingredients of common salt might be considered an indication that during their long stay on land the insects have developed a more complete adaptation to the sodium chloride-impoverished terrestrial chemical environment than any vertebrate has achieved.

The laying of subaerial eggs, and the period of active biochemical metabolism while encased within the closed system of the insect pupa influence very powerfully the problems of the biochemistry of excretion. Aquatic animals, at least if not too large and complex, have only a relatively simple problem with their excretions, one that can be taken care of by providing opportunity for the outward diffusion of the useless end products of oxidative breakdown. On land, an adult animal may have to secrete water that will serve as the medium for the removal of wastes. The embryo inside an eggshell cannot even do this and, in order not to vitiate its available fluids, must find a relatively insoluble chemical form in which the excrement may be deposited. This is believed to explain why, among egg-laying land animals, the typical major form of nitrogenous waste product is almost universally uric acid, a substance involving a much more complex synthetic procedure than urea, but easily excreted as a semisolid porridge, or deposited harmlessly within the egg shell. As far as investigated, the insects illustrate this, as do also the spiders, reptiles, birds, and terrestrial mollusks.

Eggs with impervious shells and dry surroundings make it quite impossible for external insemination to occur, as was so easy in the water. Thus it is necessary for the first true land insects to have come from aquatic forms that had already developed the reflexes of copulation, along with a nervous system adequate to be the seat of such reflexes.

A major means of dispersal among marine forms is provided by tides and other water currents. The first colonists on land must have forefeited all such advantages. Later, this was retrieved again by insects acquiring wings, and by young spiders learning to float themselves on strands of web, let out to the wind. The history of the development of insect wings, the earliest of a series of cases of such evolution in several diverse animal types, is so ancient that its story is a lost chapter of paleontology, and the prospect of its recovery is not very strong. There have been various hypotheses, but little that can be said positively, beyond the fact that the opportunity for starting such a career must, as in all subsequent cases, have resulted from a habit of climbing tall plants or trees. We can no longer tell with certainty whether the primary advantage gained was escape from predators, or greater ability to function as a predator or forager, or to find a mate, or simply a better opportunity for the dispersal of the species. The fact that wings are only present after the last molt suggests the likelihood that for most insects their evolutionary significance is principally for the propagation and dispersal of the species.

Mollusks. The terrestrial mollusks all belong to the gastropods, or snails, a group whose marine history extends through almost the entire 500 million years' duration of the total fossil record. Since the fundamental classification of gastropods is based on the soft parts, and since bewildering similarities of shells often occur in diverse branches of the snail tribe, it is often difficult to appraise the fossil relationships. Forms believed to be land snails occur from the coal measures, 240 or 250 million years ago. Anatomical and physiological characters of their modern representatives make it appear that the derivation of the majority is from fresh-water pond snails. The latter, mostly relatively delicate and hard to preserve, are only known from much later strata, beginning with the Jurassic, less than 150 million years in the past. Cyclophoridae and Helicinidae, groups of land snails whose soft parts show affinity to entirely different branches of fresh- or brackish-water snails, must be credited to independent landward immigrations. The Cyclophoridae can be traced, apparently as land forms, as far as the Cretaceous, 100 million years or a little less, the Helicinidae at least to the earliest Cenozoic, about 60 or 70 million years.

No mollusks are able to sustain activity in a dry atmosphere, because aside from the shell they have no impervious outer covering. They can live only as long as they can maintain a moist skin. Except in rain forests and similar locations they are compelled to retreat into their shells or get under cover of ground litter or the like for the greater part of the daylight hours. At such times the Cyclophoridae and Helicinidae close themselves almost hermetically behind an operculum, such as their marine littoral cousins use. The Clausiliidae of

Europe and Asia retreat behind a close-fitting spring clap valve of shell substance, and the more typical Helices establish a fairly resistant membrane of dried mucin. Among the Endodontidae—more primitive kindred of the Helices—*Pyramidula alternata* impregnates its mucin barricade with a special bitter-tasting, brown-colored secretion that presumably serves as a repellent to marauders. Shell-less forms (garden slugs, etc.) go deep under the ground litter, doubtless depending on their greater facility at burrowing their way down; there, by means of their own slime, they protect themselves from desiccation.

From the limitations of their skin physiology, one might argue that snails have at best only halfway fitted themselves to life on land. Yet it has been found that, like insects and reptiles, the Helices have adapted themselves to the problem of terrestrial egg-laying by having a uric-acid pattern of nitrogen excretion. This should count as testifying to a long evolutionary process that has carried very far the essentials of adjustment to dry land. We should also note that very few animals can make out better in an arid climate than can some species of snails, because such a climate seldom fails to have dew at night.

Other invertebrates. It would be a futile task to try to discuss all the invertebrate types that have had greater or less success upon land. For many the fossil records are simply a hopeless void—such, for example, as the giant earthworms of the circum-Pacific area which, two feet long and half an inch thick, although entirely boneless, have the muscular strength to race through a canebrake like an agile snake. Just one more invertebrate group, the land crabs, seems worth mentioning, because it illustrates the relatively little-used marine-beachcomber road for entry to the land habitat. In various parts of the world the land crabs seem to have followed independently this opportunity for finding a new field for a livelihood. But in no instance have they become entirely independent of the sea water. From the lack of contrary reports, we assume that at least their larval stages still have to be completed in that medium.

Vertebrates. The vertebrates were somewhat tardy arrivals on land as compared to arachnids and insects. Just how they compared with other invertebrates would be more difficult to tell. The beginnings of the landward progress were through fresh-water amphibian life, starting in the Devonian, perhaps 280 or more million years ago, becoming better developed in the Carboniferous period immediately following. If we traced the details of the changes involved, far too much of our report would be mere excerpts from comparative anatomy. But aside from that, there is still something to be said.

For vertebrates there were several physiological difficulties to the completion of the adjustment to dry conditions. Despite their fair size and somewhat elaborated nervous system, they may be supposed to have laid eggs that were outside-inseminated, and they doubtless depended upon aquatic life for the embryonic and larval stages. Their excretory system was presumptively based on urea, as is the case with modern amphibia, making it difficult to incubate eggs anywhere but in water. Modern amphibia have a soft moist skin, which limits their acclimation to land. The same may or may not have been true of the ancestral amphibia. Great dependence on sodium and chlorine, and a need for at least traces of iodine, may to some extent have constituted further difficulties, though the life in fresh water must have provided some training in getting along on a short supply of these substances. The difficulty that mammals experience with these shortages may come partly from their high turnover of materials and hence high losses, necessitated by a high-heat metabolism. It is possible, therefore, that the more sluggish ancestors of the reptiles did not find this problem so troublesome.

On the side of advantages we may list the possession of a swim bladder that can do duty as a lung; the well-developed vertebrate kidney, capable of evolving to carry out its new terrestrial duties, of taking over the whole burden of nitrogen excretion, and of developing control of the osmotic pressure and blood fluid acid-alkali balance in the face of more arduous conditions; the ability to produce keratin, or horny tissue, which the amphibian animals had not entirely lost, and which had prime value as an impervious external integment; and, last, the great mechanical advantage of an internal skeleton capable of growing as the body grows.

If reptilian hard parts can serve as evidence of reptilian physiology, we may infer that near, or shortly before, the start of Permian time, something like 200 million years ago, the difficulties were essentially overcome—dry skin, internal insemination, dry-shelled eggs, uric-acid excretion to conserve egg fluids and body fluids. The geological clock seems to indicate that it took 80 million years to progress from the start of amphibian life to the completed terrestrial form. A major aspect of the physiological change was the enhanced importance of the kidney. A frog or a goldfish has all-over contact with its environment. A frog both breathes and excretes all over, and its lungs and kidneys serve only to make up deficiencies and to carry out those phases of excretion that need to be accurately adjusted, such as the body fluid's acid-base balance. Even a goldfish excretes urea, etc., principally through the gills, and uses its kidneys largely to excrete surplus water after capturing from it all possible sodium chloride. But a lizard or a land tortoise has a watertight sealed exterior, and of necessity what the frog would let diffuse out through the skin must now be placed under control of the kidney.

In a sense, the reptiles completed the physiological processes of adaptation to land, but once this was accomplished the doors were opened for a further set of related lines of advance. The exigencies of physiology within the enclosed egg led to creation of a peculiar set of membranes for the embryo, and this led next to the possibility of intra-uterine development by the altered use of these membranes. This was the line of advance followed by mammals, beginning with the multituberculates, related to marsupials, that came to light first in the Triassic, dating some 170 million years in the past.

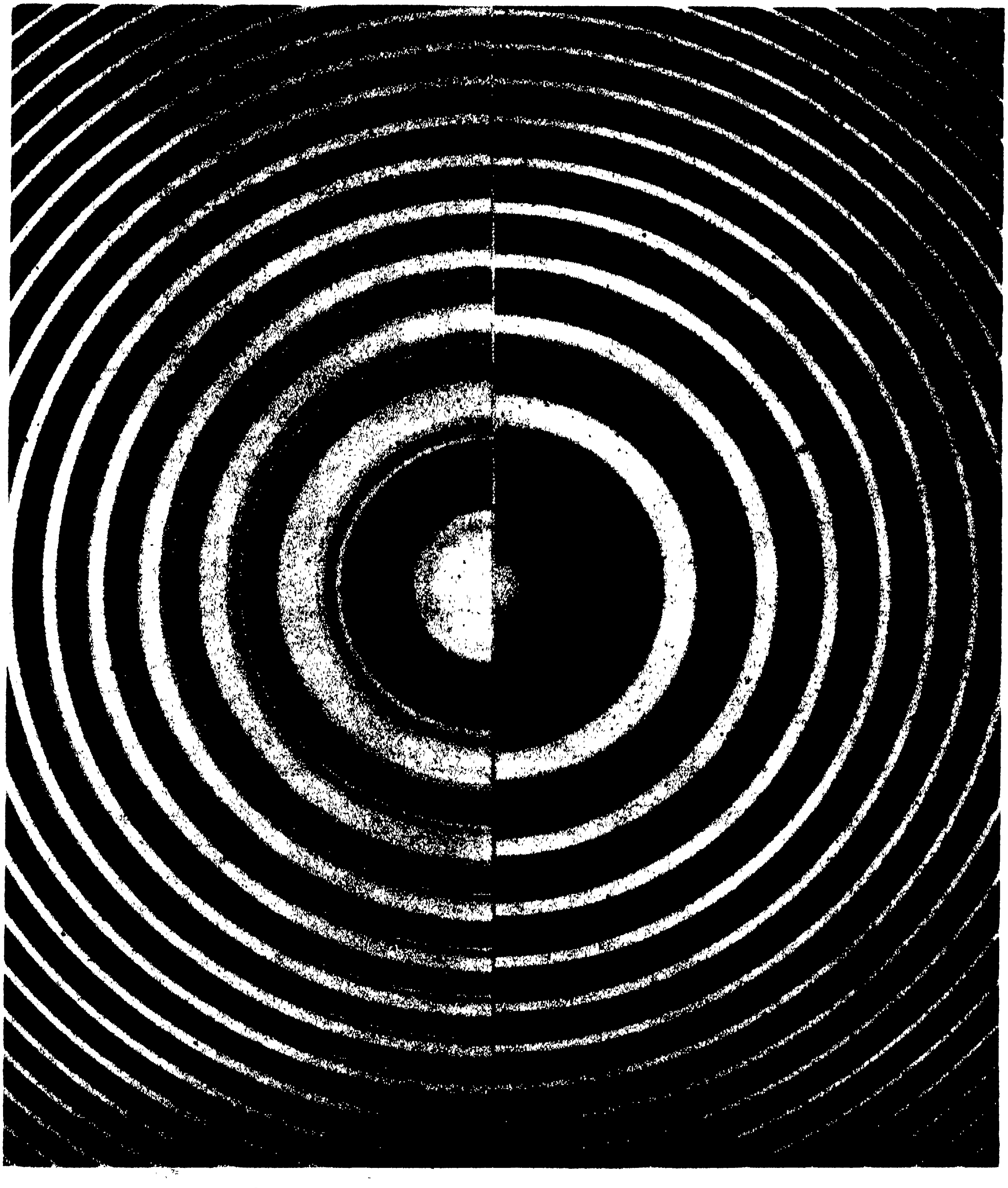
When animals were finally completely freed from a watery environment, with its powerful control over the temperature of bodies immersed in it, and had become at home in a medium that supplied more than tenfold the quantity of oxygen gas per unit volume of the medium, the way was opened thereby for warm-blooded evolution. The first step in this direction had to be the correction of a deep-seated defect of circulation. The relation between vertebrate gills and circulation was such as to provide the body with fully arterial blood. Not so the pulmonary circulation in the amphibia and reptiles, since, being derived as a branch from the visceral circulation, it merely oxygenated a fraction of the blood and mixed it back with the rest of the stream. The evolutionary stages through which the heart and major blood vessels had to go to arrive at the four-chambered heart and the perfected circulation of mammals and birds is a fascinating story, but so familiar to every student of comparative anatomy that we cannot stop for it here. It is hard to tell how far to trust bone structure as revealing physiological organization, but, since in the Triassic the first mammalian skeletons are found and those of birds soon after in the Jurassic (140 or 150 million years ago), it seems easiest to place tentatively the arrival of warm-bloodedness at about the time interval of the Triassic to the Jurassic.

Next there follows, as a close corollary to the high level of metabolism and the high grade of sensory and neuromotor complexity, a renewed evolutionary emphasis on the nervous system—the trend that reaches its culmination in intelligent beings. Thus we see that the biological conquest of those relatively restricted fragments of the earth's surface that lie above water level was a major phase in the story of the evolution of living forms.

⁂

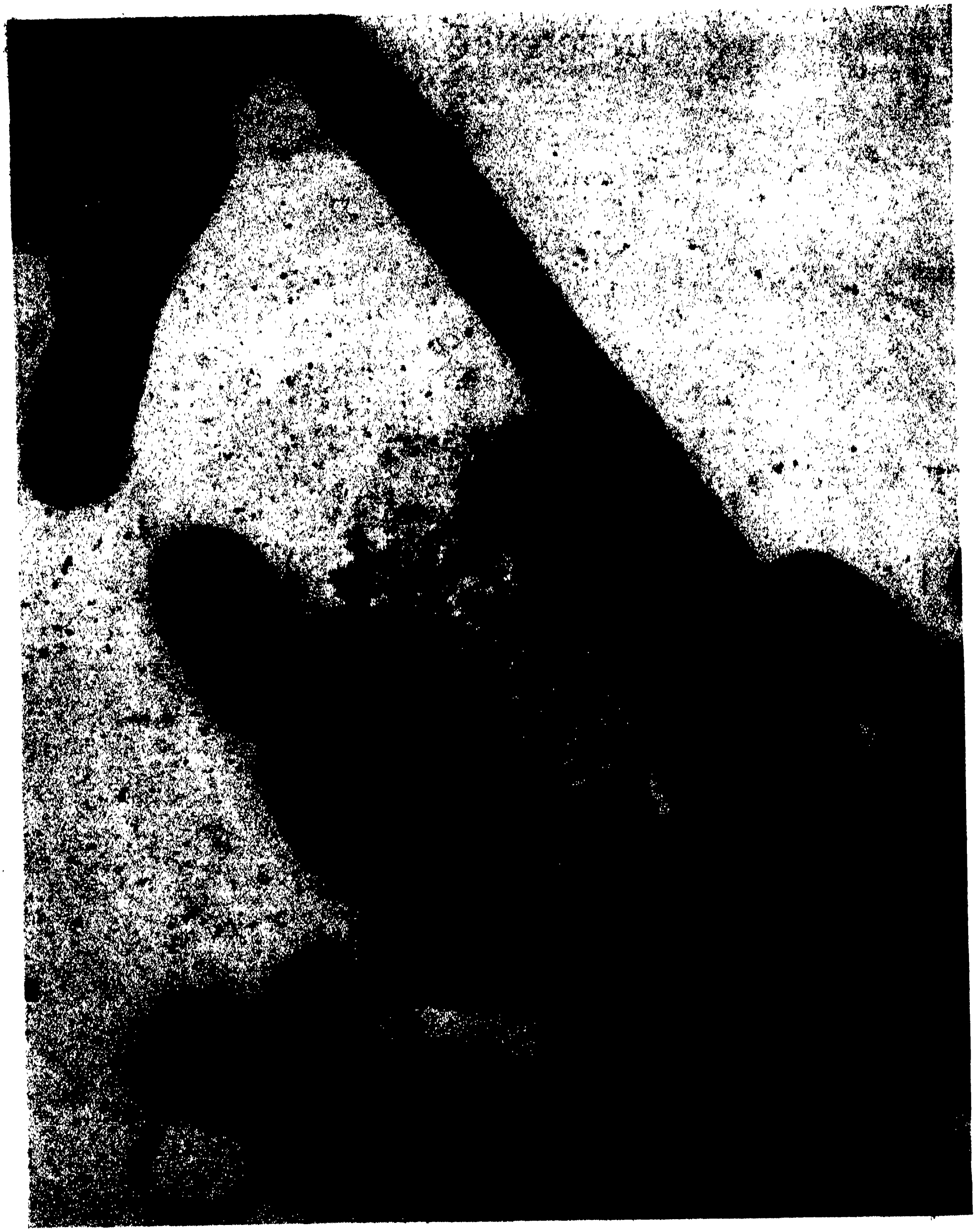
If a single cell, under appropriate conditions, becomes a man in the space of a few years, there can surely be no difficulty in understanding how, under appropriate conditions, a cell may, in the course of untold millions of years, give origin to the human race.—Principles of Biology. Herbert Spencer (1829–1903).

ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON



NATURAL AND ARTIFICIAL MERCURY, 5461 A

CIRCULAR INTERFERENCE FRINGES, FORMED BY FABRY-PEROT INTERFEROMETER ILLUMINATED WITH GREEN LIGHT (5461 A) FROM ELECTRODELESS DISCHARGE MERCURY LAMP: (*left*) 7 ISOTOPES OF NATURAL MERCURY; (*right*) A SINGLE ISOTOPE, MERCURY^{198}, MADE BY TRANSMUTING GOLD. LATTER PROVIDES A STANDARD OF LENGTH SUPERIOR TO ANY IN NATURE, WITH POTENTIAL ACCURACY OF ONE PART IN A BILLION. FIRST PLACE, BLACK-AND-WHITE DIVISION, 1948 PHOTOGRAPHY-IN-SCIENCE SALON. PHOTOGRAPHED BY WILLIAM F. MEGGERS, CHIEF, SPECTROSCOPY SECTION, NATIONAL BUREAU OF STANDARDS.

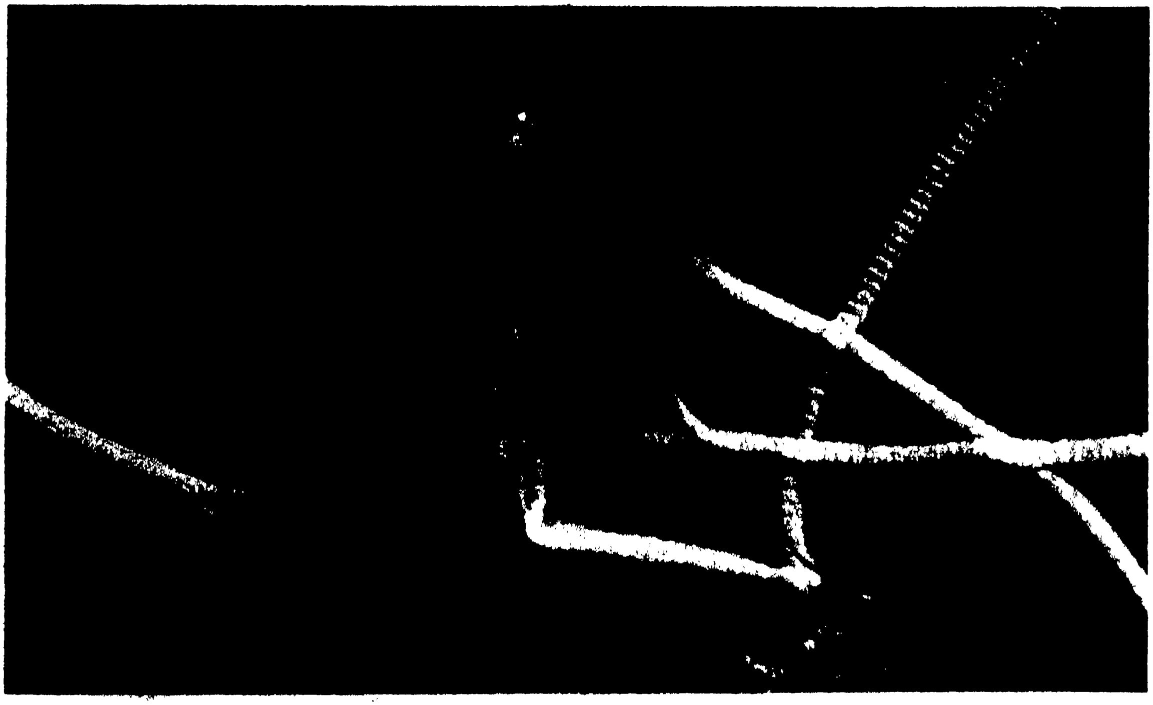


BACTERIOPHAGE LYSIS. MAGNIFICATION, 30,000 ×

ELECTROMICROGRAPH OBTAINED BY JAMES HILLIER, RCA LABORATORIES, PRINCETON, N. J., IN COLLABORATION WITH STUART MUDD AND ANDREW G. SMITH. NOTE COMPLETE ABSENCE OF DIVISION FORMS OF BACTERIOPHAGE IN LYSED CELL, BUT EVIDENCE OF PARTIALLY SYNTHESIZED BACTERIOPHAGE PARTICLES. HONORABLE MENTION, BLACK-AND-WHITE DIVISION.

EXPLOSIVE CHARGE USED IN POULTER METHOD OF SEISMIC EXPLORATION—3 CHARGES

PHOTOGRAPHIC STUDY BY THOMAS C. POULTER, STANFORD RESEARCH INSTITUTE, STANFORD, CALIF., AND WALTER K. LAWTON, SOUTHWEST RESEARCH INSTITUTE, SAN ANTONIO, TEX., WON FIRST PLACE IN THE COLOR DIVISION, 1948 PHOTOGRAPHY-IN-SCIENCE SALON. DR. POULTER ALSO WON FIRST PLACE IN THE COLOR DIVISION LAST YEAR.

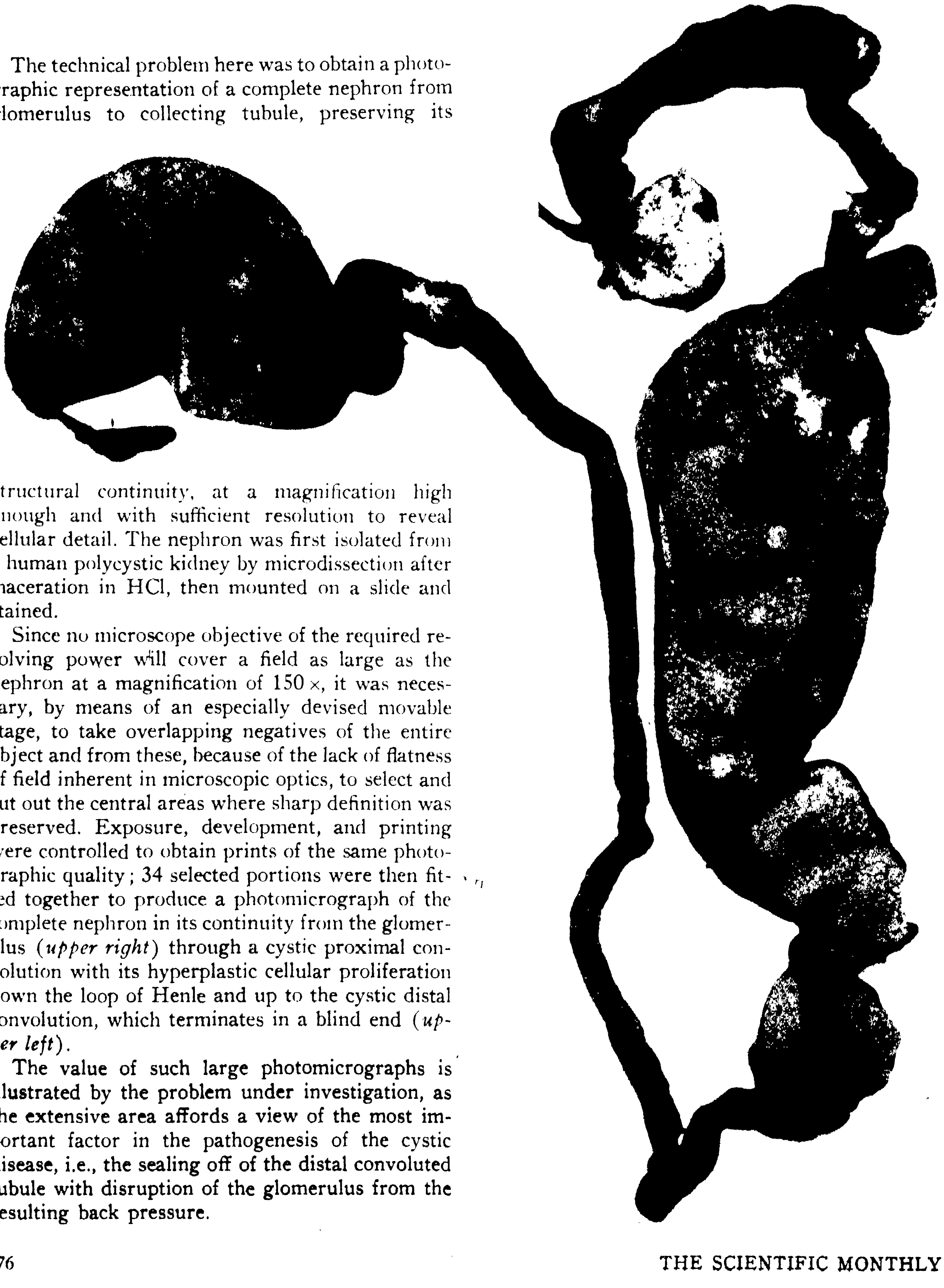


COLLAGEN FIBERS FROM COWHIDE AFTER DISINTEGRATION IN WARM WATER. MAG. 56,000 ×

SHOWING PARTIAL GELATINIZATION. (SHADOWED WITH URANIUM.) ELECTRON MICROGRAPH BY MARTIN C. BOTTY, OF THE RESEARCH LABORATORIES OF THE AMERICAN CYANAMID CO., STAMFORD, CONN.

COMPLETE NEPHRON FROM HUMAN POLYCYSTIC KIDNEY ISOLATED BY MICRODISSECTION

THIS INTERESTING PHOTOMICROGRAPH BY MURIEL C. MACDOWELL, OF THE DEPARTMENT OF PATHOLOGY, LONG ISLAND COLLEGE OF MEDICINE, BROOKLYN, NEW YORK, ALSO WON HONORABLE MENTION IN THE BLACK-AND-WHITE DIVISION.



The technical problem here was to obtain a photographic representation of a complete nephron from glomerulus to collecting tubule, preserving its structural continuity, at a magnification high enough and with sufficient resolution to reveal cellular detail. The nephron was first isolated from a human polycystic kidney by microdissection after maceration in HCl, then mounted on a slide and stained.

Since no microscope objective of the required resolving power will cover a field as large as the nephron at a magnification of 150 ×, it was necessary, by means of an especially devised movable stage, to take overlapping negatives of the entire object and from these, because of the lack of flatness of field inherent in microscopic optics, to select and cut out the central areas where sharp definition was preserved. Exposure, development, and printing were controlled to obtain prints of the same photographic quality; 34 selected portions were then fitted together to produce a photomicrograph of the complete nephron in its continuity from the glomerulus (*upper right*) through a cystic proximal convolution with its hyperplastic cellular proliferation down the loop of Henle and up to the cystic distal convolution, which terminates in a blind end (*upper left*).

The value of such large photomicrographs is illustrated by the problem under investigation, as the extensive area affords a view of the most important factor in the pathogenesis of the cystic disease, i.e., the sealing off of the distal convoluted tubule with disruption of the glomerulus from the resulting back pressure.

A HISTORY OF POPULATION GROWTH IN THE UNITED STATES

P. K. WHELPTON

Associate Director of the Scripps Foundation for Research in Population Problems at Miami University since 1940, Professor Whelpton has taught agriculture in New York high schools, done extension work with the U. S. Department of Agriculture, and taught farm management at Agricultural and Mechanical College of Texas.

THE increase of the population of the United States has been one of the world's outstanding demographic events. When the first census was taken in 1790 the nation had slightly less than 4,000,000 inhabitants; on July 1, 1948, it had more than 146,000,000 (Fig. 1). This represents the addition of more than 141,000,000 persons in a century and a half, or growth to more than thirty-five times the size at the beginning of the period. Few nations of the world have surpassed or even equalled this performance.

For at least 70 years (1790–1860) the population increased at a rate of 32–37 percent a decade. During this period several prominent people, among them Abraham Lincoln, made estimates of the additional growth that was to be expected. Like most of the others, Lincoln assumed the continuation of a relatively high rate of increase and forecast a population of over 250,000,000 in 1930. The 1948 forecast according to his assumptions would be 430,000,000—three times the actual population. Fortunately for our standard of living, the rate of increase declined after 1860. From 1860 to 1890 it was close to 26 percent per decade; during 1890–1900 and 1900–10, about 21 percent; and during 1910–20 and 1920–30, about 15 or 16 percent (Fig. 1). The great depression of the 1930s reduced growth to less than 8 percent from 1930 to 1940, whereas prosperity raised it to nearly 11 percent from 1940 to 1948 (which probably means over 13 percent for the current decade). During the 1950s and 1960s the downward trend in the rate of increase probably will be resumed.

Although the intercensal rate of growth fell after 1860, the number of persons added to the population during the 10 years between censuses increased to the 1920–30 period, when it was more than 17,000,000. The gain from 1930 to 1940 was less than 9,000,000, but that since 1940 exceeded 14,000,000 by July 1, 1948, and probably will exceed 17,000,000 when the 1950 census is taken. Smaller gains are to be expected during the 1950s and 1960s. By 1975 the population is likely to be between 165,000,000 and 170,000,000.

Growth from immigration and natural increase. How much of the large growth in population has come from immigration and how much from natural increase (the excess of births over deaths) is a question about which much has been written The best information comes from a study by Joseph A. Hill, formerly Assistant Director of the Bureau of the Census. His results indicate that the white population of colonial stock increased from approximately 2,300,000 in 1780 to 41,300,000 in 1920—a gain of 39,000,000—and that the immigrants who came after 1780, together with their descendants, amounted to about 53,500,000 in 1920. In other words, approximately 42 percent of the gain of 92,500,000 in the white population between 1780 and 1920 came from the natural increase of the colonial stock, and 58 percent from the immigration which took place during the 140-year period and the excess of births over deaths in this group. It is probable that these proportions have not changed much since 1920.

Official records concerning the amount of immigration date back to 1820 and show that until 1914 the general trend was upward, with large increases occurring during periods of prosperity and smaller decreases during periods of depression. The maximum inward movement took place in the fiscal year 1906–07 when more than 1,335,000 immigrants were admitted, but 1913–14 with over 1,210,000 was a close second. The movement back to the "old country" was relatively large in the latter period, for many foreigners came to work a few years rather than to settle permanently. In consequence, the number arriving exceeded the number departing by about 770,000 during 1913–14, and probably by somewhat more in 1906–07.

Immigration was reduced sharply by World War I, but probably would have rebounded to new heights during the postwar years had it not been checked by the quota system imposed by Congres-

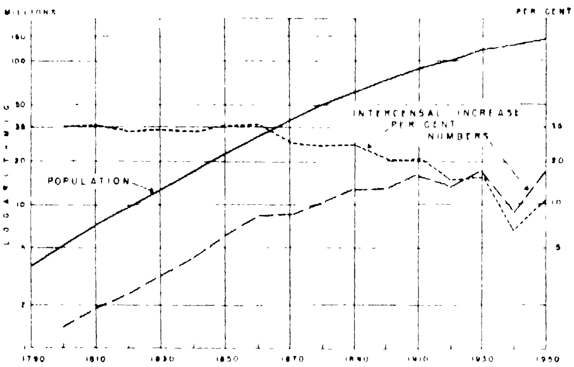


Fig. 1. Total population and intercensal increase, 1790–1950.

sional action. As it was, arrivals exceeded departures by about 3,200,000 in the 10-year period 1920–30. Administrative regulations brought further reductions and, together with the depression, led to a net *outward* movement during 1931–32. Since then the net inward movement has been increasing, and in 1947 immigrants exceeded emigrants by about 215,000. Between 1940 and 1950 the difference probably will be in the neighborhood of 1,500,000.

The net immigration during a decade was largest in comparison to the current population during 1850–60, when the excess of arrivals over departures amounted to about 12 percent of the 1850 population. A secondary high in this ratio (over 10 percent) was reached during 1880–90, but since then the trend has been sharply downward. During the present decade current immigration probably will increase the population by 1–2 percent, as it did in each decade from 1800 to 1830.

Although the excess of immigrants over emigrants has been very large in certain years, the excess of births over deaths probably has been still larger in every year, and certainly has been much larger in most years. Even in 1913, when net immigration amounted to nearly 900,000, births outnumbered deaths by about 1,200,000. Estimates prepared by Whelpton for the 10-year periods from 1800 to 1950 indicate that in most of them more than 70 percent of the current growth of the white population came from natural increase, and that in only two decades (1880–90 and 1900–10) did immigration contribute as much as 40 percent (Fig. 2). Such comparisons do not tell the whole story about the contribution of immigration to population growth, however, for a high proportion of the babies born during 1910–20 (for example) had immigrant parents.

The rate of natural increase (the birth rate minus the death rate) was largest during 1800–10, when it averaged approximately 35 per 1,000. In all except two of the following decades it has become successively smaller, decreasing to about 25 per 1,000 during 1850–60, to 13 per 1,000 during 1900–10, and reaching a low of 7 per 1,000 during 1930–40. The present decade probably will see a recovery to about 12 per 1,000. It is this decline in the rate of natural increase that is primarily responsible for the diminishing rate of population growth since 1860.

THE LONG-TIME DOWNWARD TREND OF FERTILITY

The decline in the rate of natural increase since the early 1800s reflects a more rapid fall in the birth rate than the death rate. The Birth Registration Area did not include all states until 1933, but it is possible to judge what was happening to the birth rate in earlier years by observing the ratio between the number of children and the number of women enumerated in the census. In 1800 there were approximately 1,340 children under five years of age per 1,000 white women aged twenty to forty-four, and in 1810 approximately 1,360. Since 1810 every census except one (that of 1860) has shown a smaller ratio than its predecessor. In 1860 there were 905 children per 1,000 women, a decline of about one third since 1810 (but slightly more than in 1850). In 1900 the ratio was 666, a decline of about one fourth

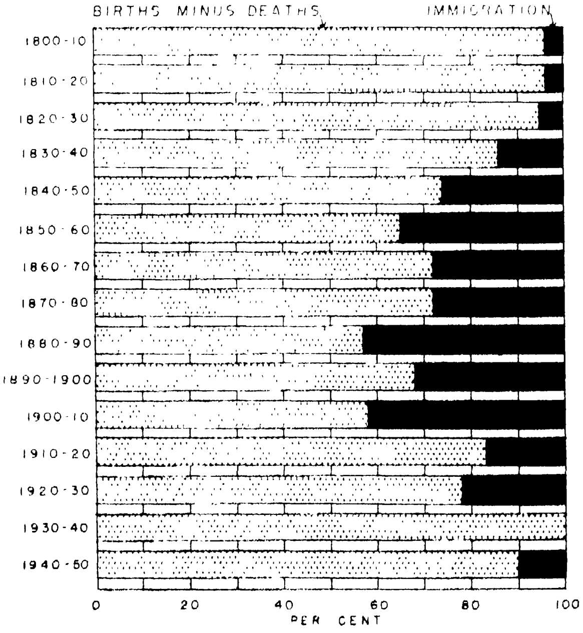


Fig. 2. Intercensal increase of the white population from births minus deaths, and from current immigration, 1800–1950.

since 1860. The last census (1940) showed a ratio of only 419, down more than one third since 1900 (Fig. 3). The next census (1950) will show a ratio substantially higher than that of 1940, and perhaps slightly above the 1930 ratio of 506. Even so, it will be only a little more than one third as large as the 1810 ratio. When an allowance is made for the great reduction which has been achieved in the death rates of small children (which should raise the ratio of children to women), it is evident that the birth rate of white women has dropped at least 65–70 percent since 1810.

Similar information about the Negro population is available from the censuses for 1850 and later years. In 1850 there were approximately 1,090 children under five per 1,000 Negro women aged twenty to forty-four. The 1880 ratio was the same, but since then there has been a decline to about 513 in 1940. The ratio will be higher in 1950 than in 1940, but probably less than 554, the 1930 figure.

Although fertility has declined throughout the United States, the reduction has progressed further in some places than in others. In general the decrease has been largest in the most urban areas and smallest in the most rural areas. Even as early as 1800 the ratio of children to white women had fallen to a much lower figure in New England (about 1,160) than in the East North Center—the area now occupied by Ohio, Indiana, Illinois, Michigan, and Wisconsin (over 1,900). Differences in 1940 may be illustrated by comparing the ratio for white women in New York City (which had fallen to 269) with that for the rural-farm population of North Dakota (which had declined only to 748). The former is barely one third of the latter. This is not an extreme difference, for several cities have a lower ratio than New York, and many rural counties have a higher ratio than the farm population of North Dakota.

Causes of lower fertility. Much has been written during past decades concerning the causes of the decline of the birth rate. Usually the blame has been placed on one or more of the following: a less favorable marriage rate, a rise in the proportion of pregnancies ending with a miscarriage or stillbirth, the more frequent resort to illegal abortion, an increase in low fecundity or sterility, and the more widespread and effective practice of contraception. The first two suggestions are of little or no importance, for the evidence now available indicates that during the past 60 years the marriage rate has become more favorable, and the proportion of pregnancies ending with a miscarriage or stillbirth has decreased. An increase in the practice of illegal abortion probably has been of only minor importance, for the studies that have been made indicate that not more than 5 percent of the pregnancies of urban white women, and a smaller proportion of those of rural white women, have been terminated illegally in recent years. Even if there were no illegal abortions in 1810 and the best current estimates are only half as large as they should be, less than one tenth of the decline in fertility would be accounted for.

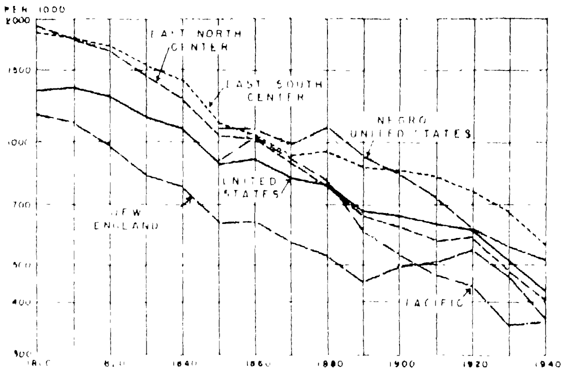


Fig. 3. Children under five per 1,000 white and Negro women aged twenty to forty-four in the United States and selected regions, census years 1800–1940.

The role of low fecundity or sterility is more obscure. There is little reliable information as to how much the birth rate is currently reduced by defects in the reproductive system, but a study of nearly 2,000 native white Protestant couples in Indianapolis indicates that it may approximate 25 percent. Still less is known about the situation in earlier decades. Some gynecologists believe that there has been a notable decrease in the physiological capacity of couples to produce children, and others that the change has been small. But even if no couples were sterile or low in fecundity in 1810 and if these conditions have reduced the birth rate by 25 percent during recent years (which appears to be an extreme combination of assumptions), only about 15 percent of the decline in fertility between 1810 and 1940 would be explained.

Much evidence is available to show that attempts to restrict family size by the practice of contraception have been widespread and effective during recent years, and have lowered the birth rate of large groups of the population by as much as 65–75 percent. That similar attempts were made early in the nineteenth century is certain. That they reduced the birth rate much less at that time than in recent years is proved by the relatively small difference that formerly existed between the actual birth rate and the maximum that is biologically possible. It appears certain, therefore, that the birth rate has dropped 65–70 percent since 1810 primarily because couples have wanted smaller families and have made great progress in learning how to restrict the number of children by practicing contraception.

The recent rise in the birth rate. Although the birth rate declined most of the time from 1810 to 1933 and probably was less than one third as large in the latter year as in the former, it has risen substantially since 1933. In that year there were 18.4 births per 1,000 persons in the population; in 1947 there were 27.2. The increase is more striking when numbers are considered, for the 3,910,000 births of 1947 exceed by nearly 1,600,000 the number in 1933. Does this recent rise mean that the long-time downward trend of fertility has been reversed, or is it a temporary phenomenon resulting from World War II and associated conditions? To answer this question it is helpful to consider separately the rates for first births, second births, third births, and births of other orders per 1,000 women aged fifteen to forty-nine, and to deal only with native white women (who bore over 85 percent of all the children in 1947), because birth certificates are filled out more accurately for these women than for colored women.

From 1920 to 1933 the rate for first births, second births, and births of other orders declined in a rather similar fashion, the reductions varying only 26–40 percent. From 1933 to 1947, however, the trends of these rates have differed widely. At one extreme is the rate for first births, which rose in most years from 1933 to 1942 and reached a record-breaking high of over 40.0 in 1947. This is almost double the low of 22.2 in 1933, and well above the high mark of 32.6 reached after World War I. At the other extreme is the rate for eighth and higher order births, which declined in most years from 1933 to 1942, and in 1947 was about 25 percent smaller than in 1933 and 60 percent below the maximum following World War I (Fig. 4). Rates for intermediate birth orders have followed intermediate trends. Thus, the decline lasted until 1935 for second births, until 1937 for third births, until 1939 for fourth births, and until 1942 for sixth and seventh births. Similarly, the reaction from the lowest rate has been small for sixth and seventh births, and increasingly larger for fifth, fourth, third, and second births.

An analysis of these changes and of changes in other conditions during the period in question suggests certain conclusions. One is that the rates for first and second births have varied from year to year primarily in accordance with prosperity and depression (modified by peace and war), but have shown no definite upward or downward trend. In other words, during the 1920–47 period as a whole, there seems to have been little change in the proportion of couples having no child or one child. A second conclusion is that since 1920 the proportion of couples having eight or more children has been declining rapidly and almost regardless of changes in economic, military, or other conditions. Conclusions regarding families of two to seven children are between these extremes.

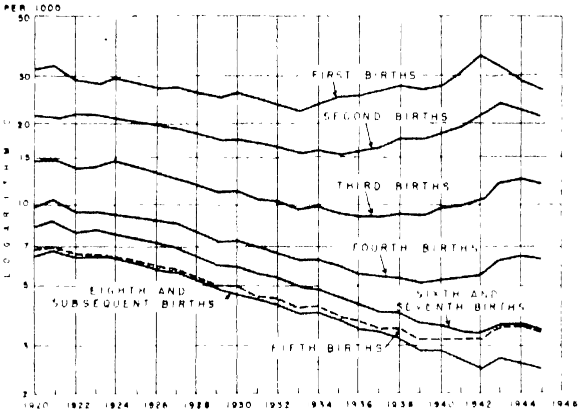


Fig. 4. First births, second births, etc., per 1,000 native white women fifteen to forty-nine years of age, 1920–45.

These specific conclusions have certain corollaries. First, the rise in the birth rate from 1933 to 1943 and from 1944–5 to 1946–7 represents primarily the occurrence of births that were postponed by the depression and by World War II, respectively. Second, when these postponed births have been made up there will be a sharp drop in the birth rate and then a resumption of the long-time downward trend. The decline will be more gradual than formerly, however, because the rates for fifth and higher order births already are so low. Third, the permanent stoppage or reversal of the long-time decline will require that many couples of the type that formerly would have had no child or only one or two children decide to have an additional one or two. Only such a development can offset the further declines which are expected in the rates for fifth and higher order births.

THE DECLINE OF MORTALITY

Although mortality rates have not dropped as much as fertility rates since 1810, striking reductions have been achieved. What happened during the first half of the nineteenth century must be judged by a few studies, for deaths were not registered in most states, the records kept by the remaining states were incomplete, and the census does not provide a measure of mortality as it does of fertility. In 1793 Edward Wigglesworth estimated that in Massachusetts four years earlier there were 27.8 deaths per 1,000 persons. In 1857 E. B. Elliott estimated the 1855 death rate as 21.4. If these estimates are correct, the Massachusetts rate was cut nearly one fourth in 66 years. Little progress was made between 1855 and 1895, the rate being between 18.0 and 22.0 in most of these years, but between 1895 and 1921 the likelihood of dying within a year was cut by more than one third. During subsequent years the Massachusetts rate has remained fairly constant between 11.1 and 13.2. Since 1933 (when the death registration area became nation-wide) the United States rate has been slightly below the rate for Massachusetts, but has been even more stable (between 10.4 and 11.6).

Death rates by age. A better appreciation of the progress made in postponing death may be obtained by considering what has happened at various ages in Massachusetts, one of the states with the best early records. In general, the proportional change in the death rate has varied from a large drop for young children to little if any decline for elderly people. During most years from 1855 to 1900, there were 190–225 deaths per 1,000 babies less than one year old. By 1920 this infant death rate had been reduced to 112; by 1946 it had been cut to 33—less than one sixth of the rate before 1900 (Fig. 5). The biggest single factor has been the control of diarrhea and enteritis, which formerly killed more than 40 babies of each 1,000 born, but which now kill fewer than 3.

Striking progress toward saving life has also been made among children aged one to four. In Massachusetts their death rate was cut from 40 per 1,000 in 1855 to 2 in 1945, a reduction of over 95 percent! After childhood, however, efforts to prevent death have been less successful age by age. At ages ten to fourteen the rate was cut by about 85 percent during the 90-year period (from over 5 to less than 1) ; at ages twenty to twenty-nine the relative drop was equally large (from more than 12 to less than 2) ; at ages forty to forty-nine the reduction was about 60 percent (from 14 to less than 6), but at most older ages the downward trend has been slight, or absent (Fig. 5). Thus at ages sixty to sixty-nine and seventy to seventy-nine the probability of dying during the year was about as great in Massachusetts in 1935 as in 1855.

Mortality rates for the nation as a whole in recent years have been similar to those for Massachusetts at ages over fifty, though slightly higher at younger ages. The latter rates have been reduced substantially since 1935, however, and there are some indications of real progress in postponing deaths among older persons. If conditions should continue as they were in 1945 the average white baby would live to age sixty-five—about twice as long as was the case 150 years ago.

Causes of death. The large reductions in death rates from infancy to middle life are the result of scientific progress, general economic betterment, and expanded public-health programs. Certain contagious or infectious diseases which were important causes of death only 50 years ago have been almost eliminated, and the mortality from others has been greatly reduced. Typhoid fever and diphtheria each killed more than 28 persons annually out of every 100,000 of the population in 1900–02, and measles, scarlet fever, and whooping cough more than 10 each, but in 1945 these five diseases combined took a toll of less than 4 (Fig. 6). The bronchitis death rate has been reduced by a larger absolute amount in the same period (from 40 to 3), and the pneumonia and influenza rate still more (from 190 to 52). The largest reduction of all is in tuberculosis mortality, the rate for this disease having been cut from 186 to 40. A very large reduction has also been achieved in the rate for diarrhea and enteritis, from 122 to 9 deaths per 100,000 persons per year.

In contrast to the foregoing is the relatively little progress made in preventing deaths from certain other causes. The rate for intracranial lesions of vascular origin (popularly called apoplexy) declined only from 106 per 100,000 persons in 1900–02 to 98 in 1945, and that for nephritis from 90 to 67 (Fig. 6). More discouraging is the fact that

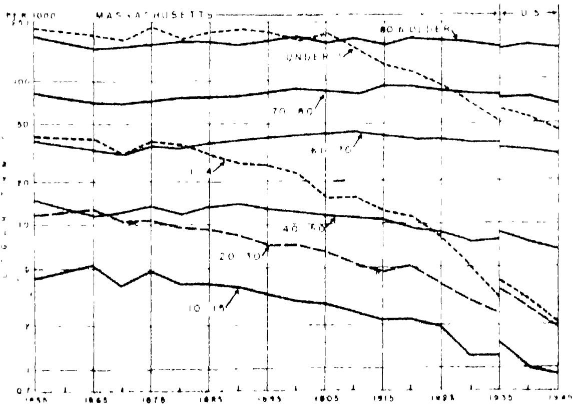


Fig. 5: Deaths per 1,000 persons of specified ages, selected years, Massachusetts, 1855–1935, and United States, 1935–45.

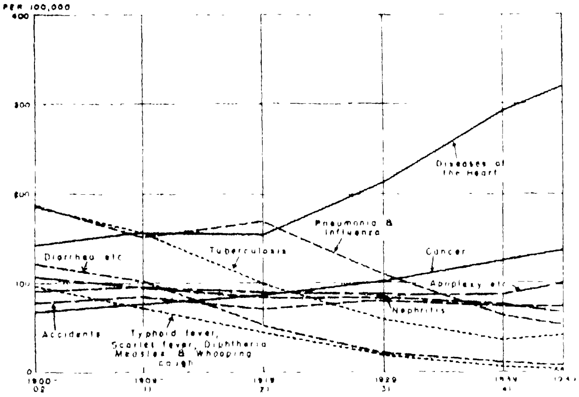


Fig. 6. Deaths from important causes per 100,000 persons in the death registration states, selected years, 1900–02 to 1945.

recent death rates for cancer and diseases of the heart (134 and 322 per 100,000, respectively) are more than twice as large as formerly; each of these is now responsible for many more deaths than any other cause. Part of the greater incidence of cancer and the so-called degenerative diseases is apparent rather than real, however, because diagnosis has become more accurate and because there has been an increase in the proportion of the population in the older age groups where these causes of death are most prevalent. But aside from such factors the increase in the rates for these causes has been sufficiently large to justify the greater attention now given to them in the national health program.

Future trends. Whether the over-all death rate will decline substantially during the next 50 years will depend in part on the continuation of the progress made in controlling such diseases as tuberculosis, influenza, and pneumonia. Here the outlook is favorable, for the more widespread utilization of present knowledge regarding the causes and control of these diseases would lower the number of deaths to a point substantially below the present levels. There is also a reasonable expectation that more effective measures for the control of these diseases will be developed, and that they will be applied to larger sections of the population as public-health programs are intensified and extended.

Although the degenerative diseases have not as yet been brought under control, there is hope for the future. Because certain damaging infectious diseases (e.g., scarlet fever, diphtheria, and typhoid fever) have almost been eliminated, a substantial reduction should occur in the organic impairments and aftereffects caused so commonly by such diseases. As these sequelae are reduced in frequency there should be a reduction in the number of organic breakdowns or a postponement of these breakdowns until later in life. Similar gains should result from the more recent campaign to control venereal disease. For this reason, and because of improved techniques for early diagnosis, there should be some reduction in the mortality from the degenerative diseases even without the discovery of better methods for their prevention or treatment. In view of the great amount of research being done on the causes and control of cancer, it is quite possible that the number of deaths from this disease will be much lower before many years pass.

MIGRATION WITHIN THE UNITED STATES

Americans have long had the reputation of being "on the move" to a greater extent than most people. Since the early 1600s the frontiers have gradually been pushed back, and the settled areas expanded from small communities on the Atlantic coast to all the habitable portions of our 3,000,000 square miles. During most of the time the main movement was westward, in accordance with Horace Greeley's famous advice, "Turn your face to the great West, and there build up a home and fortune."

Interstate migration. The first reliable information regarding the amount of internal migration comes from the census of 1850, and is based on a comparison between the number of persons born in a state and the number (excluding foreigners) living in that state. In 1850 each state from Georgia to Maine (and Kentucky and Tennessee as well) was found to have sent its native sons to other states in greater degree than it had attracted the native sons of other states. In contrast, Florida and each state west of those mentioned had attrated natives from other states in larger numbers than it had sent its own natives to other states. The net loss through out-migration had been especially large for South Carolina, the number of persons living in South Carolina being only slightly more than half as large as the number born there. Vermont, Connecticut, Virginia, and North Carolina also had suffered a net loss of 25 percent or more. California had had the largest gain, for it contained more than ten times as many people as had been born within its boundaries! Ratios of more than 2 to 1 occurred for Michigan, Wisconsin, Iowa, Arkansas, and Texas.

Although a large westward movement continued after 1850, its influence gradually was offset by an increasing movement northward and eastward. By 1940 this had gone so far that the Northeastern states as a group had attracted about as many people born in other regions as they had lost to other regions by the departure of their own natives (Fig. 7). The North Central region had gained somewhat in its exchange with the South and Northeast, but had lost heavily to the West. The South had lost slightly to the Northeast and more heavily to the North Center and West. The West had gained greatly from the North Center, moderately from the South, and slightly from the Northeast. Vermont and Iowa are the only states showing in 1940 a net loss of 20 percent or more through interstate migration, but 15 other states (mostly in the Midwest and Southeast) had lost more than 10 percent. California had benefited the most from migration, for nearly 3,400,000 persons—almost half its population—had been born in other states. Florida, Arizona, Nevada, Oregon, and Washington also owed much of their pre-1940 growth to a net inward movement.

During World War II the migration of the civilian population followed the general pattern just described but took place on a larger scale. Nearly 15,000,000 civilians were classified as migrants during the three years 1942–44, compared with about 14,000,000 during the five years 1935–39 and smaller numbers during preceding periods. Again it was California that attracted outsiders in greatest numbers, with some of its neighbors not far behind. In consequence, California now ranks third in population, having passed Ohio and Illinois since 1940; before 1950 it probably will pass Pennsylvania and rank second.

Migration by color, age, and sex. The white and colored populations appear to have been equally mobile during recent decades, for in 1940 approximately one out of four persons in each group was living outside the state in which he was born. The situation changed temporarily during 1935–39, when the proportion of the population classified as migrating was half again as high among white persons (12.3 percent) as among colored persons (8.5 percent). From 1940 to 1947, however, about one person in five in each group has moved across county lines.

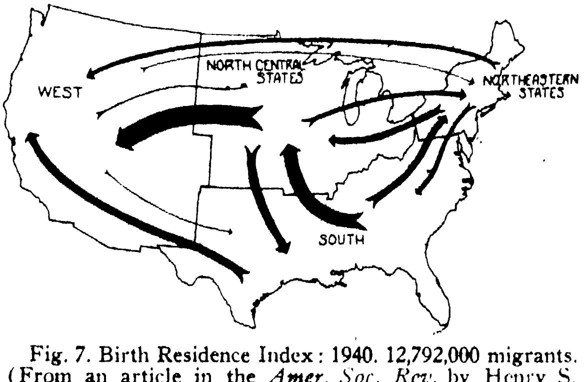


Fig. 7. Birth Residence Index: 1940. 12,792,000 migrants. (From an article in the *Amer. Soc. Rev.* by Henry S. Shryock and Hope T. Eldridge. Used by permission.)

Migration rates were highest for young adults and lowest for elderly persons during 1935–39 and 1940–47, and probably during most other years also. Because an important proportion of the young adults who move have married previously and started their families, the migration rate for children under fourteen has been higher than that for persons middle-aged or older. Youngsters of high-school age (fourteen to seventeen) have been least likely to move among the population under fifty.

Although men are usually thought to be more foot-loose than women, the proportion that makes one or more moves has been about the same for each sex. This is confirmed by a comparison of state of birth and state of residence for 1940, and also by the census data on migration during 1935–39 and 1940–47. If the number of moves could be counted, however, it might be found substantially higher for males than for females.

The rural-urban movement. For many decades a high proportion of the persons migrating within the nation were moving to new land, which they began to farm. During the nineteenth century the situation was completely changed by the industrial development, for it greatly stimulated the growth of cities. In 1790 nearly 95 percent of the population were classified as rural, and only 33,000 people lived in the largest city (New York). By 1850 the rural proportion had dropped below 85 percent, and New Yorkers numbered over 500,000. Since then the urban trend has been accelerated. By 1900 barely 60 percent of the population lived in rural areas, nearly 20 percent were in cities of 100,000 or more, and three cities were over the million mark. By 1940 only 43 percent were rural, 29 percent were in cities of 100,000 or more, and five cities were in the million class (Fig. 8).

The great urban growth came about in important degree through migration from other areas, but the net inward movement prior to 1935 can only be estimated roughly for cities with the most reliable birth statistics. Net migration to New York City probably accounted for 70–80 percent of its population increase from 1900 to 1910, and the excess of births over deaths for only 20–30 percent. (In this case, however, the majority of the migrants came from abroad rather than from other parts of the United States.) The proportions

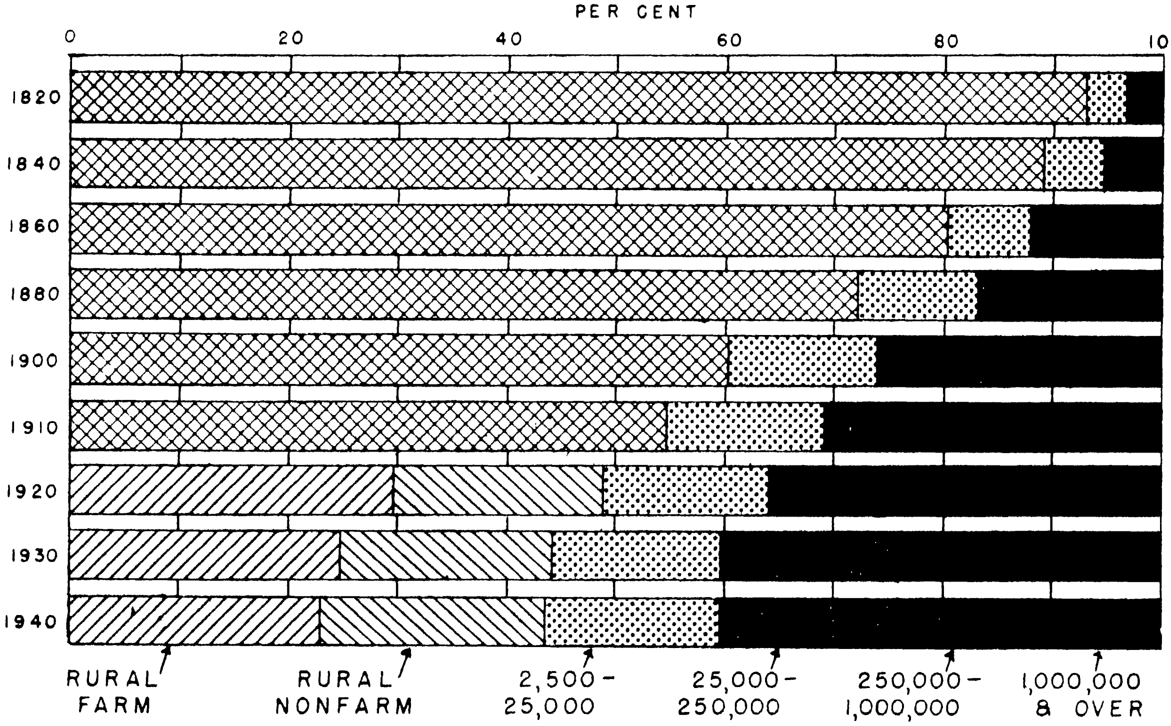


Fig. 8. Proportion of the population in rural areas, and in cities of specified size, selected years, 1820–1940.

were almost reversed during 1910–30, when about 70 percent of the growth came from natural increase and only 30 percent from migration. The two were of approximately equal importance during 1920–30 and 1930–40.

Los Angeles is the outstanding example of a large city which has grown from migration. Its population was about 577,000 in 1920; 1,238,000 in 1930; and 1,504,000 in 1940. Of the 661,000 increase during 1920–30, approximately 90 percent came from net migration and only 10 percent from an excess of births over deaths. The latter was somewhat more important during 1930–40; nevertheless, nearly 85 percent of the increase came from net migration.

The urban fringe. During the past 20–30 years automobiles and paved roads have enabled people to live in the rural areas surrounding cities, but to continue to work in the cities. At the same time the development of tractors and other laborsaving machinery has reduced not only the proportion of the population on farms but the actual *number* of persons as well, the reduction exceeding 1,000,000 from 1920 to 1940, and 3,200,000 from 1940 to 1947. In consequence, the rural population has come to consist more and more of persons who are like city folk rather than farmers. The rural-nonfarm group was less than two thirds as numerous as the farm group in 1920, but outnumbers it by a wide margin (probably about 4,000,000) at present. The movement to rural areas around cities apparently has even checked the growth of cities, for the rate of increase from 1930 to 1940 for the rural-nonfarm population (nearly 15 percent) was almost double that of the urban population (less than 8 percent). A more striking illustration of this tendency is found in the 140 metropolitan districts, where the rate of gain from 1930 to 1940 was about 30 percent for the rural-nonfarm population, but less than 6 percent for the urban population.

Decentralization? Unless war can be prevented, or at least as long as atomic bombs can be used in war, it seems criminal from a military standpoint to permit further increases in the concentration of population in and around large cities. To stop the trend in this direction, however, probably will require government action of some type. Private enterprise apparently finds it more profitable in a large proportion of cases to expand facilities for production in or near large cities than elsewhere. To sacrifice lower costs during an unknown number of years in return for making the nation somewhat stronger from a military standpoint and the factory safer in case of war is a choice few business executives can make for various reasons. So far there has been much talk of the need for real decentralization of population and industry, but little or no progress toward bringing it about. Unless some agency capable of keeping world peace is developed, it is to be hoped that some feasible method of stimulating decentralization will be devised.

THE COMPOSITION OF THE POPULATION

Country of origin. The proportion of white persons in the population increased steadily from 1790 to 1930. In 1790 approximately 81 percent were white; by 1930 this had risen to nearly 90 percent. Since then the Negro group has gained slightly in relative importance. Others—Indians, Chinese, Japanese, etc.—have amounted to less than 1 percent of the population in each census. Indians would have been more important in the censuses before 1890, however, if those in Indian Territory and on all Indian reservations had been enumerated.

When the nation obtained its independence most of the white population was of British origin (nearly 80 percent), with the German group second (over 7 percent), and the Irish third (over 4 percent). Although many thousand Britons immigrated in subsequent decades, they were outnumbered by Germans and Irish, especially around the middle of the nineteenth century. Toward the the end of that century the immigration stream changed more radically in composition, the proportion of persons from northern and western Europe decreasing and the proportion from southern and eastern Europe rising rapidly. The net result was that by 1920 persons of all northern and western European stocks made up about 83 percent of the white population (compared with about 98 percent in 1790) and persons of all other stocks nearly 17 percent (compared with about 2 percent in 1790). The British group had declined to about 44 percent, the German group had risen to over 16 percent, the Irish group to over 11 percent, the Polish group to over 4 percent, and the Italian group to over 3 percent.

The southern and eastern European group would have become considerably more important since 1920 had it not been for the quota restrictions that have been in force since shortly after the close of World War I. During most of this period the number of immigrants from countries in the Eastern Hemisphere could not exceed 150,000 in any year, and the proportion of the 150,000 allotted to each country depended on the importance of that stock in our 1920 population. In consequence, there have been no large changes since 1920 in the relative numbers of persons belonging to the various stocks. Because the more recent immigrants had higher birth rates than others part of the time after 1920, the Italian, Polish, and other southern and eastern European stocks probably compose a slightly larger proportion of the population at present than in 1920.

Little change in the distribution of the population by country of origin is to be expected in the next few decades. Relatively few persons—barely 0.1 percent of our present population—can come annually from quota nations, and relatively few actually do come from nonquota nations. Moreover, as mentioned above, immigrants from quota nations are distributed by stock like our 1920 population. Although the millions of immigrants from southern and eastern Europe who arrived during 1900–25 had larger families than the rest of the population, and for a time contributed more than their share to population growth, most of their children have adopted the American fertility pattern and have small families.

The sex ratio. Every census has shown more males than females in the United States. The number of males per 100 females varied between 102 and 105 from 1790 to 1900, reached a high of 106 in 1910, and declined to 100.7 in 1940. Subsequent estimates indicate that the decline has continued, and that the ratio was slightly above 99 on July 1, 1948. The excess of males in the population of 1940 and earlier years was due in part to the excess among immigrants, the number of males per 100 females in this group varying between 140 and 230 in most years prior to World War I. The more important factors in the aggregate, however, were the birth of about 105 boys for every 100 girls, and death rates for females which were nearly as high as those for males. The decline in the sex ratio since 1910 has resulted from the large reduction in immigration, and from greater progress in lowering death rates of females than of males. Because the full effect of the latter has not been felt as yet, further declines in the sex ratio are to be expected.

Although males have outnumbered females in the total population until very recently, the reverse has been (and still is) true for old people. At age eighty and over there have been only 80–90 males per 100 females—the result of higher death rates for men than for women, especially after age forty. The two sexes have been equally represented among persons in the seventies, but males usually have predominated at ages under seventy. The general pattern has been as follows: Starting with about 105 males per 100 females at birth, the sex ratio has declined until the teen ages, because of higher death rates for boys than girls. Formerly the ratio rose from the teens to middle life because of the excess of males among immigrants, going as high as 118 at ages fifty to fifty-four in 1910. In recent years, however, the decline has continued to the young adult ages and the subsequent rise has been smaller. At present there probably are slightly fewer men than women at most ages from twenty-five to forty-five, and only a small excess of men at ages fifty to sixty-five. These changes may have reduced slightly the ratio of prospective grooms to prospective brides at the ages when most persons marry for the first time, but it is unlikely that they have had, or will have, a significant effect on the proportion of men and women who marry at some time.

Age composition. As the nation has become older the average age of its population has risen also. In 1800 half the people were sixteen or younger; a century later the median age had risen to almost twenty-three years; now it is almost thirty. Future years are sure to see a further rise, perhaps to thirty-four by 1975.

The aging of the population has come about primarily because of the large decline of the birth rate, which has meant relatively fewer babies year by year. With over 50 births per 1,000 persons (as before 1830), nearly 20 percent of the population were less than five years old, but with fewer than 20 births per 1,000 persons (as during the 1930s) it is impossible for as much as 10 percent of the population to be younger than five. Second in importance during the past 30 years has been the large decrease in immigration. The millions of young adults who arrived during 1900–14 have gradually moved into the older age groups and have not been replaced in the younger groups because of the restrictions imposed on the inward movement. Third in importance during this period (but second earlier) is the large reduction in the death rates of children and young adults. This has increased the percentage of persons living to age seventy or older from less than 20 for those born in 1780 to more than 33 for those born in 1880, and will bring much larger gains in longevity for the babies of later years.

The foregoing changes have been accompanied by a substantial decrease in the relative number of children in the population. In 1820 children under five made up over 18 percent of the population; by 1900 the proportion had declined to barely 12 percent, and by 1935 to less than 8 percent (Fig. 9). The rise in the birth rate since 1933 will make this group exceed 9 percent of the population in 1950, but subsequent years are almost certain to see a further decline, probably to well below the 1935 ratio.

School-aged youngsters and their older brothers and sisters (fifteen to nineteen), most of whom are also attending school or have only recently gone to work, also have declined greatly in relative importance. In 1820 this group composed nearly 40 percent of the population; by 1900 the percentage had decreased to 32 and by 1945 to 24. Still further reductions are in prospect, though prob-

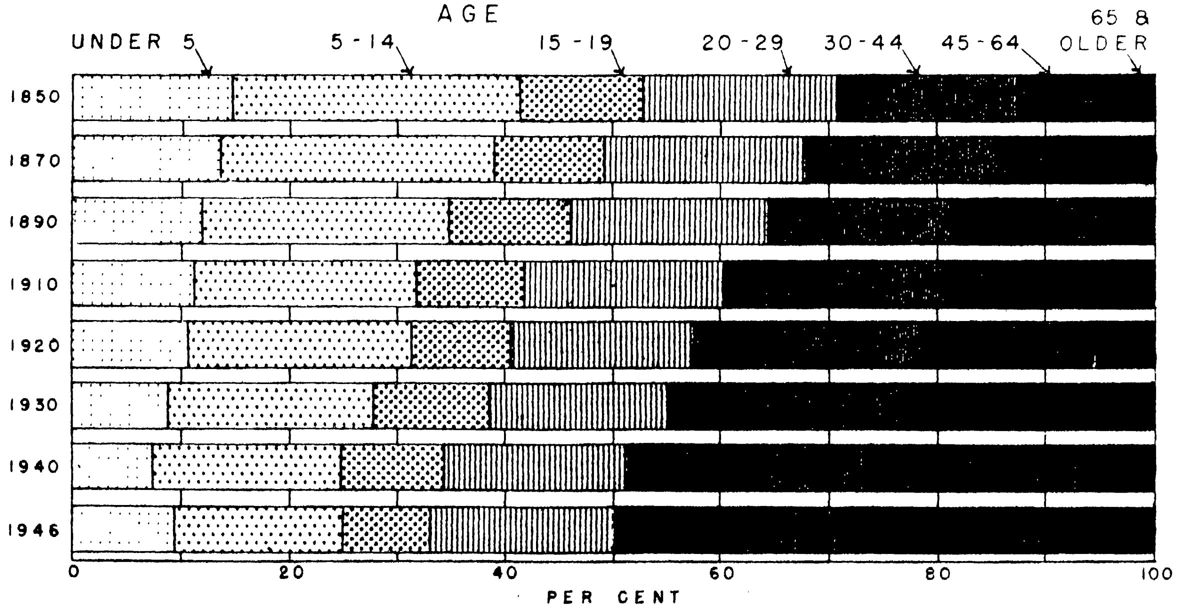


Fig. 9. Proportion of the population in specified age groups, selected years, 1850–1946.

ably not to as low as 20 percent until after 1975.

The fluctuations in the school-aged group during the next few years will cause serious problems for school administrators. In September 1947 there were approximately 2,500,000 children of the usual age to enter the first grade. In September 1953 there will be more than 3,700,000—an increase of 1,200,000, or nearly 50 percent! (Fig. 10.) The increase in the potential grammar-school population will also be striking. In September 1947 there were about 18,000,000 youngsters aged six through thirteen, but in September 1953 there will be about 23,000,000. This gain of 5,000,000 (over 27 percent) will have grade schools bulging at their doors unless there is an adequate building program during the next 5 years. Finding a sufficient number of teachers for the first grade will be very difficult in 1953 unless the teacher-training institutions turn out many more than the usual number of qualified persons during the next few years, or unless former teachers can be induced to return to classroom work.

The proportion of young adults (twenty to twenty-nine) in the population has been comparatively stable, fluctuating between 16.8 and 18.8 percent for more than a century. This has been the pivotal group, for all younger groups have had medium-to-large decreases in relative numbers, and all older groups medium-to-large increases. For ages thirty to forty-four as a whole the increase has been large, persons of these ages making up nearly 22 percent of the population in 1945, compared with less than 15 percent before 1830. Because twenty to forty-four includes the best working ages in most occupations, the increasing concentration of the population in these ages has been one of the factors tending to raise the level of living in the United States during past years. The group will have no such influence in the future, however, for it probably will change but little in relative size during the next few years and then decrease slowly.

Persons middle-aged or somewhat older (forty-five to sixty-four) are more than twice as numerous compared with other persons as they were a hundred years or more ago. In 1840 this group included about 9 percent of the population; now it includes over 20 percent (Fig. 9). Further increases are to be expected in the future, probably to between 23 and 26 percent by 1975. Nearly 90 percent of the men and 20 percent of the women of these ages were gainfully employed in 1940; they made up more than one fourth of the labor force. By 1960 this proportion is expected to rise to nearly one third. The success of employers in industry and other fields in adapting their activities so as to use to advantage many more older workers and fewer younger workers may have a significant effect on the change in our level of living during coming decades.

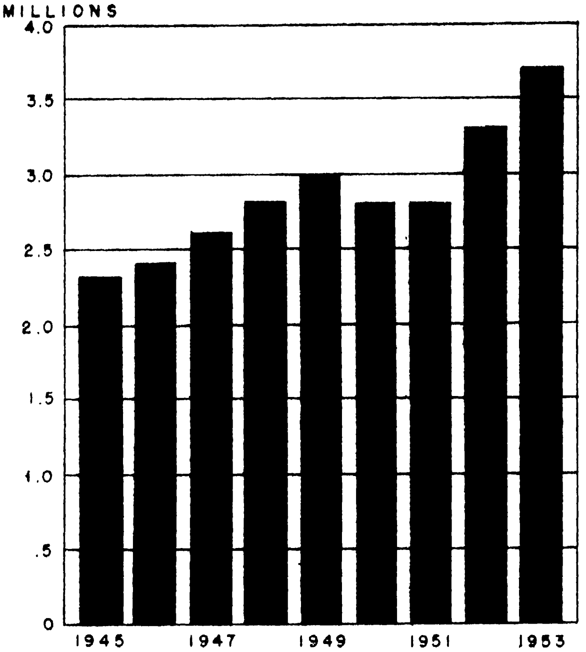


Fig. 10. Number of children of the usual age to enter the first grade, September, 1945–53.

It is the elders—persons sixty-five and over—that have had the largest relative gain in the population. In 1840 less than 3 percent of our people were sixty-five or older; now more than 7 percent are in this group. During the next 25 years the proportion probably will rise to about 11 percent—more than half again as large as at present. This large increase will call for a corresponding expansion in the services and institutions needed by older people.

Although the number of dependent elders will increase greatly in the future, the total proportion of dependent persons in the population will change little if any because of the decrease in the proportion of the population younger than eighteen. In other words, no marked increase is to be expected in the ratio of consumers to producers. Instead, the tendency for a larger proportion of women to enter the labor market is likely to lower the consumer-producer ratio.

OUTLOOK FOR THE FUTURE

The continued slowing up of population growth in the United States that is to be expected is highly desirable from the standpoint of the pressure of population on resources. Moreover, as less of our productive energy needs to be devoted to providing the things needed by additional people, it should be possible to devote more of it to producing things for the people already here. The resulting change in emphasis should help to raise the standard of living.

In the old days of hand-to-hand conflict a nation with a slowly growing population gradually became weaker compared with a nation with a rapidly growing population. At present, in contrast, military might depends in large measure on capacity for economic production. This in turn depends in important degree on ability to have a high standard of living, for it is the difference between total production and production needed for subsistence that influences the amount of war matériel that can be made available. In consequence, a gradual deceleration of population growth by tending to raise our level of living, should increase our power to conduct modern warfare if it is necessary for us to do so.

Whether the expected aging of the population proves to be a serious handicap remains to be seen. Young adults are supposed to be more progressive, ready to change and adopt what is new; middle-aged and older persons are supposed to be more conservative, suspicious of change, and desirous of maintaining the *status quo.* Because of these beliefs the relative youthfulness of our population is cited frequently as one of the reasons why we have achieved the high standard of living we enjoy. If our youthfulness has had an important influence in the past, the rising proportion of older people will be a disadvantage in the future unless steps are taken to retard mental aging. Psychologists say that ability to learn reaches its peak at about age twenty, and declines extremely slowly prior to age fifty. In consequence, if mental aging occurs in important degree before fifty, it is because of a decline in the use of the ability to learn rather than in the ability itself. During future years it should be possible to change the attitudes of many people toward continuing to learn, and to expand and broaden adult educational programs in sufficient degree to more than offset the adverse effect of the rising average age of the population on its ability to progress.

⁂

ON TIME AS A PRODUCT OF MOTION

THOMSON KING

Mr. King is a graduate of Lehigh University. His article "On Life as a Separate Entity" appeared in the February 1947 issue of THE SCIENTIFIC MONTHLY.

THE LONG AND THE SHORT OF IT

The tooth of time gnaws all material things,
In vain the mountain's granite mass resists
***M**otions of wind and rain and heat and frost*
Ever at work tear down the loftiest range.

In ceaseless flow the tireless tides of time
Sweep future through the present to the past.

***M**otion begets events which bring us time,*
***O**n this we build to show what time must be,*
The thing that makes our universe alive
Is that which gives it time, else it were dead.
***O**n motion look, for there you will behold*
***N**ot one, but all the qualities of time.*

IF YOU ask an intelligent person for a definition of the word "time" he will usually hesitate a moment while he decides that both you and your question are quite simple and then tell you it is duration. If you consult a dictionary to be sure you understand the meaning of duration, you will find that it is "continuance of time." You are right back where you started. If you read a few of the many books that have been written about time, you will learn a great deal about the history and philosophy of the subject, from Aristotle to Einstein. You will find other definitions and many interesting facts and fancies, but you will probably end with the feeling, or at least the suspicion, that the epitome of all you have learned, the final answer to your questions, is "Time is time."

I wish to propose and briefly discuss a definition that (whatever it may be) is not circular, for it defines time in terms of something more fundamental than itself. This definition, which is also a hypothesis, is: *Time is a product (or derivative) of motion.* A theory is established when its consequences are sustained by observed facts. I hope to be able to show that many of the accepted qualities and aspects of time follow as natural consequences of the definition. Something must first be said about time in general to clarify and illustrate the definition and our approach to the subject. We shall then present and examine the evidence we can adduce to sustain the definition. If this seems satisfactory, we shall consider very briefly some of the consequences of the acceptance of the hypothesis contained in the definition.

There is an age-old argument as to whether the study and discussion of time belong to science or metaphysics. The whole structure of science is built upon order in nature and intelligence in man. Its province surely includes the investigation of all entities and phenomena to which accurate observation and measurement can be applied, particularly those that are most commonly met with in the study and description of the universe and its laws. Such a conception of science indicates time as a proper subject for scientific study. In this brief article I shall endeavor to use a scientific approach and method, avoiding as far as possible the metaphysical subtleties and philosophic terminology that are so often associated with inquiries as to the nature of time. In order to conform to the method proposed, discussion must be restricted to the entities and phenomena existing in the universe, and strictly exclude anything capable of existing outside it. Therefore, all that will be said applies to time within the universe and not to time as it might concern, or appear to, a creator of the universe, who must have existed before and outside the universe he created.

The debate about time and the literature on the subject began with the Greeks and no doubt will continue as long as men think, talk, and write. After all, time is the one thing that every creature uses all its life, and the only thing of which we all have each day exactly the same amount, no matter who or where we are. I believe the majority of those who have written about time have thought of it as a primary or elemental thing, so my definition puts me in the minority. The philosophy and the literature of time are full of references and commentary on its subjective, objective, and perceptive aspects, of its quality of becoming, and a great deal more that is beyond the scope and unnecessary to the development of my simple hypothesis. There seems, however, to be a general agreement that essential features, qualities, or aspects of time are a flow of events; an order or sequence of occurrences in which we can always perceive a relation of before and after; and a division into three parts, past, present, and future.

To say that time is a product of motion in no way denies it the quality of reality, but does emphatically deny that it is a primary, an independent, reality. If it is a product or derivative of motion, it must be a dependent or secondary reality; motion is the fundamental, the creative, reality. Consider a room in which the only source of light is an electric bulb. If the electric current flowing through the bulb increases, the light increases; if the current decreases, there is less light; if the current falls to zero, the light disappears. The light is just as real as the current, they are both forms of energy, but the light is due to, and a derivative of, the current. We must certainly consider the current primary and the light secondary. If time were a fundamental, an absolute, we could not define it as a product of something else. We might say a good deal about its qualities and attributes, as we do of matter and energy, but we could not explain it in terms of something more fundamental than itself. We can say matter is that which possesses mass, but we can only say mass is an attribute of matter. The definition of true fundamentals is always circular.

Neither does my definition imply that we should stop talking about time and substitute for that good word some reference to its derivation from motion. The definition will, I hope, lead to a better conception and understanding of the nature of time. It does imply that the time of any interval, a minute or a century, is the result of the summation, the integral of the motions that took place during the period. It is much simpler and more convenient, however, to use one word rather than many. It is better to say "Here is a dollar" than "Here is a piece of paper which the government of the United States has declared legal tender with the value of one dollar." The longer expression is useful to explain the real nature of the thing spoken of, but when this is once grasped it is natural to replace the explanation of the derivation of value by a single word.

Scientists continually use the concept of time and measure time intervals with great accuracy, just as they measure mass of matter, dimensions of space, and quantity and intensity of energy. Scientists can describe the phenomena of what Dunne calls "elementary undefinables," but they do not attempt to explain the existence of whatever they accept as a fundamental reality. If a thing is a product or derivative of something else, it can be defined and treated as such, and its existence can be explained in terms of something more fundamental than itself. To show that time is a product or derivative of motion, it will be necessary to prove that motion can create time. I think it will appear self-evident that time of itself cannot create motion.

Some philosophers have taught that time is only an abstract mental concept, having no physical basis of reality, that exists only because of our consciousness of its passage. This view, making the concept and definition of time dependent upon human perception, seems to me narrow, untenable, and useless. It has always led to unsurmountable contradictions or a resort to mystical interpretations. If time has no physical existence, what do we mean when we say that the dimensions of the unit of action, which is an invariant, a constant of the physical world, are energy multiplied by time? It is true that we are conscious of the passage of time because we are conscious of events. The perception of events is necessary to intelligent beings, but intelligent beings are not necessary to events. Events and, consequently, time existed long before there was life on this planet and would continue to exist if all life were destroyed. Mountains rise and are worn down, rivers flow, cycles and seasons, night and day, exist and recur whether or not we are here to observe them. All these changes are produced by motion. I think that a strict analysis of our perception of time will always prove that it is derived from a perception of motion. The ultimate argument for this is, of course, that if there were no motion, there could be no change, no events, no flow of time, nor perception to take note of them or their absence.

The full meaning of my definition may be most easily realized by considering what would happen to time if all motion in the universe were reduced to zero. As the rapidity of movements were everywhere reduced, events would occur more slowly compared with their present rate, but not as compared with each other; for I am assuming that all motions would be reduced at the same rate. A "year" would be longer than our year, but a clock would tick the same number of times, and our hearts would make the same number of beats as in normal years. Light would travel the same distance in a second, for seconds would be longer. When all motion was reduced to zero, time would disappear. When the galaxies and the molecules ceased to move, when electrons no longer spun, when radiation, losing both frequency and velocity, ceased to be energy, nothing could happen, there would be no change, no events. There could be no flow of time; everything would be frozen in an eternal present. If time were a true fundamental, we could not, even in imagination, make it disappear or cease to be, by reducing something else to zero.

To support the hypothesis that motion is more fundamental than time, it will be necessary to meet the argument that motion is a secondary reality because there is always something that moves, and, therefore, movement may be regarded as an attribute of matter, like color or density, rather than a fundamental something in itself. Since it will also be claimed by some that motion must take place *in* time, this must also be disposed of. The idea of the primacy of motion has been developing for a long time. Democritus held that all change could ultimately be resolved into movement. The greatest of the Greek philosophers debated these matters, with the general conclusion that the two great fundamentals of the universe were stuff (matter) and motion. Today the evidence for motion is far more conclusive. If we know anything of the nature of the universe, it is that it is dynamic. So far as we know, all matter in the universe, from electrons to galaxies, is in motion. We appeal to motion to establish the reality of space, for motion consists of something having gone from where it was to where it was not, which would hardly be possible unless what we call space existed—or perhaps motion creates space.

When we consider energy, we find that it is always associated with motion. Perhaps we can say *energy is motion.* The measure of the kinetic energy of matter is the square of its velocity. The energy equivalent of a given quantity of matter is the product of its mass by the square of the velocity of light. The measure of every photon of radiant energy is its frequency, which gives the rate of repetitive cycles involving some form of motion. The development of relativity theory has shown that many things are relative to motion, whereas motion seems to be relative only to other motions. Even mass varies with motion, indeed the whole c. g. system seems to be relative to motion, although of course such effects are observable only at velocities that are an appreciable part of the velocity of light. Motion seems unique in having a fixed upper limit in the speed of light, which is a constant of nature.

It will prove interesting to think a little more about a universe in which there is no motion. What can be said about a universe that contains matter, but has never had motion? By definition this universe contains nothing but matter. There can be no change, no energy, no action (energy × time), no temperature, no life, and no time. We can only describe it as an aggregation of matter, and its history as timeless existence. But these descriptions could not be made from within the universe; they could only be made from the outside by someone who has knowledge of it, or who imagines it. This person must be familiar with the qualities we have referred to as not present in the motionless universe.

I have asserted that time would not exist in the universe I have asked you to imagine. The majority who believe that time exists as a flow or sequence of events, and must have a present, past, and future will agree, but there may be some who will say, "You admit that such a universe has existence, therefore it must have time to exist in." I reply that time may be duration, but cannot be mere existence. Duration always requires the perception of before and after, but to have a before and after we must have change. Duration must have a period of time, and the permanence of something that endures contrasted with the flow of time or the change of something else. In our motionless universe there can be no periods or intervals, and no change. If we reduce the meaning of time to mere changeless existence, we strip it of every quality by which we can apprehend it or perceive its reality. Like the Cheshire cat in *Alice in Wonderland,* it shrinks till there is nothing left but the grin, and finally that disappears. Definition as mere existence makes the whole concept of time meaningless and perfectly useless.

In this motionless universe that contains only matter the concept of space, as we understand it, would be invalid and without meaning. There is no room here for the development of this idea, but the argument would be along the same lines as that for the nonexistence of time. It also seems evident that there could be no organization of matter into atoms, and consequently no differentiation of matter into elements. We would have the rest-mass of the ultimate particles without form or organization.

Let us do one more thing to our imaginary universe. We introduce the various forms of motion that exist in our real universe. There will probably be some form of explosion such as Le Maitre supposed to have occurred in the giant concentrated atom containing all matter which exploded to create an expanding universe. As soon as motion is introduced we have the kind of universe in which we live. It is endowed with energy, time, space, action (including Planck's constant), temperature, atoms, a diversity of elements, and the possibility of life. I think we can safely say the introduction of motion created both the space and time that are aspects of the universe as we know it. Perhaps this will account for the necessity of the space-time concept in the mathematical structure of relativity theory.

Minkowski said, "From henceforth space by itself and time by itself are doomed to fade away into mere shadows, and only a kind of a union of the two will preserve an independent reality." I am a little doubtful that the union of two shadows by a hyphen, or even a mathematical formula, can create an independent reality. If, however, both space and time are created by, and dependent upon, motion, we can understand why Minkowski calls them shadows, and also it is natural that they should be associated to form the four-dimensional continuum of relativity. If we can imagine each electron, or other elementary particle, as creating its own space and time we can immediately understand the reason for Pauli's exclusion principle.

I believe the foregoing illustration will be easily followed and that the birth of time will appear as a natural and inevitable consequence of the introduction of motion into a static universe. There are many other considerations that support the primacy of motion and indicate time as a derivative or product. Long ago Aristotle realized the intimate connection between time and motion when he said: "Time is measured by movement, and movement by time." This does not imply equality in primacy. The amount of metal deposited in an electroplating operation is a measure of the electric current, and the amount of the electric current is a measure of the metal deposited, but there is no question as to which is the primary cause of the operation. Einstein has shown that there is no such thing as absolute time. I think it is generally accepted that time is relative to motion. If time be defined as a derivative of motion, the fact that the rate at which clocks run (as ascertained by light signals) will be different for observers in different frames of reference is easily understood. These observed and accepted facts of relativity are a natural and inevitable result of the definition of time as a product or derivative of motion. I do not know of any other definition of time of which this may be said.

All who write about time must deal with its aspect as an order or sequence possessing three parts, and with its attribute of "becoming." The future is always becoming, or flowing into, the present, the present into the past. In fact, the true present must be thought of as a line or plane, with no thickness. It is simply the boundary between the future and the past. The philosophers have been forced to invent the "specious present," which they define (not too successfully) as the shortest time in which the mind can entertain a single thought and realize duration by experiencing the sensation of before and after; or, according to Webster: "The time span of immediate consciousness; that interval in which what is earlier may be distinguished from what is later, though both are directly present to consciousness." This was necessary to give our thoughts and actions a period with duration in which they may occur. If time were a fundamental thing in itself, we should have great difficulty in saying in what part of it our thoughts and actions occur. They certainly do not occur in the future. Since the "specious present" is obviously a convenient fiction, and the true present has no duration, there is no room for them in the present. They cannot occur in the past. We shall return to this paradox later.

At this point it is interesting to note that all events are in the past before we are aware of them. Suppose the lead of your pencil breaks while you are writing, you may observe the event by seeing or feeling it happen. It requires time for the light to reach your eye, although the distance may be only a few inches, and it takes time for the transmission of the message along the nerves from hand to brain. What was the future, in no time at all becomes the past. An event can take place in a year, a day, or a second, but the only way I can imagine it taking place in the present is by supposing it to move with the present. All events are produced and brought to us by motion. Time is the product, not the cause, of events.

The scientific approach to any subject always involves measurement; measurement requires units. In trying to define time, it will be helpful to consider its measurement and units. Measurement is always accomplished by comparing the thing to be measured with some standard or unit of its own kind; length with a standard length, weight with a unit weight. Our units of time always refer to motion, and, since motion takes place in (or even possibly creates) space, there is usually, but not necessarily, a spatial dimension. The year involves motion and distance; the latter does not enter into the half-life of a radioactive element, which could be used for the measurement of time. If time is truly a product of motion, it is quite natural that its measurements and units must be referred to motion.

Time intervals, like lengths, are most conveniently measured by comparison with a standard, but the measurement cannot be accomplished by direct application of the standard, as in the measurement of length. A standard of length or weight can be preserved and used many times, but time intervals, and the motions that define them, are transient things—as soon as completed they are gone beyond recall. All that can be done is to select some natural phenomenon or to contrive some machine, such as a pendulum, that will continue to repeat a cycle of movement with the least possible variation. The regular repetition of the cycle measures time and gives us our standard.

The measurement of time always involves motion, but it is possible to measure time without recourse to a repetitive cycle. We could measure a second in many ways: for instance, the time it took a weight to fall a certain distance at the surface of the earth; we could measure an hour by a sun dial or the ancient water clock—but we would always come back to motion. Even the decay of radioactive elements is brought about by the motion of particles or photons from within the atoms to the outside. I think Bolton in his *Time Measurement* has given us the final essence of the matter of units and measurement of time if we omit the word "repeated" and quote "All measurement of time depends upon uniformity or invariance of (repeated) motion."

Almost all who write about time sooner or later tell us what Kant and Bergson thought about it. This is indeed indispensable in any general and complete consideration of the subject, but the scope of this article forbids any such excursion no matter how attractive. What Newton thought and wrote is interesting and worth quoting. He realized and acknowledged the dependence of what he considered "common time" on motion, but he believed there was another kind of time that was absolute and independent of all else. For nearly two centuries the prestige of his name helped to sustain belief in the existence of time that was absolute and independent. Today the general acceptance of the theory of relativity has displaced his idea of an absolute time:

> **Absolute, true, and mathematical time of itself, and from its own nature flows equably without regard to anything external, and by another name is called duration; relative, apparent and common time, is some sensible and external, whether accurate or unequable measure of duration by the means of motion and this, which is commonly used instead of true time such as an hour, a day, a month, a year.—*Principia Mathematica*, Book I, Scholium I.**

Unfortunately Newton does not tell us how "absolute, true, and mathematical time" can be known, measured, or distinguished from "relative, apparent and common time," nor has anyone else. It is very possible that he was influenced by his religious convictions in his belief in the necessity for an absolute time. We also know that he thought there might be a point or region in the universe at absolute rest. If this were the case it would afford a point of reference from which absolute motion could be reckoned and, consequently, an absolute time. Today we see no hope of finding such a point; therefore, most things are relative.

From the weight of the evidence I think we may reach the following conclusions: Motion cannot be an incidental attribute of matter, rather it is motion that possesses matter. Motion gives the universe its most important characteristic as a dynamic system in which change is continuous, a sense of before and after is possible, and there is a continuous flow of the future through the present to become the past. All events are due to change, change is always the result of motion, time is a sequence of events, motion is the begetter of change, events, and time. Time is an aspect of the universe that is due to motion. A dynamic universe must have time; a static universe cannot have time.

I have already anticipated one of the most important consequences of my hypothesis by stating that the relativity of time (and the simultaneity of events) to the motion of the observer, which was demonstrated by Einstein, follows as a natural and direct consequence from the definition of time as a derivative of motion. The time of each observer is only another name for the flow of events produced by the motions of and in his frame of reference. Therefore the observations concerning time by an observer in another frame of reference with a different motion will not be the same.

I also believe that the acceptance of the definition may be useful in clarifying the relation of the past and future to the present and the question of their reality. Equally it may help in understanding and disposing of a paradox that has been a most fruitful source of perplexity and the cause of various and lengthy dissertations and explanations. I have no intention or desire to discuss either the meaning of reality or the paradox itself from the philosophical point of view, nor even to imply that I, or anyone else, can give an entirely satisfactory definition of reality. I believe the subject still stands about where Plato left it. But practically every intelligent person has his own idea of the meaning of reality, and I shall be content if he will apply it to what I have to say.

So long as time is considered as a primary and independent something-in-its-self, apparently coming from nowhere and vanishing into nothingness, there must be a strong question as to the reality of the future and the past. It is self-evident that our actions and events cannot take place in the future or in the past. This leaves us only the present, but the present is merely a boundary between the future and the past; it has no thickness, no duration in which events can occur. Here is our paradox: Events cannot take place in the true present, and they cannot take place outside it. Yet they certainly take place. No doubt this delighted Zeno, and it has continued to delight his philosophical heirs and assigns.

Let us approach this problem sticking closely to our definition of time as a product of motion and remembering to substitute motion for time in our thinking of the relation of past, present, and future. I think we shall find the whole problem has been simplified somewhat according to the following argument. I take it that the vast majority of persons (who think at all of such things) consider matter and energy as real and primary entities or, since they are at least to some extent interchangeable, two aspects of one entity. The conservation laws tell us that matter and energy are enduring realities. They change their form, but they are not destroyed. Something cannot become nothing, nor can nothing become something. If matter and energy are real, the motions of matter and energy are real. Motion is inextricably associated with energy.

All the events that we can observe, with the possible exception of certain mental and physical phenomena, are due to the motion of matter and energy. We know of time only by a succession of events, by our sense of before and after. The flow of time is the result of the succession of innumerable events produced by the innumerable motions taking place in the universe. The transition of the motions-that-are-to-be, to the motions-that-are, to become the motions-that-were, gives us our flow of the future through the present into the past. We are equally conscious of motions-to-be and motions-that-were. We recognize the reality and validity of motions-to-be every day of our lives, particularly when we are driving a car. The present is created, brought to us, and carried forward by motion. It is indeed a boundary line between the future and the past, but it is a moving line, generated and swept forward by the same agency that creates events and time, and moving at the same rate. From and because of motion the present moves, events take place, and time is apprehended by conscious beings. Since all are generated by the same motions, all develop at the same rate, and within the line (or plane) of the present. A homely analogy may help to illustrate my meaning. Consider water rising or sinking in a vessel. We can think of the surface as a boundary plane of no thickness between air and water. Yet the events of emergence or submergence of the sides of the vessel all take place in the moving plane. As the water rises, in this parable, the unsubmerged area is the future, the surface of the water is the present, the submerged area is the past.

I think we must conclude the past possesses reality because it consists of events created by motions that were real. The water that ran over the dam yesterday was real, its motions were real. Neither has become unreal because we refer to them in the past tense. The effect of past motions, at least to some extent, determines and creates the motions and events of the present and future. The motions that led to the formation of the earth and the motions of the winds and water of yesterday will certainly continue to produce events today and tomorrow. By similar reasoning the future can be assigned a potential reality. At least some of the motions that will produce the events of the future have been assured by the motions of the past. This potential reality is created and imposed by the nature of the universe and the constancy of its laws. It is illustrated whenever we say, "The sun will rise tomorrow;" or whenever an astronomer predicts an eclipse in 1960, and the event happens as predicted. This is by no means an argument for complete causality. The principle of indeterminacy seems to impose definite limitations as to the events that can be predicted, and we know too little about the nature of life and its potentialities to say much about the prediction of events in which it plays an active part. Nevertheless, unless the future has at least a potential existence, I do not see how any events could be predicted with the certainty that we constantly observe. In so far as we can predict future motions, we can predict future events.

There has been much debate as to whether time is reversible. The general conclusion seems to have been that it is not. The second law of thermodynamics, which is a statistical-probability law, predicts the inevitable increase of entropy in the universe, or any isolated system where work is being done, with the passage of time. This indicates the direction of the flow of time and forbids its reversal. The behavior of energy is always to run downhill to lower frequency numbers whenever it does work. Like Humpty Dumpty when he fell from the wall, the universe can never return to a former state; or, if you prefer, the probability against such a return is practically infinite.

By definition, time is the name we use to denote the integrated result of the practically infinite number of motions taking place in the universe. Most of these motions, so far as we have knowledge of them, are random. The probability that all the motions of even a very small period will be reversed in the next like period, to restore a former state, is practically zero; it may be that the nature of energy makes it not only vastly improbable, but absolutely impossible. Whether this be so or not, the probability against the reversal of all motions is large enough to effectively settle the matter. The irreversibility of time is a direct consequence of its definition. In fact, it is hard to see how time can be reversed for even a very small isolated system. Take a small clock pendulum: at the end of each double swing it returns to the same position with regard to the objects immediately around it and, so far as outward, superficial appearances go, to its former state at the beginning of the cycle. But during the cycle there have been billions of motions and changes in relation among the billions of molecules, atoms, protons, neutrons, and electrons of which it is composed. The probability that all these have been reversed to restore its original state is practically zero. As a single mass, it repeats a cycle; but as an aggregation of particles its behavior is quite different. For it, as for the rest of the universe, time can never return, a past state can never be regained.

Let us see how we would go about stopping and reversing time for one particle of matter. We will consider we have accomplished the first when no events affecting the particle have occurred for one second. Reversibility will be established when we return the particle, at the end of one second, or some other interval, to exactly the same state and conditions that existed with regard to it at the beginning. For our experiment we select a neutron, shielding it from cosmic and other radiations. The effects of molecular motion may be eliminated by reducing the particle and its surroundings to absolute zero. Perhaps it is spinning; each revolution is an event, so it will be best to stop the spin. Have we secured an uneventful second for our neutron? No, for change of position is an event. It will be necessary to stop the rotation of the earth, its motion in its orbit, and all the motions of the solar system and of our galaxy. To restore the former state these motions must all be reversed. Even this will not suffice, for, unless all the motions of the stars within our galaxy and of the other galaxies are stopped and reversed, the position of our neutron with regard to them and their attraction for its mass continue to change, events are occurring, and the probability of a return to a former state is practically zero.

The conclusions to be drawn from the foregoing argument seem to me quite definite. The irreversibility of time for the universe, or even for one particle of matter, can be established quite simply, without appeal to the second law of thermodynamics and the increase of entropy. Furthermore, to stop or reverse time for one particle of matter it would be necessary to stop or reverse all the motion in the universe. The time of the universe is the result of all its motions, of its nature as a dynamic system. If we allow one particle of matter to have motion, we have change of position relative to that particle and a change of gravitational attraction (or, if you prefer, a change of curvature of space) for every other particle; we have change, events, and time. There is an element of unity in this view of time, motion, and the contents of the universe—it seems to be a case of one for all and all for one.

One of the interesting consequences of our definition is that we find in it an answer to the question, To what extent can life change future time? The answer is a definite yes, in so far as it can change motions-to-be, life can change events-to-be, which is changing time. Motion is the outstanding characteristic of the universe. Purposeful directed motion is characteristic of life. "We are observers who possess the power to interfere with what we are observing." Consider a river and its valley. The continued flow of the river and many events in the valley, with regard to both living and nonliving things, may be foretold with considerable accuracy provided there is no interference by man. Many events have taken place in this river and valley in the past; our experience as observers indicates what may be expected in the future if we do not interfere with the natural processes. Then men build a dam, a lake is formed, certain areas are flooded, submerged areas become dry, others are irrigated. Motion, life, and events in the river and valley are quite different because of the decision and actions of certain men. Events and their order and sequence, of which time consists, were changed in the illustration, and are being changed every day in larger and smaller ways by the action of life. Time is certainly changed, unless someone can prove that it was time that compelled the men to build the dam. To do this one must endow time with qualities I am sure it does not possess.

Our definition also answers the question, When did time begin? The answer is, of course, When motion began. And we may add, It will end when motion ceases. These answers may not be entirely satisfactory for those who desire an actual date, but they are the best we shall have for a long time. If a way of determining the age of atoms analogous to the method now in use for determining the age of uranium-bearing rocks from their lead isotope content is ever found, perhaps we may have an indication of the age of the universe in its present form.

I think we can push our analysis of the evidence still further to disprove the argument that time is elemental and that motion takes place in time. If time were a fundamental, rather than a secondary characteristic of the universe, and not dependent upon motion, it would exist in a motionless universe and would not lose or change its nature with the introduction of motion. This absolute time that could exist without motion could have no flow, it could contain no change, no events, no past, and no future. The only quality we can ascribe to it is changeless existence. Motion could never take place in this kind of time, nor can we ascribe to it any genuine reality. It is simply a name, signifying nothing. A universe of which such time were a fundamental characteristic could never be anything but dead mass, it could contain no energy, nothing could ever happen in it. It is certainly not our universe.

In contrast to the difficulties and contradictions that infect and plague the systems that consider time as a primary reality, a something in itself, its definition as a product of motion gives it a reality that is consistent with the universe as we know it, and explains the qualities and characteristics that we constantly observe. Its flow, its quality of becoming, its past, present, and future, all that we have referred to earlier, and much more, demand motion as the creator of time. I believe that further exploration of the consequences of our definition may yield many interesting and useful advances in our understanding of time and the physical world. The idea has never been carried to its ultimate logical conclusions.

A few parallel or paired statements will serve to summarize the evidence and conclusions and illustrate the idea that all that can be said about our perception and experience of time can be asserted with equal propriety and validity if we accept its definition as a product of motion. Time is continuous, not atomic; so is motion. Time is irreversible; the motions that produce events, even for one particle of matter, are irreversible. Time is known by events; motion produces events. In time we have past, present, and future; in motion we have motions-that-were, motions-that-are, and motions-to-be. Entropy increases with the progress of time; entropy increases with the progress of motion. The dynamic character of the universe makes time possible; the dynamic character of the universe is due to motion, *is* motion. The tooth of time gnaws all material things, changing their form; the change of form of all material things is due to motion. Time is different for observers in different frames of reference; the definition of difference for frames of reference is a matter of motions. The state in which we find the universe (less than maximum entropy, less than complete equipartition of energy, and other matters) indicates that the time that has elapsed since its present state began is finite; the same assertion can be made with regard to motion.

These are but a few of many assertions that could be found to support my arguments. I have searched faithfully to find a single statement that is incompatible with the simple hypothesis I have proposed. So far my search has been unsuccessful.

◆

THREE METHODS OF VOTING

WILLIAM S. TAYLOR

Professor Taylor received his doctorate in philosophy at Harvard in 1921, and since 1926 has been professor of psychology at Smith College. He has published two books and various monographs and articles in dynamic and abnormal psychology and several articles in social and political psychology.

MANY groups have learned that they can reach decisions better, and can reach better decisions, through voting than through fighting. Such groups include political units, social organizations, vocational associations, boards, and committees.

Many of the same groups do not realize, however, that scarcely less important than voting is the method of voting. Often, given a variety of candidates, a wrong method of voting elects only the candidate or candidates whom the majority do not want. Sometimes a wrong method elects candidates whom not even an important minority want. Often a wrong method elects only the candidates of an organized or lucky minority. When several candidates are to be elected to a representative body, a wrong method fails to elect candidates to represent the majority, if there is one, and the minority or minorities in proportion to their numbers. Such unsatisfactory elections discourage good candidates from running for office, and discourage voters from voting conscientiously. Such elections also encourage schemers to take advantage of the poor method, to use it for selfish ends. Thus true progress is hampered or thwarted, and even the principle of voting is impugned.

It would seem that every electorate, whether large or small, should know how to choose and apply whatever method of voting is best for any given election. In what follows, we shall consider three general methods, which we shall call Ordinary Vote, Majority Preference, and Proportional Representation, respectively. (Here Proportional Representation means not the European methods of block or party listing but individual use of a single transferable ballot.) We shall find that these three methods often yield significantly different results; that each method is best for particular, everyday circumstances; and that each method is workable, not only for large elections but also, in one form at least, for elections by small associations, departments, and committees.

In Ordinary Vote, as everyone knows, the voter marks his choices either by writing names or by crosses; and election is by the largest numbers of names or crosses.

In both the other methods, the voter either arranges the names, or marks the several candidates 1, 2, 3, etc., from his first to his last choice. The ballots, however, are counted differently for each method. We shall refer to the rules for the counts later. Here let us note only that the rules are simple for Majority Preference, and fairly simple for Proportional Representation; and that they cover uncompleted ballots, erroneous ballots, the counts of the valid ballots, and various ties, even for elections by only three voters.

Majority Preference elects the candidate or candidates whom the whole electorate taken together prefers over every other candidate

Proportional Representation recognizes that many an electorate divides naturally into subgroups, of which one may be a majority; and Proportional Representation elects those candidates who, according to the ballots, best represent the subgroups in proportion to their numbers. For example, if six conservative and three moderate voters are to elect three out of several conservative and moderate candidates by Proportional Representation, two conservatives and one moderate will be elected.

In some elections, depending upon the assortment of candidates, voters, and issues or points of view, the different methods of voting will yield identical results. Often, however, the different methods will yield very different results. Let us compare the several methods through simple examples in which the results will be different (Table 1).

In the first pair of examples, as the underlined scores show, Ordinary Vote elects Candidate C. In this case, C is favored by only three of the seven voters. Moreover, as appears when the same seven voters vote for the same three candidates by Majority Preference, C is just the candidate whom every one of the other four voters wants least. Majority Preference, on the other hand, elects the second M, the candidate who received the smallest sum of preferences. (If he were the first choice of every voter, his sum would be 7.) He is the candidate whom the electorate as a whole wants most.

In the second pair of examples, Ordinary Vote elects the two R's. These candidates represent only three of the eleven voters. Also, as appears when the same eleven voters vote for the same lot of candidates by Majority Preference, the two R's are the candidates six of the other eight voters want least. M. P. elects the first and fourth M's, the two candidates the electorate as a whole wants most.

Nevertheless, these two candidates do not best represent the whole electorate, when we consider that every electorate reduces to individual voters, and that, in most elections, the voters arrange themselves into subgroups—the voters who vote one way amount to one subgroup; those who vote another way amount to another subgroup; and so on. In the case before us, M. P. gives the four m's, who constitute 36 percent of the electorate, 100 percent of the elected candidates; and it leaves the four c's and the three r's unrepresented except in so far as the c's and r's agree after their differences are canceled out. If this electorate consisted of six c's and five r's, with no m's, and the candidates included at least two C's, M. P. would elect only C's. Thus Majority Preference yields, as the name implies, only the majority preference, or the preference of the minorities crudely added to make a majority; M. P. treats the electorate as a lump, and fails to reflect the actual voters' preferences.

Proportional Representation, in contrast, elects the second C and the first M. Consequently, the four c's, who are 36 percent of the electorate, find themselves represented by 50 percent of the elected candidates; and the four m's, who are another 36 percent of the electorate, by the other 50 percent of the elected candidates. Only the three r's, who are nearly 30 percent of the electorate, are not directly represented. They are not so represented because only two candidates were to be elected, and those two cannot be divided to represent three subgroups. If three candidates were to be elected by the same four c's, four m's, and three r's, and

TABLE 1

To Elect One Candidate

	C	M	M
c	X		
c	X		
c	X		
m		X	
m			X
m			X
m		X	
O. V.:	3	2	2

	C	M	M
c	1	3	2
c	1	2	3
c	1	3	2
m	3	1	2
m	3	2	1
m	3	2	1
m	3	1	2
M. P.:	15	14	13

To Elect Two Candidates

	C	C	C	C	M	M	M	M	R	R
c		X		X						
c	X		X							
c		X	X							
c	X			X						
m					X		X			
m						X	X			
m					X			X		
m						X		X		
r									X	X
r									X	X
r									X	X
O. V.:	2	2	2	2	2	2	2	2	3	3

	C	C	C	C	M	M	M	M	R	R
c	3	1	4	2	8	6	5	7	10	9
c	2	4	1	3	6	7	8	5	9	10
c	4	1	2	3	5	6	8	7	9	10
c	1	3	4	2	5	7	8	6	10	9
m	6	5	7	8	2	4	1	3	9	10
m	9	7	8	10	3	1	2	4	6	5
m	5	6	8	7	1	4	3	2	10	9
m	10	7	9	8	3	2	4	1	5	6
r	8	7	9	10	4	6	3	5	1	2
r	10	8	9	7	3	5	6	4	2	1
r	9	10	8	7	6	4	3	5	1	2
M. P.:	67	59	69	67	46	52	51	49	72	73
P. R.:		5			5					

X's represent voters' crosses. The other single letters are initials for "conservative," "moderate," and "radical," respectively. Of these, small letters stand for voters, and capital letters for candidates. Thus, in the examples, voters head rows, and candidates head columns. Each voter's row shows his choices for the respective candidates.

O. V. means Ordinary Vote; *M. P.*, Majority Preference; and *P. R.*, Proportional Representation. Each pair of examples represents the same voters' choices for the same candidates, but with election by the different methods indicated. The first pair of examples compares Ordinary Vote and Majority Preference in electing one candidate. The second pair compares O. V., M. P., and P. R. in electing two candidates. At the bottoms of the examples, the underlines mark the scores of the candidates so elected.

Each small letter could as well represent any fixed number of voters, to make the examples typify also certain large elections.

if the r's put up a candidate, he would be elected. Thus, so far as may be when only a few candidates are to be elected, and the tellers must take the successive numerals on the ballots to represent equal steps between choices, P. R. reflects the actual voters' preferences.

The examples which we have used are rather extreme. They are not more extreme, however, than many real elections;[1] and they bring out the main differences between the several methods, suggesting wherein those differences are significant.

Which method of voting is best; or, which methods are best, and for what purposes?

Clearly, *Ordinary Vote is the best way to choose one of only two candidates.* When, however, there are more than two candidates, "the old single-shot ballot," as Hoag and Hallett called it,[1] often fails to show what the voters want. For that reason, the mathematicians Condorcet, Gergonne, Andrae, Nanson, and Hallett, the schoolmaster Hill, the barrister Hare, and others individually contributed to methods which do show what the voters want when there are more than two candidates.

Majority Preference is the way to choose only one of more than two candidates. This is because Majority Preference compares every candidate with every other candidate, all in a single election. In this way it reveals the whole electorate's first choice of all the candidates. The chosen candidate will be a conservative, a moderate, or a radical (assuming, for illustration, that these are the possible types), according to whether the electorate is preponderately or in effect conservative, moderate, or radical. Thus, in each pair of examples which has been given, M. P. elects moderate candidates because those candidates are most preferred by the electorate as a whole. The first electorate is made up of three conservative and four moderate voters, hence is preponderately moderate. The second electorate comprises four conservatives, four moderates, and three radicals. Since the radicals nearly counterbalance the conservatives, this electorate taken as a whole prefers moderate candidates to any other one type.

Majority Preference is also the way to choose more than one of more than two candidates for congeniality; congeniality of the elected candidates either with the electorate as a whole or with each other. For example, a socially homogeneous club may use Majority Preference to elect new members whom the club as a whole finds congenial. Any electorate may use M. P. to elect a mutually congenial group as a purely executive body, one which is not to develop any new policies or ideas but merely carry out policies which the electorate has fully defined. Such homogeneous clubs and executive bodies, however, seem relatively uncommon. Modern life is so complex that many social groups prefer a somewhat diversified membership; and most electorates authorize their executive bodies to make many important decisions, and at least to develop and recommend policies. Moreover, many elections are to choose members of essentially legislative bodies.

Proportional Representation is the way to choose more than one of more than two candidates for rich representation. Here the voters want to express themselves not as a single unit, which is often a fiction, but as voters whose various interests and ideas should contribute, if only as stimuli, to every decision that affects the whole electorate. Through P. R., the voters as free individuals constitute themselves into groups, whether wittingly or not, and obtain representation somewhat in proportion to the sizes of these groups. Thus P. R. expresses best the interests of the whole electorate.

Each of the methods is useful likewise for choosing between policies, plans, or other alternatives: O. V. for choosing one of two, M. P. for choosing one of more than two, and P. R. for choosing more than one of more than two alternatives.

Contrary to some current impressions, Proportional Representation is not a special instrument of and for political parties. Political parties have used P. R.; and we have heard much, including much that is erroneous, about how P. R. helps minority parties into office. A recent example was the election of two Communists to the Council in New York City in 1945. Politicians and other opponents of P. R. made the most of that election to persuade the voters to repeal P. R.; and the voters did repeal it in 1947. Apparently the voters overlooked the facts that political groups always use whatever methods of voting are available to them, and that such groups have often misused O. V. as they cannot misuse P. R.

Examples are Hitler's coup in Germany; machine rule in Kansas City as compared with popular rule in Cincinnati; and machine rule in New York City under O. V. as compared with popular rule there under P. R.[2] Indeed, before New York City adopted P. R., the Tammany organization elected, year after year, more than fifty-five of the sixty-five aldermen. One year that organization elected, somehow or other, sixty-four of the sixty-five. In contrast, under Proportional Representation, according to Hallett and Woodward, "the votes for candidates of all groups have always been faithfully reflected by P. R."[3] Naturally, any self-seeking group of politicians opposes P. R., unless the group is underrepresented under other methods.

P. R. neither creates a multitude of parties nor destroys the two-party system. In Italy, ten minority parties tried to win the general election in 1913, which was six years before Italy adopted anything like P. R. Conversely, in Great Britain, Australia, New Zealand, South Africa, and Canada, "the single-member district majority system of election" failed to save the two-party system.

P. R. does not split up the electorate into factions of any kind. When whatever groups that occur within an electorate are allowed fair representation through this system, they are likely to become less prejudiced and more cooperative. Some students of voting believe that if the several groups within the North, and the South had been fairly represented in Congress in the 1860s, such representation "would at least have tended towards making a peaceful settlement possible."[1]

Instead of creating groups within the electorate, P. R. recognizes how the voters themselves divide on any given issue. Often these groups are temporary and shifting; for example, Voter Smith votes as a conservative on one issue, as a radical on another, and as a moderate on a third. Even in municipal elections, close observers report that many voters do not follow party lines but cross those lines frequently in registering choices on the ballot.[4]

Proportional Representation does not work against majority rule. On the contrary, it enables any majority of voters to elect their candidates to a majority of the offices. Without P. R., a minority is likely to rule not only through an accidental or a manipulated election process, as we have seen, but even under the guise of majority government. For example: A body of voters are, say, 51 percent conservative and 49 percent moderate. Of the total body, 26 percent, who are conservative, oppose a certain waterway project, and all the remaining conservatives and the moderates—74 percent in all—favor that project. In electing a legislature, the conservatives win all the seats. More than half the legislators represent the 26 percent of voters who oppose the waterway project. So constituted, the legislature puts through a bill that defeats the project. Thus, 26 percent of the voters thwart the remaining 74 percent. P. R., given fair opportunity for the voters, would have elected nearly as many moderates as conservatives to the legislature, and would have included among the conservatives a number who favored the project; so that, given faithful representatives, the project would have been authorized in the legislature by a vote of about three to one.

Proportional Representation protects the majority, when there is one, and minorities alike from tyranny by any group within the electorate; and it makes for enlightened majority decisions. P. R. recognizes that elections occur only because there are groups, actual or possible, many of whom have ideas which the whole electorate, or its representatives, should consider. Through P. R., the majority recognizes the minorities as part of the electorate; the majority gains from the minorities important truths, or reveals the minorities' errors, and so strengthens the position of the whole electorate; and the majority avoids martyring any minorities and driving them underground. Moreover, through P. R., whenever a minority before an election gains enough converts to become a majority after the election, this new majority is represented by its experienced, hitherto minority, representatives; whereas this new majority under O. V. might not, and under M. P. would not, have been represented at all.

Proportional Representation, finally, does not paralyze efficiency. The historic disunity of government in France and some other countries, often ascribed to P. R., can be explained quite otherwise. Those countries did not use the method of the single transferable ballot. For many years France used majority systems, then, for a time, a block-list method which Poincaré called "a transactional, hybridous, equivocal system . . . dangerously favorable to mistakes and deception."[1] The several countries have had various party-list methods which lend themselves to political manipulation and hamper the voter's freedom as what we are calling P. R. does not. Even the party-list methods in vogue in those countries, however, have brought many advantages over the cruder methods of voting which they displaced.[1]

A legislative body, a board, or a committee which is elected by P. R. can often work out plans which combine the insights and interests of the various groups within the electorate. Hoag and Hallett pointed out that "the P. R. councils of Cincinnati and Sacramento were able to choose the first city managers of those cities by unanimous vote."[1] Whenever the elected representatives of different points of view cannot agree on a plan, they can put forward their several plans and choose one of these, or have the electorate choose one, by M. P.

Of course, none of the methods works perfectly. Each method, to work well, requires honest and sufficiently able candidates, voters, and tellers. Each method can be hampered, for example, by "strategic" voting—voting not according to one's preferences for candidates but according to one's estimates of candidates' chances for election; though this distortion is impossible in O. V. for one of only two alternatives, and is unlikely in either M. P. or P. R. Moreover, the numerical intervals between the voter's choices on his ballot often fail to reflect the real intervals between those choices in his mind. Still more accurate methods than the three that have been described are possible under some circumstances. To apply each of the three methods, however, to the special conditions mentioned—O. V. for election of one of only two alternatives, M. P. for election of one of more than two, and P. R. for election of more than one—seems to be the most workable way to improve elections themselves; and to so improve elections is to encourage suitable nominations, voting, and consequent action.[1, 5]

For elections by more than a few hundred voters, of the three methods, only Majority Preference is seriously unwieldly. It is so greatly needed, however, that someone ought to invent a voting machine that would make this method convenient for large elections.

For smaller elections, each of the methods is readily workable. True, when the ballots are few, the usual way of counting the ballots for P. R. introduces chance elements which may give somewhat different results on a recount of the same ballots. An exact way to count the ballots is therefore essential. Such a way is available; a way whereby every accurate count or recount of the ballots comes out the same, even for an election to fill two offices from four candidates by four voters who may themselves be the candidates.[6]

It follows that any voting body, an association, a faculty, a department, a board, or a committee, may well adopt the rule that "elections of one of two alternatives shall be by ordinary vote; one of more than two alternatives, by majority preference; and more than one of more than two alternatives, by proportional representation with the single transferable ballot."

REFERENCES

1. Hoag, C. G., and Hallett, G. H., Jr. *Proportional Representation*, 1926, 13–33, 2, 30, 435, 60ff., 140, 100–10. New York: Macmillan.
2. Hallett, G. H., Jr. *Proportional Representation: The Key to Democracy*, 1940, 7, 2, 18–20. New York: National Municipal League.
3. Hallett, G. H., Jr., and Woodward, W. R. *Nat. Municipal Rev.*, 1947, **36**, 287.
4. Articles by Hallett and others in *Nat. Municipal Rev.*, *passim*.
5. Hallett, G. H., Jr., and Woodward, W. R. *Forum*, 1947, **107**, 97–104, 392–8.
6. For large elections by M. P., cf. Hoag and Hallett, *op. cit.*, 496–505; by P. R., inquire of National Municipal League, 299 Broadway, New York City.

   For small elections, cf. the foregoing sources or, for what some find more convenient and exact methods, W. S. Taylor, *Rules for . . . Majority Preference*, and *Rules for . . . Proportional Representation* (both mimeographed), 15 Pierce Hall, Northampton, Mass.

⁂

SCIENCE ON THE MARCH

TROPICAL MEDICINE—A PARTIAL REPORT

As a nation, we as North Americans have manifested little interest in tropical diseases, although they cause more than one half the illnesses and deaths suffered by mankind. With the exception of a small group of doctors and technical men, this attitude is true also of the medical and allied professions.

During the first world war there was a flurry of interest, which soon subsided. During the second world war, however, when it was necessary to send large contingents of our soldiers into tropical regions, the nation as a whole began for the first time to realize in a small way what was meant by tropical diseases and to understand what a great influence they have on the world's economy and, indirectly, upon our life here. During the war years this interest stimulated intense study of these diseases, and it is gratifying to know that that interest continues.

Notable advances that are of extreme interest both to the professional man and to the general reader have been made during the past several years. To my mind, the most absorbing story in this field is the story of the development of chemotherapy in the treatment of Hansen's disease (leprosy). Only a small percentage of the people in the United States are familiar with the ravages of this chronic and fatal affliction, but everyone has some idea (usually mistaken) about its results and dangers.

The story of the new life interest and hope that has been brought to inmates of leprosaria with the advent of sulfone treatment has never been written. These sulfones are chemically related to sulfanilamide and sulfathiazole. The initial study was begun by the late Dr. Faget at the National Leprosarium, Carville, Louisiana. Early in 1940, he began using Promin in treatment of a group of patients in that hospital. Three years later, after careful study and observation in order to be sure that he did not make a falsely encouraging report, he published a preliminary report of his results. Since then different sulfa drugs have been tried out in various hospitals throughout the world, with uniformly good results. At present it is undecided whether sulfone therapy results in a permanent cure. The question is of technical interest only, however, since so far as the patient is concerned the progress of the disease is stopped and there is the possibility that he will become bacteriologically negative, receive a discharge from the institution, and return to his family and friends. I am glad to report that the International Leprosy Congress held last April in Havana voted the sulfones to be the treatment of choice for Hansen's disease.

The world in general little realizes what this means to a patient with Hansen's disease, and the written word is powerless to convey even a remote idea of the hope it brings to these formerly condemned individuals. Anyone who visited the different leprosy hospitals before 1940 and saw the hopeless attitude of the patients, the masklike, apathetic faces, the dull eyes from which the light of hope, ambition, and desire for life had been extinguished; who visited countries where sufferers were chased out of villages and forced to live like animals in the jungle; or who saw them isolated in deep pits from which they could not climb, but were fed by meager rations lowered at the end of a rope (knowing that when merciful death at last arrived the hole would merely be filled up)—only such a person can have some idea of the humanitarian side of the story back of such a discovery.

It has been my privilege to visit several leprosaria where the sulfone drugs had been used during the previous twelve months—the change was astounding! The characteristic ragged ulcers were healing, the nodular, disfigured faces were returning to normal, and many of the beginning cases of blindness could see again. Many patients who had been complete invalids were going about assisting in the work of the hospital colony. It is quite true that because the drugs are of recent origin there remain numerous old cases of Hansen's disease with mutilated faces and extremities, but the entire mental atmosphere is changed. You now meet patients who know they have an opportunity to get well, who realize that they have a chance to discard the fictitious names—adopted in order to protect their families—and to return to their homes as cured, or arrested, cases.

Those who visit one of the larger colonies leave with the memory of the unfortunate children, sometimes as many as five hundred, who are confined in a single colony because of Hansen's disease. These patients, some of them only five years old, do not as yet realize what the future holds in store for them—but the visitor can see on all sides the misshapen, the blind, and the disfigured derelicts into which these children will develop should they be denied sulfone therapy.

To illustrate "man's inhumanity to man," one has to seek no further than the treatment of these unfortunate people. The fear and dread that the public has manifested are out of all proportion to the danger involved. I believe any doctor familiar with the disease will agree that a person with active tuberculosis is a far greater danger to the community than the patient infected with leprosy. In fact, the mode of transmission of this disease has not yet been discovered or demonstrated, although well over a hundred human inoculations have been performed without a single positive result.

The most recent advance in the field of tropical medicine was announced in two reports given at the International Congress of Tropical Medicine in Washington, D. C., May 11 of this year. One described the work of Smadel and associates who studied the use of Chloromycetin in treatment of scrub typhus; the other report was the study done by Payne and Knaudt, who treated epidemic typhus in Bolivia. These two dreaded diseases are caused by minute organisms belonging to the *Rickettsia*.

The rickettsial diseases, which often range far north, have continuously defied medical science to offer anything in the way of a cure or remedy. Chloromycetin, a new antibiotic, was tried with spectacular success in these two diseases. Epidemic typhus, which is transmitted by the human louse, has had a great influence on the history of the world—perhaps second only to malaria. It is a disease that has often accompanied armies, devastating and destroying them before they reached or accomplished their objectives. And it remains after the army has passed, to add its horrors to the starvation and wreckage of war. Where typhus fever is endemic the fear of the disease testifies to its seriousness. The mortality rate, varying from 20 to 50 percent, bears witness to its destructiveness.

Scrub typhus is perhaps the most serious disease of the East Indies. During the recent war with Japan, this disease at times caused more casualties than the enemy. It was a serious threat to successful military operations. Mortality from scrub typhus is high and has rendered considerable territory uninhabitable throughout the Orient. It is transmitted by a small insect, a larval mite, which has the musical name *Trombicula* and is so small that sixty-four of them standing bumper to bumper would be only one inch long.

The greatest scourge of mankind, however, has been, and continues to be, malaria, from which approximately 10 percent of the world's population suffers each year. Formerly the only remedy available was quinine. The world never produced more than 600 tons of this drug per year; this was used not only for the treatment of malaria but also in the preparation of tonics and even of cosmetics. Yet to treat all cases of malaria the world would each year need 4,000 tons of quinine. During the first world war, Quinacrine (commonly known as Atabrine), the first substitute for quinine, was discovered. It has therapeutic advantages over quinine but also has the disadvantage of imparting a yellow color to the skin. Although this discoloration does not as a rule have any effect on the well-being of the patient, nevertheless it is the basis for many objections to taking the drug. During World War II, because of the immense importance of malaria to military activity in tropical regions, several cooperative studies of the disease itself were made in different countries and intensive search was made for new and better curative drugs.

An outstanding study, organized under the direction of Sir Hamilton Fairley at Cairns, Australia, was a monumental work in all phases of malaria research. In North America the malaria section of the Office of Scientific Research and Development carried on a similar study. These studies resulted in the discovery of several new compounds, some of which have already been added to the therapeutic agents for treatment of this disease.

These new synthetic drugs are more efficient than either quinine or Atabrine. In addition, they do not discolor the patient's skin. With these new drugs for treatment, and the discovery that DDT is highly effective against certain species of the *Anopheles* mosquito, which transmits malaria, it is now possible to carry on commercial and agricultural enterprises in many formerly deserted regions.

The use of penicillin in conjunction with arsenicals has reduced to a few days the duration of illness due to yaws. Yaws affects, as a rule, only the poorer populations and is one of the most loathsome diseases encountered in tropical regions. Kala azar, found in Mediterranean and Oriental countries, now can also be successfully combated with synthetic compounds.

African sleeping sickness, or trypanosomiasis, has been the subject of a great deal of work and study. This disease, which has depopulated large areas of the African interior, attacks domestic and other animals, rendering stock raising impossible in regions where the disease is endemic.

Progress has been made in the therapeutic field as well as in the field of prevention. Study of the habits of the vector, the tsetse fly, has enabled control measures to be put in practice which have greatly reduced the fly population. In the field of chemotherapy, a group of drugs has been found which forms a fairly successful combination. Bayer 205 is used as a prophylactic, amodines are used in treating early cases, and Tryparsamide in treating late or neurological cases. Thus the problem of one of the most contrary and baffling diseases of the tropics has been at least partially solved.

One disease in which no recent therapeutic advance has been made is amebiasis. The protozoa responsible for this disease, which can cause serious illness and sometimes death, may live for years in the human body without causing trouble for the host, as is evidenced by the fact that 10,000,000 people in the United States harbor these parasites, yet few suffer serious results. Although recently much time, effort, and money have been expended in the search for more effective treatments, nothing new has been developed that constitutes an improvement on classical, established treatment.

Recent studies have taught us a great deal, however, about the life and habits of *Amoeba histolytica*, and such information is necessary for a properly organized attack against such a parasite. In this way progress is made in the war against disease. Any new discovery is the result of a long series of studies by numerous primary researchers who deserve, but seldom receive, the plaudits of an adoring, appreciative public.

The student entering the field of tropical medicine need not fear that nothing remains for him to discover. Tropical medicine contains many mysteries—numerous diseases for which no cure is available and many others whose etiology is unknown. Tropical ulcer, which incapacitates so many in the Amazon Valley and is especially severe along the Rio Abuna, occurs in varying severity throughout the tropical world. Its cause is in doubt, and no effective satisfactory treatment is available. Chagas' disease, or South American trypanosomiasis, although its cause is known, still remains the great therapeutic mystery of this hemisphere. No drugs so far have given any indication of activity. The cone-nosed bugs that transmit the disease are not easily controlled even with DDT. The student will be intrigued by the highly fatal Oroya fever, with its romantic history; by filariasis, with its startling sequela of elephantiasis; and Espundia, with its horribly disfigured victims, will stimulate his search for an effective cure.

These are only a few of the problems that remain in the heavily populated tropical countries.

EUGENE H. PAYNE

Parke, Davis & Company
Detroit, Michigan

PETROLEUM AND NATURAL GAS AS SOURCE MATERIALS FOR CHEMICALS

PETROCHEMICALS were a negligible factor in our economy in 1925. Annual production was less than 150,000 pounds. Today they comprise a mammoth industry which produces over 3,500,000,000 pounds of raw materials for synthetic organic chemicals yearly. This development also represents a marked increase in the percentage of aliphatic chemicals produced. In the 20-year period ending in 1945, aliphatic production rose from about one third of all organic chemicals to two thirds of the total. Petroleum and natural gas, however, are becoming increasingly important in supplying aromatics because present requirements are not being met by carbonization of coal. Although petroleum and natural gas are in extremely high demand as fuels, the quantities needed to expand chemical production are very small percentages of their total. The present high tonnages of petrochemicals require less than 1 percent of the total petroleum production.

Even abroad, where petroleum is for the most part imported, construction is under way for increasing petrochemical production. In England, the first installation of a high-temperature cracking process is now expected to have a capacity of about 150,000,000 pounds annually. Other plants for petrochemicals in England will provide for an annual output of 100,000,000 pounds of detergents, 60,000,000 of insecticides, 60,000,000 of ethylene, and 48,000,000 of olefins, aromatics, and oxidation products. A new chemical industry based on petroleum is also being established in Holland. Plants now near completion will produce 4,000,000 pounds of plastics, 50,000,000 of detergents, and over 100,000,000 pounds of nitrates. Other countries are also considering installation of petrochemical plants.

The exponential growth of the petrochemical industry has been brought about largely through chemical research. It is interesting to note that more than 5,400 finished products are now made from petroleum. Many of the raw materials are by-products of the streamlined refinery processes used in motor fuel production. Catalytic and thermal cracking, hydrogenation, dehydrogenation, polymerization, isomerization, alkylation, and superfractionation make possible the production of individual hydrocarbons of high purity. The oxidation of hydrocarbons to carbon monoxide and hydrogen is one of the most versatile used in the petrochemical industry. Among its products are many types of oxygenated products and higher molecular weight hydrocarbons. The American Fischer-Tropsch process, which is being developed primarily for the manufacture of motor fuel from natural gas, will also produce large quantities of alcohols, aldehydes, acids, and ketones. Production of these compounds from the two plants now projected has been estimated as high as 300,000,000 pounds per year. Improved processes for direct oxidation of natural and refinery gases and others are continuously increasing the number and amounts of products. A process has been worked out for the thermal conversion of natural gas or components thereof into acetylene, and a huge plant is being installed to carry out the process. Acetylene can also now be produced from natural gas by a "glow discharge electrical process." The first commercial plant, located at Houston, Texas, will be completed this year. A high-temperature cracking process perfected in England, in which olefin gases and aromatics are produced catalytically from petroleum distillate, is a new development. Even before the first plant is completed, research has brought about improvements which will result in a 50 percent increase in capacity over the initial estimates.

The hydrocarbon raw materials derived from petroleum are principally ethylene, propane, propene, butanes, butenes, butadiene, pentanes, pentenes, pentadienes, toluene, and xylenes. Large amounts of the cresylic and naphthenic acids are also present in petroleum. The cresylic acids have a variety of commercial applications in resins, adhesives, surface coatings, gum inhibitors, flotation agents, disinfectants, metal cleaners, solvents, and insecticides. The naphthenic acids are used in fungicides, high-pressure lubricants, and rust preventatives. Their copper and zinc derivatives are used as paint driers and for treating textiles, in which they prevent deterioration from mildew and mold.

For the past few years, synthetic rubber has been the largest single consumer of hydrocarbon chemicals. In 1946, over 1,000,000,000 pounds, or about one third of the total petrochemical production, was butadiene, all of which was allocated to the synthetic rubber industry. Large amounts of styrene, isobutylene, and other hydrocarbons are also used for this purpose. The availability of substantial quantities of natural rubber probably will not result in a decrease of petrochemical production, for these compounds are in demand for other products, such as plastics and resins. Second, new rubbers are being invented that are far superior to the natural for many purposes. Butyl rubber is a good example. As a material for inner tubes, Butyl is far better than natural rubber because diffusion of air is about one tenth of that through natural rubber, and it has unusually good age and tear resistance as well. Research also indicates that Butyl rubber is superior material for tractor tires, where deterioration from oxidation and light are important factors. A recently developed synthetic rubber has properties that make it superior for tires over previous types. In numerous road tests, tires of this rubber have outworn the natural.

The agricultural industry is being revolutionized by chemicals from petroleum and natural gas. The oil industry is engaged in extensive research on a wide range of products from pesticides to preservatives. An outstanding development for the extermination of nematodes is the soil fumigant, D-D (dichloropropane and dichloropropene), which has resulted in increasing production of some crops as much as 200 percent. Weed killers of the synthetic hormone type are being made from by-products of catalytic cracking. Indications are that petroleum will play an increasing part in supplying insecticides as the necessity for synthesizing aromatics from petroleum becomes greater. DDT and Gammexane, for example, will be made from petroleum. The industry is now doing a $100,000,000-a-year business in insecticides and fungicides. The newly developed weed killers are expected to provide an even greater volume of business for petroleum chemicals. Other petroleum products which conserve food supplies are ethylene and higher gaseous olefins, used to color citrus fruits and hasten the maturing of peaches, apples, cherries, plums, and other fruits.

Another regulating synthetic chemical is "endrop," or alpha-naphthalene acetic acid, which prevents apples from dropping prematurely.

The detergent field is one of the principal markets for petrochemicals. It is in a state of rapid growth and is expected to reach 1,000,000,000 pounds annual output in a few years. Total production of synthetic detergents in 1947 was about 600,000,000 pounds, which contained about 240,000,000 pounds of surface active agents. The general types derived from petroleum, which include the alkyl aryl sulfonates, alkyl sulfonates, and nitrogen-containing aliphatics, constitute a high percentage of the total produced. For many purposes, the synthetic detergents are superior to soap. They are more effective in hard water, can be used in acid solutions, and do not leave curd, which causes such annoyances as "rings in the bathtub." Two factors that have inhibited more widespread replacement of soap have been the inability to produce them in bar form and the comparative ineffectiveness of detergents on heavily soiled cotton goods. Recently, the bar formation problem has been solved for some detergents, and they are now being marketed in that form. Research indicates that the cotton problem can be solved by adding small amounts of phosphates, carbonates, borax, or carboxymethylcellulose. A group of surface active agents, of which quaternary ammonium compounds are the most important, possess excellent germicidal properties. Synthetic detergents also have a variety of applications in textile finishing. Other interesting uses include settling coal mine dust and washing dirt from city streets.

The plastics and resin industries comprise one of the largest markets for petrochemicals. Many types such as polystyrene, vinyl resins, and polyethylene are produced either wholly or in part from petroleum. The 1947 consumption of polystyrene, which requires large amounts of ethylene, was 92,000,000 pounds. It is expected that capacity will be expanded to 200,000,000 pounds by the end of this year. Petroleum also supplied a high percentage of the material for 180,000,000 pounds of vinyl resins produced in 1947, and demand is not yet satisfied in this field. This year's production of polyethylene is anticipated to be 50,000,000 pounds. Petroleum is also becoming a growing factor in phenolic resins and plastics, of which over 200,000,000 pounds were produced in 1947. The demand for phenol for all purposes is much higher than production at present. Indications are that the plastics and resin field will require increasing amounts of chemicals for a number of years. Through research, new adaptations and new products are continually being invented. These products, which range from textile finishes and fibers to radio cabinets, make possible such innovations as water-repellent felt for hats, glass that sheds ice and moisture, and unbreakable phonograph records.

Petroleum also supplies raw materials for a host of other synthetic chemical intermediates and solvents. The protective-coatings industry, for example, utilizes a number of these products for various purposes. Such solvents as acetone, methyl ethyl ketone, isopropyl alcohol, and methyl isobutyl carbinol are derived from petroleum gases ethylene, propylene, and butylene. Polyhydric alcohols have been developed which improve the drying and durability properties of varnishes. Synthetic glycerin and its by-products, allyl chloride, allyl alcohol, epichlorohydrin, also have numerous applications in the coatings industry. The initial plant has a capacity of 35,000,000 pounds of glycerin per year. Another development of major importance is the production of phthalic anhydride from *ortho*-xylene. The first commercial installation is producing at the rate of 8,000,000 pounds a year. This process will undoubtedly be expanded because naphthalene, formerly the sole basic chemical for phthalic anhydride, is not meeting present requirements.

From a tonnage standpoint, synthetic methanol is one of the most important products made from petroleum and natural gas. In 1947, total production was 545,000,000 pounds. A great increase for 1948 is indicated by the fact that almost 73,000,000 pounds were produced in March alone, compared to a maximum monthly production of about 48,000,000 pounds in 1947. The trend toward utilizing petroleum and natural gas as raw materials is illustrated by the shift in sources for synthetic methanol. In 1946, 71 percent was based on coke or coal, whereas, in 1948, 77 percent is expected to come from natural gas.

An important consequence of the expanded methanol production is the increasing quantities of formaldehyde available. Maximum monthly production in 1947 was 46,000,000 pounds; in March 1948 it reached over 60,000,000.

Although complete U. S. data on synthetic ethyl alcohol are not available, it has been estimated that the annual production is about 200,000,000 gallons and that over 50 percent of this volume comes from petroleum and natural gas. It is made largely through reaction of ethylene with sulfuric acid followed by hydrolysis. Commercial installations are also being made to utilize a catalytic process for hydration of ethylene to ethyl alcohol in a plant with an annual capacity of 18,000,000 gallons.

Other high tonnage chemicals are manufactured either wholly or in part from petroleum and natural gas. Among those in over 100,000,000 pounds annual production are acetaldehyde, acetic acid, acetic anhydride, acetone, butyl alcohols, carbon tetrachloride, ethylene glycol, and isopropyl alcohol. It is estimated that of the total domestic production, 35 percent of the normal butyl alcohol, 75 percent of the acetone, and all the isopropyl alcohol come from petroleum and natural gas.

Petroleum and natural gas also furnish materials for certain inorganic chemicals. Large quantities of sulfur compounds are present in many crude oils and natural gas. As an example, 75,000 tons of elemental sulfur were recovered in 3 years from an Arkansas natural gas containing 5-8 percent hydrogen sulfide. In a number of refineries, hydrogen sulfide from processing crude is oxidized to sulfur dioxide for conversion to sulfuric acid. Hydrogen is obtained from the high-temperature cracking of natural or refinery gas. It is of particular importance because it is used to manufacture ammonia and ammonium nitrate, which are so important in meeting fertilizer demands. One particular plant produces sufficient quantities to meet 11 percent of the United States requirements.

The outlook for petrochemicals is extremely good despite the competitive demands on petroleum and natural gas for fuel purposes. The picture in relation to other raw material sources for chemicals is so clouded with acute shortages and high costs that the necessity for increasing use of petroleum and natural gas is evident. As a supplier of aliphatic chemicals, fermentation of agricultural products is the largest competitor. Prices for grain, however, are such that it is more valuable as food for humans and livestock than for fermentation purposes. There is also strong competition for all fats and vegetable oils as food products. The cost of aliphatics from coal, starting with acetylene, is excessive because of the price of coal. Wood distillation, once the second largest source of aliphatics, cannot be expected to furnish the large-scale demands of today from our dwindling forests.

In the aromatic field, petroleum is becoming continually more important. Coal tar, the principal source of aromatics, is limited by the extent of by-product coke-oven operations in the manufacture of steel. Demands have already greatly exceeded the capacity of this industry. Many of the new petrochemical plants now under construction will make large quantities of aromatics. For example, one projected plant on the Gulf Coast is expected to produce as high as 200,000 pounds of aromatics per day from natural gas.

Plant construction by major oil and chemical companies now under way is in itself indicative of the future of the petrochemical industry. Developments in the Gulf and West Coast areas are particularly significant. On the Gulf Coast alone, about $400,000,000 is being invested in chemical plants. Announcements of further construction are continually forthcoming, and this estimate is conservative. On the West Coast, one oil company plans expansion to manufacture 300,000,000 pounds of detergents annually, about 30 percent of the future market. Another oil company will expand its Western operations to increase ammonia production by 50 percent and to manufacture new organic chemicals. Other Western companies are also increasing petrochemical output. These are but a few examples of the skyrocketing growth of the industry. The foundations that are being laid for the petrochemical industry attest its sound position in our economy.

GUSTAV EGLOFF
Universal Oil Products Company
Chicago, Illinois

* * *

BOOK REVIEWS

BASIC TOOL

The Genius of Industrial Research. D. H. Killeffer. vii + 263 pp. $4.50. Reinhold. New York.

PERHAPS it is the publisher's fault, perhaps it is Mr. Killeffer's, but the fact is that the latter should not appear as "author" of this book. Call him editor, collator, or compiler, but certainly not author, for more than 60 percent of the book consists of direct quotations from the writings of others.

Just why Killeffer does not appear in his true guise is hard to understand. Listed as editor, he would receive ample credit, for he has done an excellent job of collecting and assembling material to illustrate and prove the points he wishes to make. At least ten Perkin Medalists and more than a score of other scientific notables relate their experiences and contribute advice.

The book is one of several which might well be required reading in a course that should be given every third- or fourth-year science student.

A thorough understanding of the methods of research is one of the basic tools, the hammer or saw, that must be in the kit of the young research man. Without it he can build no permanent structure but only odd bits of ill-assorted bric-a-brac. Yet personal experience and observation of some hundreds of chemists lead the reviewer to believe that a majority escape college with little real conception of the methods they must use if they are to do creative work. Killeffer puts it bluntly: " . . . methods of research and development as practiced in our industries are not taught in our colleges and universities. The average graduate or postgraduate going into research has to learn, if he ever does, by hard experience the simple principles of his art."

The fault is that of uninspired, or ignorant, instructors, and a book such as Killeffer has edited could do much to direct the undergraduate gaze toward unsuspected vistas. Surely no better examples of the clear, logical reasoning that leads to great discoveries can be found than in some of the great classics of chemical research. Among those Killeffer has included are the story of Carothers and synthetic rubber, Langmuir and the gas-filled lamp, Midgely and lead tetraethyl, Sabatier and catalytic hydrogenation, Hall and aluminum, Hyatt and celluloid, Dow and bromine, Cottrell and the dust precipitator, Williams and thiamin.

The book contains a number of good bits of advice for the research man. Typical are:

> Although it is difficult to persuade research men to admit such a thing, it is possible to provide too much ease and too great convenience for the researcher. The process of incubation and relaxation may become so pleasant that the time of the researchers may be so wholeheartedly devoted to this phase of the operation . . . that nothing at all is accomplished.
>
> The process of research reduces itself to a succession of statements of the problem ever more precisely focussed upon the solution as information accumulated bit by bit shrinks the unknown area around the objective. . . It is vital [to the researcher's] own efficiency that he draw up frequent reports to himself, if not to others.
>
> Far too often the research man abandons his brain at the door of the pilot plant, and because he understands that it is the place to make mistakes, proceeds to make every reasonable error and some unreasonable ones.
>
> . . . most research workers [are reluctant] to face the fact that they often do not know what they mean and intend to say when they come to write a report. . . That leads to the conditions most often complained of in scientific writing: that meaning is hidden or lost entirely among useless words.

And any research director may profitably read the chapter dealing with the proper way to state a problem. As Editor Killeffer says: "Every research laboratory is cluttered with problems left unsolved. Presumably these are beyond or outside the abilities or skills of the available workers, but much more likely they have not been stated in practicable terms."

RALPH F. WOLF

Research Department
Columbia Chemical Division
Pittsburgh Plate Glass Company
Barberton, Ohio

THE STORY OF THE NACA

Frontiers of Flight. George W. Gray. ix + 362 + ix pp. Illus. $6.00. Knopf. New York.

THE characteristic ability of the author to describe highly technical matters in a language intelligible to the scientifically untrained layman is admittedly the secret of the readability of this book. The ease with which the reader comprehends the many functions of the research establishments of the National Advisory Committee for Aeronautics serves to impress upon him the numerous and varied factors that must be considered in designing present-day aircraft.

This story of the NACA is the story of the scientific development of aviation in the United States. Fostered by Dr. Charles D. Walcott, then secretary of the Smithsonian Institution, the idea of a government-supported aeronautical research organization was unpretentiously realized in 1915 when Congress passed the Navy Appropriations Bill. Attached as a rider to this bill was an act which empowered the President of the United States to appoint a committee whose function should be "to supervise and direct the scientific study of the problems of flight, with a view to their practical solution."

The Act specifies that in appointing the Committee, the President should give representation to certain groups naturally interested in aeronautics. Thus, the Committee is made up of two members from the Army; two from the Navy; two from the aeronautical activities of the Department of Commerce; one each from the Smithsonian Institution, the National Bureau of Standards, and the Weather Bureau; and six additional members from the ranks of "those acquainted with the needs of aeronautical science, either civil or military or skilled in aeronautical engineering or its allied sciences."

This Committee is the controlling body of the government's aeronautical research establishment, which today represents an investment of $85,000,000, located in three laboratory centers: Langley Field, Virgina; Moffett Field, California; and Cleveland, Ohio. Members of the Committee serve without pay and have been of such high character and distinction as to render the Committee completely free from political influence.

Employing a staff of scientists and research engineers (wartime peak—6,804 people), the NACA has undertaken and solved countless problems relating to the fundamentals of aircraft design and performance. This work is carried on not in competition with private industry, but in support of it. As rapidly as findings are made and confirmed they are made available to the Army, the Navy, the Civil Aeronautics Authority, the aircraft industry, and the engineering profession. Scientific study and experiments carried on by the NACA have resulted in improved designs for seaplane hulls, engine cowlings, high-lift wings, high-speed wings, propellors, control surfaces, wing panels, engines, superchargers, and many other items essential to more efficient aircraft.

Mr. Gray has set down an illuminating narrative of events concerning the establishment and the subsequent growth of the NACA; but the larger portion of the book is rightfully devoted to lucid descriptions of some of the more important findings of this capable research organization, which has contributed more to the success of American aviation than any other one group.

The author has brilliantly portrayed the pioneering role of the NACA in exploring and conquering the manifold "frontiers of flight."

J. H. Bell, Captain, USMCR

U. S. Naval Photographic Center
Naval Air Station, Anacostia, D. C.

REAPPRAISAL OF DARWINISM

Charles Darwin. Jean Rostand. 237 pp. Illus. 220 fr. Librairie Gallimard. Paris, France.

A SINGLE volume on Darwin which includes not only a short biography and a character sketch, but discussions of his major works and theories, of his predecessors and successors, his philosophy of biology and the present-day status of Darwinism—a single volume which includes all these subjects (each of which have occupied volumes), yet without skimping them with brief one-paragraph summaries, is a marvel of concise writing. The French, as has often been observed, seem to be best at this sort of thing, although it can be done well in any language if the author is sufficiently master of his subject. As the author of several works on the history of biological theories, the lives of insects, and of formal texts, Professor Rostand has the command of his subject that makes the concise treatment possible. There is at present no similar book in print in English, although there should be, for Darwin and his work deserve reappraisal every few years in the light of our changing theories and increasing knowledge. Such a book can serve two or three college generations before it must be written again. In the meanwhile, there could be no better volume than this for the student in biology who is also trying to learn French as one of his tools.

Joel W. Hedgpeth

Institute of Marine Science
The University of Texas
Port Aransas

MORPHOGENESIS AND ADAPTATION

Organic Form and Related Biological Problems. S. J. Holmes. vii + 169 pp. $5.00. Univ. of California Press. Berkeley and Los Angeles.

IT HAS been said that scientific research is 99 percent perspiration and only 1 percent inspiration. Laboratory drudgery leaves little time for thought. Trees and even bushes obstruct the view of the forest. It is therefore gratifying that some scientists are willing and able to undertake the work of thinking for themselves as well as for their less fortunately situated colleagues. Professor S. J. Holmes, in his early eighties, is one of the deans of American biology. A theoretical book from his pen definitely merits respectful consideration.

The theme of the book is stated in the first sentence: "The ability of living creatures to effect their own construction and to keep it in a tolerable state of repair presents a problem which has long baffled the efforts of the naturalistic explainer." This is the biological riddle of the Sphinx—the riddle of evident purposefulness in a machine compounded of evidently purposeless physicochemical processes. Professor Holmes does not offer any new or complete solution of the riddle, but he does give a critical discussion and evaluation of the proposed ways toward such a solution.

The riddle has at least two parts. First, there is the morphogenesis that results in the origin of a smoothly functioning and often very complex system from an egg or a rudiment which appears to have a much simpler and less active organization. Second, there is the direct adaptation, which results in the body's altering its structure or activity to meet changes in its environment. Both parts can be considered in two aspects.

One may describe the phenomena of morphogenesis and of direct adaptation in terms of physiological, and ultimately physicochemical, reactions. The origin of these phenomena may also be analyzed and explained causally in terms of the historical development of a given organism and of the living world as a whole. Professor Holmes' book is largely devoted to the first of these two aspects, although he is fully aware of the paramount importance of the second. To use his own words, "In response to the question how the parts of the organisms came to have this fortunate endowment, the mechanist has no other consistent recourse—although many have tried to find one—than to fall back upon the interpretation of adaptation supplied by Darwin." To pursue the subject further he would have to enter the field of modern population genetics and evolution. This he refuses to do, although repeated references to Darwinism are made throughout the book. Quite consistently, the central idea of the book is that "Each step in morphogenesis may be regarded as a matter primarily of functional adjustment, a part of the general process of homeostasis, or balancing, as much as the various processes subsumed under this name in the adult body." But homeostasis is evidently not comprehensible except as a product of the kind of natural selection that Schmalhausen has recently called "stabilizing selection."

In a very thoughtful and carefully balanced chapter on Autocatalytic Enzymes and the Origin of Life, Professor Holmes develops one of probably the most fruitful ideas current in modern biology, namely, that phenomena of self-reproduction or self-synthesis are among the basic manifestations of life. But he stops short of the conclusion that the origin of self-reproduction constitutes the origin of life in the world. Natural selection, and consequently adaptive and progressive evolution, are implied in self-reproduction. He accepts, however, the dubious theory of Oparin, according to whom complex organic substances, perhaps even proteins, appeared on earth spontaneously before the dawn of life. The chapter on Cancer as a Biological Problem is somewhat isolated in the book, but it gives an interesting discussion of recent hypotheses which attempt to explain the nature and origin of malignant growths. Two of these hypotheses are favored: that of somatic mutation and that of "evocation" resembling cellular differentiation in the normal development. Professor Holmes concludes that "The somatic gene mutation theory and the evocation theory may not be so wide apart as they may appear. It is quite possible [he believes] to account for the facts favoring both of them by a somatic mutation theory which attributes the origin of malignance to changes not in genes but in gene derivatives." Provided that the "gene derivatives" are self-reproducing bodies, their change which is perpetuated in their division products is a mutation by definition. To a geneticist, the somatic mutation theory does not necessarily mean that a gene borne in a chromosome must be changed. This theory furnishes, then, the most hopeful path of approach to the problem of the origin of neoplastic growths.

Th. Dobzhansky

Department of Zoology
Columbia University

THE MOVING FINGER WRITES

Our Plundered Planet. Fairfield Osborn. xiv + 217 pp. $2.50. Little, Brown. Boston.

MORE significant than a political and military history of the world is this moving account of the impoverishment of the earth's biological balance. Man's history of self-destruction as written in the deserts of Asia, the Mediterranean lands, and Africa, in Europe, and even in Russia and Australia is being rewritten at an accelerated pace in the continents of the New World. The pattern is unalterably the same—misuse of the land. Despite the strengths of their armies, the acumen of their mechanical and industrial geniuses—in fact, often because of them—nation after nation has fallen into decay through ignorance and lack of cooperation with the natural forces of the planet.

Fairfield Osborn's question: "Are we to continue on the same dusty perilous road once traveled to its dead end by other mighty and splendid nations, or, in our wisdom, are we going to choose the only route that does not lead to the disaster that has befallen so many other peoples of the earth?" remains unanswered. To understand the question in its full and awful meaning, one must study this most disturbing history. To answer the question hopefully, statesmen and scientists together must devise positive action to stem the flow of lifeblood—soil and water—from a rapidly dying and highly complex biological entity, the planet Earth.

In writing the history, in pointing the finger of effect at the causes of the world's No. 1 problem, the president of the New York Zoological Society has performed a task equal in importance to that of the prophets of old.

A. G. Hall

The American Forestry Association
Washington, D. C.

TECHNOLOGICAL NOTES

Distinctive subject. Scientists who pursue photography for the love of it or for its use in their work are generally "up" on what is new in equipment and techniques. Manufacturers' releases on cameras, accessories, and methods are relatively few and highly technical.

On the other hand, nearly every release, regardless of subject, is an example of the use of photography in industry. Pictures supplement the written word or even replace it. Technical description by means of photographs is comparable to the felicitous use of language to explain what and how, and to some extent why. Improvement in this form of expression is evidenced in concentration on the technical objects themselves rather than making them merely incidental to the beauty of the model holding what is to be described.

The same sort of serious economy in presentation appears in motion pictures of industrial subjects. A subject worth presenting is worth immediate and concentrated attention instead of being dragged into a story by the heels.

History. Tracing the beginning back to Edison's Kinetoscope of 1889, the Royal Bank of Canada has a letter on movies in education and industry. The letter differs from the comment above: "The informational film contemplated by business and industry must be sharpened by human emotional interest; they will find information-giving a dangerous business if the emotion-kindling process is not incorporated." Yes, but sheer interest in a fascinating subject is a sign of kindled emotion.

What Radio Corporation of America calls "television history" was made June 25 when pictures of a fight in New York appeared on a 20′ × 15′ screen in Philadelphia. Every seat in the theatre "was transformed into a ringside seat at New York's Yankee Stadium."

Travels with a camera. Geography begins at home. Amherst's students in geology get such a beginning in the new teaching technique that turns a sequence of pictures into a travelogue, showing first the near-by countryside for easy comparison of pictures and original.

A bird's-eye view is hard to get by squinting at two aerial pictures. Fairchild has a stereoscope that folds like spectacles and weighs only 7½ ounces. In use by photogrammeters (or anybody who wants to get a stereoscopic view from aerial photographs), the device stands on three legs, 4 inches above the prints.

Glass. Eastman Kodak has reported a new glass formula, described by the company as a major optical development of the past fifty years. The formula includes lanthanum, tungsten, and tantalum. With this high-index glass, designers have produced lenses that give sharpness and clarity, particularly in the corners of the field.

Air Force photographers have a new telephoto lens by Eastman. This "giant" is nearly 4 feet long and 15 inches in diameter, to take 9″ × 18″ pictures from ten miles up. To protect against the cold of the high altitude, the mounting has a thermostatically controlled electric heater.

Quite the opposite is the Photographic Arts Manufacturing Corporation's Britar telephoto lens for Keica or Kardon. It weighs only 7½ ounces.

Speed. Last year General Electric and Eastman worked together to develop an industrial camera that will picture the millionth-second surge shown on a cathode-ray oscillograph. After the snap the camera develops the picture and projects it on a viewing screen.

Another General Electric development is apparatus that produces a brilliant flash by discharge of electricity through argon to expose objects in the field for only two millionths of a second. The camera waits, shutter open. A shell moving at more than twice the speed of sound covers only 65 thousandths of an inch in that interval.

Camera's camera. Precision makes a new Bausch & Lomb camera suitable for testing new lens formulas. The rigid frame keeps film plane and lens board parallel within .002 inch. The negatives are compared with test charts for check of resolving power, astigmatism, etc. The camera takes 5″ × 7″ plates, and is also used for precise photographic and copy work.

Easy reading. Microfilm use, regardless of advantages, has its bad side. It removes some of the reason for visiting distant libraries and it condemns readers to stuffy booths. American Optical Company's new microfilm reader looks comfortable (in the picture) and seems to have advantages. It weighs 70 pounds, and can be wheeled about quickly and silently. The image has no "hot spot," and the light can be adjusted to suit and save the reader's eyes. The motor has high and low speeds each way to reach a "page" quickly, and a 360° reverser to "turn" the page around.

Serious work. The "Classic 35" by Craftsmen's Guild, Hollywood, takes 35-mm pictures and yet is only 1⅜″ × 2⅜″ × 3⅝″ over-all, "world's smallest" camera for such size film. Fuss and bother are nil, according to the makers, even to preventing double exposure or no exposure; yet the camera is efficient for "really serious work."

Drying. Photography and other processes, including some preparations in medicine, may benefit from the "Anhydrator" of Oscar Fisher Co., New York. Clean air is recirculated for drying films and materials without heat.

M. W.

COMMENTS AND CRITICISMS

CAN INTELLIGENCE BE MEASURED?

(A Reply to Davis and Havighurst)

It would appear that the crux of the article by Davis and Havighurst on "The Measurement of Mental Systems" (SM, April 1948) is the assertion that a so-called intelligence test is invalid if it gives higher average scores to the highest socioeconomic group than to the lowest socioeconomic group, on the ground that actually there is no difference between these two groups in intelligence.

This belief by the authors seems to be made very clear by the statement (p. 312, col. 2) that "any differences between the average response of different cultural groups to a mental problem may be attributed to their unlike cultures. Therefore, all problems that show socioeconomic differences in performance should be ruled out of the tests as unfair."

The assumption that all socioeconomic groups are equal in innate intelligence and that they make different average scores in intelligence tests only because of faults in the tests seems to run through the article from the first page. Yet this assumption appears to be wholly gratuitous. In the whole 16-page article there appears to be no proof whatever of this statement. The authors seem merely to give any other contention a wave of the hand with the assertion (p. 312, col. 1): "There is no evidence, and no theory shared by the leading human geneticists, to the effect that the underprivileged socioeconomic or racial groups are genetically inferior to the more prosperous socioeconomic groups." One is led to wonder who these "leading human geneticists" are. No names are given, nor is any reference given to any research or published opinion.

The authors go on to say: "There is abundant scientific evidence, advanced by sociologists and social anthropologists during the past twenty-five years, that a child's particular social and cultural environment directs, trains, and motivates his behavior." The odd fact is that nobody would contradict that statement, but it proves nothing with reference to the preceding statement. We all know that environment governs behavior, and it would seem that most scholars believe that heredity also helps govern behavior. The age-old controversy has been merely: Which has the more influence, heredity or environment? It is obvious, then, that to offer the assertion that scholars believe that environment governs behavior as proving that heredity does not govern behavior is fatuous and beneath the intelligence of the two authors.

Let us do a little logical reasoning in this matter. We all agree that there are hereditary differences in intelligence—that is, in the ability to solve life's problems and adjust oneself to the environment. If two young men have a hereditary difference in intelligence—the ability, as we said, to solve life's problems and adjust themselves to their environment—it is obvious that the favored one will be more likely to succeed socially and economically than the less favored one. This means that in the long run the less favored individuals—those with less hereditary ability to solve life's problems and adjust themselves, especially in these days of insecurity—must necessarily tend to gravitate into the lower socioeconomic groups. This means that there comes to be a tendency for low socioeconomic groups to be of lesser innate intelligence than high socioeconomic groups.

It certainly has been proved beyond question that intelligence is inherited; that is, on the whole, the children of parents of lesser intelligence tend to be themselves of lesser intelligence. This fact, coupled with the fact that parents in the lower socioeconomic groups tend to be of lesser intelligence, means that *children* in the lower socioeconomic groups tend to be of lesser intelligence.

We see, therefore, that logical reasoning brings us to the inescapable conclusion that children in the lowest socioeconomic group must be expected to make lower average scores on a valid intelligence test than those in the highest socioeconomic group.

It is interesting that the authors consider the Otis Alpha Non Verbal Test to be the most valid in that it differentiates least between socioeconomic groups. Actually, however, there would seem to be no way to determine on a priori grounds how much difference between socioeconomic groups is to be expected in average scores or in percentages of passes of items in a test; hence, it would seem that the authors' Figure 1, showing various amounts of average difference between percentage of passes for items of several tests, is of no significance in this connection. We don't know whether the test that showed small differences between the two socioeconomic groups is a better or poorer test of intelligence than the others.

One important consideration that the authors failed to take into account is that intelligence tests have various purposes, and that different kinds of validation of items are needed for the different purposes. Thus, one purpose of an intelligence test might be to study scientifically the hereditary differences in intelligence between races or socioeconomic groups, whereas another purpose might be to determine for practical purposes the probable rate at which different pupils in a given school will progress in that school.

If our purpose is of the first kind, we may need to go to great trouble and expense to get a test that is as free as possible from any influence of school environment. It might be worth while to spend five hours testing each individual with a great variety of items, including, say, tests of manual dexterity, learning ability in an actual learning situation, etc.

On the other hand, if our purpose is merely to prognosticate pupils' ability to progress through a given school and perhaps to classify them accordingly within that school, we can use a much more convenient test. This is because even the differences among pupils that may be the result of different "cultures" have a marked effect in determining pupils' rate of progress through a given school, and consequently it is of no great concern to the test maker if the score is affected to some extent by "cultural influences" so long as it makes an accurate prediction of what it is desired to predict.

The authors criticize the Otis tests because they were validated on the principle that that item is best which best differentiates between pupils who are making slow progress and those making rapid progress. But it would seem obvious that if predicting rate of progress is the chief purpose of the test, then rate of progress of pupils is the best criterion of validity of the items. To be sure, we cannot prevent cultural influences from helping to determine pupils' scores but, so long as those influences also affect rate of progress in school, we need not consider them altogether vicious in influencing the pupils' scores.

As indicated above, the making of a practical and workable intelligence test involves a compromise—some sacrifice of scientific validity for the sake of economy and efficiency of administration. This means that for practical purposes the variety of types of items is necessarily somewhat restricted—to the distress of the authors.

If the authors wish merely to point out the shortcomings of the present tests for such scientific uses as studying differences in intelligence between various racial or socioeconomic groups, well and good. But in the present study the authors have made the following errors:

1. It is assumed almost gratuitously that all socioeconomic groups are equal in inherent intelligence. No evidence is given in support of this assertion.
2. The obvious error of such a hypothesis, as becomes clear through a little logical reasoning, is ignored.
3. The conclusion drawn from the erroneous hypothesis, namely, that any test that differentiates between high and low socioeconomic groups is invalid, is necessarily erroneous.
4. No account is taken of the different uses to which intelligence tests are put and of the difference in mode of validation needed for the best results in the different uses.
5. Tests are condemned for having been validated in the manner actually best calculated to make them most helpful for the purpose for which they will be used in ninety-nine cases out of a hundred, that is, predicting relative rates of progress of the pupils of a given school through that school.

Incidentally, it is noted that the title of the article uses the term "mental systems," yet the article would seem to give no explanation as to what is meant by mental systems nor any justification for the assumption that there are such things as mental systems.

ARTHUR S. OTIS

Washington, D. C.

(A Rejoinder to Mr. Otis)

Mr. Otis makes two basic criticisms of our paper:

First, he says that we assume that all socioeconomic groups are equal in innate intelligence. This is stating our position a little more strongly than we would do; we would rather say that in view of what is now known about genetics and about intelligence testing, the safest assumption is that the several socioeconomic groups in the United States are equal in innate intelligence. We cannot prove this assertion on the basis of evidence at present available, but neither can the assertion that socioeconomic groups differ in innate intelligence be proved with evidence now on hand. (See the mathematical papers of J. B. S. Haldane, Lancelot Hogben, Julian Huxley, and Levy on genetic selection; the studies of twins by Newman, Freeman, and Holzinger; and the papers of B. Kuppusawny.)

In particular, the "logical reasoning" Mr. Otis offers on this matter of differences of innate intelligence between socioeconomic groups leaves us unconvinced. We agree that two young men may have hereditary differences in the "ability to solve life's problems and adjust themselves to their environment." We suggest, in passing, that intelligence tests probably do not measure this ability very well, nor are they intended to do so. The argument advanced by Mr. Otis is that the more innately intelligent young man, and others like him, will be more likely to succeed socially and economically than the less gifted ones. In the long run, he argues, the less intelligent ones (genetically) will gravitate into the lower socioeconomic groups, and will pass on their less favorable genetic characteristics to their descendants.

We cannot agree with this argument, on the basis of what is now known about genetics. Intelligence, in so far as it is passed on genetically, is transmitted as a highly complex set of mental characteristics on an unknown but certainly large number of widely separated genes. The kind of argument advanced by Mr. Otis might apply to such a relatively simple characteristic as eye color, if eye color had any function in socioeconomic competition, but it probably does not apply to such a complex set of characteristics as intelligence, particularly when intelligence is defined as ability to solve life's problems and adjust oneself to the environment. From what is known about genetics, the children of a man who was well favored with innate intelligence would have very little chance of being better favored than the children of a man who was less well endowed genetically in these respects. Both men carry many latent characteristics as well as manifest ones in their genetic structure. Both men, furthermore, have wives who in each conception contribute half the genes to the offspring. There is very little chance that socioeconomic differences have been translated into genetic differences of intelligence under these conditions in the course of the half a dozen generations that span the life of American society. Possibly a geneticist, with present-day knowledge and power to control human mating, could breed groups of humans with different average innate intelligence in the course of fifty or a hundred generations, but that remote possibility does not bear on the present situation.

Thus we believe the safest assumption that can be made is that there are no innate differences of intelligence between socioeconomic groups in the United States today.

But suppose we grant the possibility that there is a small innate difference of this sort (which we must consider as a possibility, though insisting that it is certainly small if it exists). Even in this case, we should point out that, with the great individual variability of innate intelligence in all socioeconomic groups, and with the higher birth rate of the lower socioeconomic groups, there are many more highly endowed children born in these groups than in the upper socioeconomic groups, because the latter groups make up a smaller fraction of the population. Hence, any use of intelligence tests that denies educational opportunity to children of the lower socioeconomic groups is a socially unhealthy act. This is the basic criticism we are making of current intelligence testing.

This brings us to the second criticism made by Mr. Otis, namely, that we do not take account of the different uses to which intelligence tests are put, and of the consequent differences in desirable modes of validation of the tests. On the contrary, we are very much concerned with the uses to which intelligence tests are put. Our observation of the use of intelligence tests shows us that where these tests are used in school systems on a large scale, they are used to determine what kinds of educational opportunity shall be given to boys and girls.

In the very first grade, children in most large cities are segregated into "fast" and "slow" groups, partly upon the basis of their intelligence-test scores. This practice is continued in many secondary schools. In many cities, discrimination against lower socioeconomic neighborhoods or against ethnic groups, with respect to school equipment and curricula, is defended upon the basis of their low average test scores. Thus any cultural loading in the tests becomes a matter of urgent practical influence upon public education in this democracy.

The school authorities go to the expense of giving intelligence tests, not merely as Mr. Otis claims, to prognosticate pupils' ability to progress through school, but actually to put pupils into one or another kind of learning program, or to decide how much to expect from pupils. If, therefore, a boy of high innate learning ability but of experience inadequate to score high on a certain test gets a low score, he may be put into a class from which not much is expected, or he may be shunted into a curriculum that will prevent his entering college, or his parents may be advised that he is not "college material."

It is just because intelligence tests are used as a means of determining what educational opportunity children shall have, and of giving the best opportunities to children with the higher scores, that we insist that nothing but the best test of learning ability should be used in the schools. The best test of learning ability for use in American schools is one that does not penalize any group because of its cultural experience. We believe that such a test can be devised; we believe that it can be devised from problems which all children have had approximately equal chances to experience; and we expect that it will show as high a correlation with teachers' ratings of the same pupils as did the Stanford-Binet. The correlation of the Stanford-Binet test with "school success" was only 0.48.

ROBERT J. HAVIGHURST
W. ALLISON DAVIS

Department of Education
University of Chicago

THE SCIENTIST AS PHILOSOPHER

I wish to congratulate Dr. A. W. Lindsey on his article on "The Faith of Science" (SMO, May 1948). . . . It seems to me replete with good concepts and sound viewpoints.

Following through on the subject of the philosophies of scientists, which Dr. Lindsey and I agree in distinguishing from the philosophy of science, I should like to suggest that it is hardly necessary to concede anything like as much austerity to the scientist as Dr. Lindsey seems to assume. Is not the fact of the scientist's leaning toward science usually grounded upon a warmth of imaginative appreciation roused in him in youth by the visible content of this material universe? And is not his supposed austerity really something rather fictitious, attributed to the scientist by bewildered nonscientists because they have failed to orient themselves successfully regarding the vitally personal matter of the philosophies of their scientist friends? Where such a group difference seems to occur between scientists in general and other educated persons in general, I hesitate to ascribe it to any higher loyalty to truth on the part of scientists. Surely bankers, statesmen, and historians would be as truly averse to grounding their lives on fallacies as would scientists. Where there is a difference it must be due rather to the nature of the training they have had in the process of ascertaining truth, or to aspects of their informational equipment.

Until due allowance has been made for the differences of mental baggage, there will be many failures to grasp the deeper similarities in the human qualities of men of science and their fellow-beings. . . .

If our scientist has less to say about faith to which men have recourse when they reach the limits of true comprehension, and more to say about an agnosticism with no craving for an explanation when accurate (or reliably approximate) explanation is obviously beyond reach, that also is not so much through austerity as it is through a different training in the processes of ascertaining truth, a different familiarity with the great cosmic reliabilities, and a better appreciation of the congenial livability of an unperfected, growing, solidly verified system of truths.

ADDISON GULICK

Department of Bio-Chemistry
University of Missouri

OF ERROR, A TRACE

It is mystifying that Professor Ferguson, who wrote such an interesting article on contact lenses (SM, June 1948), should confuse optometrists with opticians, but possibly this accounts for his statement on page 497 that optometrists "are neither trained nor qualified to diagnose eye defects or prescribe lenses, regular or contact." Anyone familiar with the training and experience of optometrists knows that Professor Ferguson's generalization has no bases in fact, for there are a great many optometrists in this country well qualified to determine errors of refraction and to prescribe eyeglasses. A *scientific* monthly should not supply space for such an unjustifiable attack upon a recognized profession and widespread practice.

MATTHEW LUCKIESH

Lighting Research Laboratory
General Electric Company
Cleveland, Ohio

THE

SCIENTIFIC MONTHLY

NOVEMBER 1948

ELECTRONS AND COMPUTATION

W. J. ECKERT

Dr. Eckert (Ph.D., Yale, 1931) is Director of Pure Science, International Business Machines Corporation, and Director of the Watson Scientific Computing Laboratory at Columbia University. He was a member of the Department of Astronomy at Columbia from 1926 to 1940 and Director of the U. S. Nautical Almanac Office from 1940 to 1945. In 1934 he organized the first large-scale automatic computing laboratory for scientific computation. This was sponsored by IBM and Columbia University.

AT A recent ceremony in New York City, a new electronic calculator was dedicated to science. The calculator reads "written" instructions for performing the most complex calculations, has an internal "memory" capacity for instructions and problem data totaling 400,000 digits, performs the various arithmetical operations in a few thousandths of a second, and prints the desired results. Known as the IBM Selective Sequence Electronic Calculator (Fig. 1), it is located at the World Headquarters Building of the International Business Machines Corporation, where it is operated by the IBM Department of Pure Science. It was built in the company's engineering laboratories and dedicated on January 27, 1948, by Mr. Thomas J. Watson, president of the Corporation.

Actually, each operation of the Calculator could be performed by any reader of this article, given time enough; the speed and accuracy with which these operations are performed, and the flexibility and ease with which they are combined into a large calculation, are the source of the Calculator's usefulness.

With a little practice the average person could multiply two 14-digit numbers together to give a 28-digit answer in about 20 minutes; the machine does 50 such multiplications in a second, or 60,000 in 20 minutes. If the machine should make an error in an 8-hour day this would be only one error in over a million multiplications—less than one per lifetime for a computer with pencil and paper. Division requires just a little more time, and addition and subtraction of 19-digit numbers are performed in less than a thousandth of a second.

The basic capacity of 14 digits in multiplication and division and 19 in addition and subtraction is adequate for the great majority of problems; where additional accuracy is required these basic operations may be combined easily to handle numbers with 28, 42, or any desired number of figures.

To schedule multiplications, divisions, additions, and subtractions at such a prodigious rate, it is necessary to have the required numbers available at the right time and in the right order. Each multiplication, for example, requires two 14-digit numbers and generates a 28-digit answer; in other words, thousands of digits must flow into and out of the calculating unit each second. Moreover, these arithmetical operations must fit into a complicated pattern where each computed result may be one of the factors of a subsequent operation. It is therefore necessary to have thousands of numbers instantly available for use as multipliers, multiplicands, etc., and to select them as required.

This selection is made possible by providing a vast storage reservoir, or "memory" for numbers.

The memory units are devices that will accept a number, hold it until needed, and deliver it when called upon. The Calculator has a total internal memory capacity of 400,000 digits, or 20,000 19-digit numbers with algebraic signs. As the calculation progresses, results are recorded from the memory and new data are read into it. The speed of reading in and out is such that the entire 400,000 digits can be replaced in about 10 minutes.

In order to combine all these operations into a single calculation, the control facilities for instructing the machine to perform the required operations are of a very high order. The control is accomplished by a system of written instructions, which the machine reads and follows. The following are typical commands:

"Read a number from one of the reading units and store it in a given memory unit."

"Take the number from a given memory unit, multiply it by the one in another unit, drop a specified number of digits from the answer, and deposit it in a third unit."

Since multiplications and other operations are scheduled at the rate of 50 or more per second, it is necessary to supply the corresponding instructions at the same rate. To provide the necessary instructions for a complicated problem, a number of innovations in control technique have been introduced:

a) All instructions are given in numerical form. Each unit of the machine and each operation it can perform have been assigned a number, and the presence of this number in the instructions calls into action the associated unit or operation. This procedure permits the use of all the numerical facilities of the machine, not only for problem data but for handling operating instructions as well. For example, instructions totaling 400,000 digits may be stored in the machine at one time and new ones read in at the rate of 25,000 per minute. In fact, the machine can compute detailed instructions as it goes along from general outlines presented to it.

b) Each line of instruction carries with it a number instructing the machine where to look for the next command. This command may depend upon the result of a computation just completed. The commands are usually for a series or sequence of operations to be performed in a given order. At any one time a new sequence can be started at any one of about 200 places. Any given sequence can be repeated a prescribed number of times or until a given result is derived. This facility is reflected in the title "Selective Sequence."

c) Mechanical means are provided for preparing the instructions before they are taken to the machine. The basic instructions are recorded in punched cards which can be automatically checked, rearranged, duplicated, and printed to give the best procedure. Changes in instructions can be made quickly by removing a card from the deck and replacing it by a new one.

The machine records results in two forms: as a printed record and on cards which can be used for further work with this or other punched-card machines. Selected results are printed at the rate of 25,000 digits per minute so that the scientist can keep in close touch with the calculation as it progresses (Fig. 2).

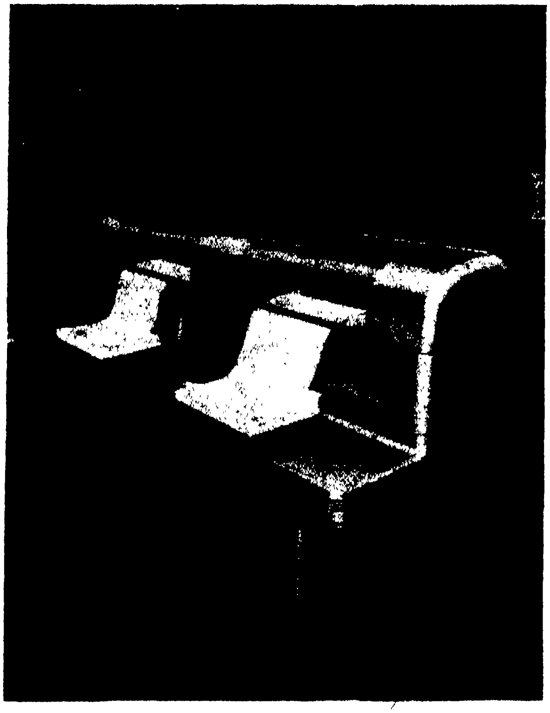


Fig. 2. Printers which record intermediate and final results.

The next question is, "How are these operations performed?" Two principal kinds of units are employed in the machine, electronic and electromagnetic. In the ordinary radio set, electronic circuits involving vacuum tubes are used to receive and amplify the radio waves. In these circuits there are no moving mechanical parts; all operations are performed by small streams of electrons which start and stop as required. In order, however, to produce audible sound it is necessary to move a material diaphragm in the loud speaker. This is accomplished by means of an electromagnet which moves the diaphragm in accordance with the output of the electronic circuits. Similarly, the Calculator employs electronic circuits and electromagnetic devices to control many of the operations. Electronic circuits are used for all the arithmetical operations, for part of the control operations, and for part of the memory; in fact, they are used wherever highest speed is required (Fig. 3). The electromagnetic devices are used to control the reading and recording of data and a great part of the traffic through the machine. The time of operation of an electromagnetic unit is a few thousandths of a second and that of an electronic unit is a few millionths of a second. The machine is so arranged that an over-all balance is maintained between the various parts. For example, a multiplication requires about 20 milliseconds (0.02 second) in the electronic unit; about the same time is required for the electromagnetic units which transfer the data into and out of the electronic multiplying unit.

The entire operation of the machine depends upon whether a current flows in a given circuit at a given time. The current or absence of current in one or more circuits in turn is used to determine whether current will flow in a third, etc. Great numbers of such circuits are operated in sequence and in parallel to give the required results. It is the efficient design and combination of these many circuits that determine the effectiveness of a calculator.

As stated previously, many of the machine operations are performed by means of electromagnets (Fig. 4). When a current flows through the coil of an electromagnet, the movable armature is attracted and moves toward the coil. By means of this device a current in a circuit can be used to inaugurate an operation such as throwing a switch in a second circuit, punching a hole in a card or in a paper tape, or printing a digit on a sheet of paper. In the Selective Sequence Calculator, 180 such small magnets are used to control the printing, 160 for punching cards, 240 for punching tapes, and 25,000 for throwing switches.

In the switching device operated by an electromagnet (a relay), the movement of the armature separates one pair of contacts and connects another pair, thus causing current to cease flowing in one circuit and to flow in another. By connecting three relays as shown in Figure 4 (lower diagram), it is possible to select 1 channel from 4 possible channels; similarly, 7 relays will permit selection from

Fig. 3. A section of the arithmetical unit showing arrangement of electronic tubes that perform the basic counting operations of the Calculator.

8 channels, and so on. A single magnet may operate as many as 12 independent circuits simultaneously. In these relays the movement of the armature is only a few hundredths of an inch, and the time required for operation is only a few thousandths of a second.

Information can be read from a card or tape by means of punched holes which permit the current to flow in the associated circuits. The card or tape acts as an insulator between a wire brush and a metal roller, and contact is made through the hole in the card (Fig. 5). The presence or absence of a hole thus determines whether the current will flow when that circuit is tested.

For circuits where high speed or small current is required, the familiar electronic tube is the basic element. These tubes, or "valves," as they are called in Great Britain, are capable of switching a current off and on in the order of a millionth of a second. Such a tube consists of three elements: a glowing filament which emits a shower of electrons, a metallic plate toward which the electrons travel, and a screen, or grid, capable of interrupting the stream (Fig. 6). When the stream of electrons strikes the plate, it causes a current to flow in a circuit connected to the plate. When a voltage is applied to the grid through another circuit, the stream of electrons is prohibited from reaching the plate and the current is interrupted. By proper design of the circuit such a tube may be used not only as a switching device but as an amplifier where the current so controlled is greater than that required to control it.

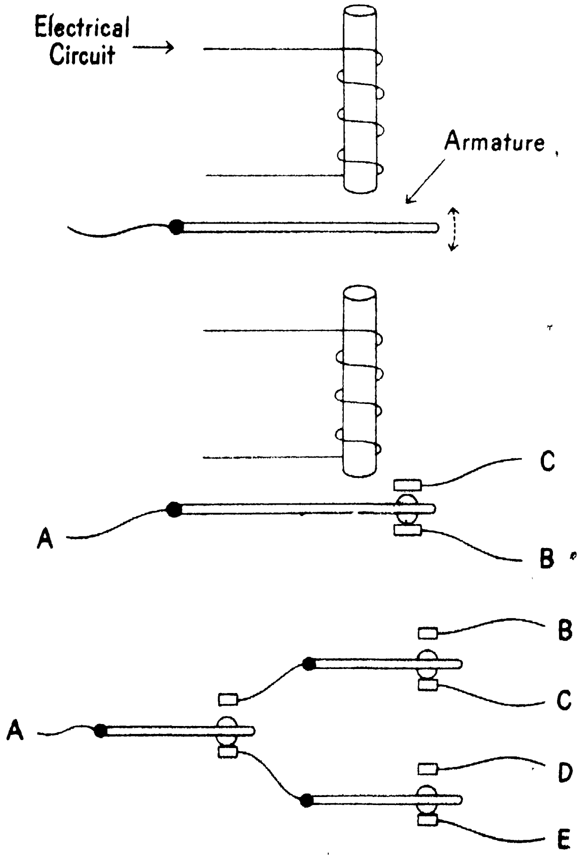


Fig. 4. Electromagnet (*upper diagram*). Armature moves up when current flows in circuit. Relay (*center*). Movement of armature breaks circuit A → B and makes circuit A → C. Three relays give choice of four circuits (*lower diagram*).

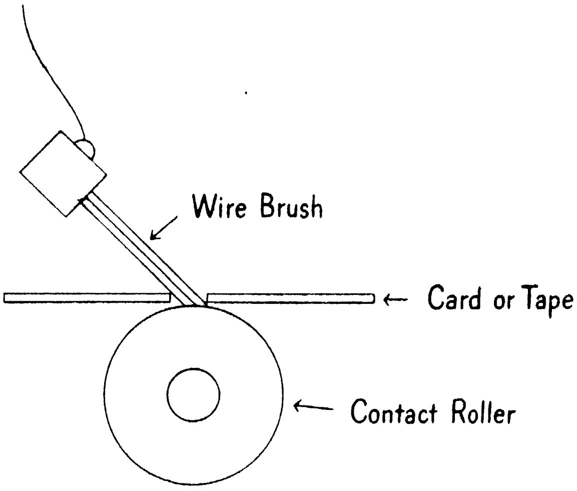


Fig. 5. Wire brush makes contact with roller through punched hole.

An ingenious electronic circuit commonly used in calculation is the so-called "flip-flop," which involves two "valves" arranged so that the current from the plate of one will operate the grid of the other (Fig. 7). This combination has the property that current always flows in one circuit and not in the other, and a slight pulse from an outside source will cause the condition to be reversed.

We come now to the assembly of the basic elements (punched-hole readers, printers, punches, relays, electronic valves, and flip-flops) into an integrated machine that will perform the operations outlined.

The initial data and basic instructions are transcribed into punched cards by means of an auxiliary device whose operation is controlled by a keyboard similar to that of a typewriter. If the problem is extensive great quantities of data and instructions will be required, and automatic methods will be employed also for checking, arranging, duplicating, listing, and editing the material. The machines used for this purpose are standard punched-card units such as are used in accounting and will not be described here.

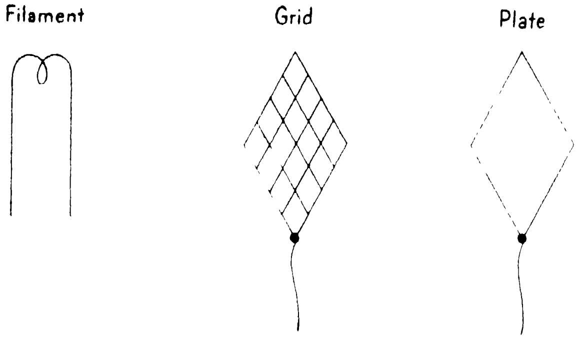


Fig. 6. Electronic "valve." Flow of current from plate is determined by voltage on grid.

Figure 8 shows a sample instruction card. The punching on the right-hand side of the card is the instruction data that will be read into the machine; that on the left-hand side is in the nature of shorthand notes for the guidance of the coder in examining or rearranging the cards. The pencil notes were written on the card before it was punched and the figures at the top were placed there by a machine which reads the holes and prints the corresponding characters. In the instructions to the Calculator the quantities *P*, *Q*, and *R* are the numbers of the three memory units involved in the operation; *OP* specifies the operation to be performed; *Shift* controls the position of the figures in the result; and *Seq* tells where to read the next command. *T*, *U*, and *V* pertain to a second instruction and correspond to *P*, *Q*, and *R*.

The card readers on the Calculator (Fig. 1) sense the holes in the cards by means of wire brushes, and the corresponding information is stored in the appropriate memory units. Each card reader has 80 brushes and is capable of reading as many as 80 digits from each card. Cards are fed at the rate of 200 per minute, totaling 32,000 digits per minute from the two readers. Electronic tubes are used in the reading circuits to reduce the necessary amount of current at the brushes.

The memory units of the machine are of three types: electronic, electromagnetic, and punched paper tape. In all three types the quantity stored is a simple "yes–no" condition. In the electronic storage the basic element is a flip-flop circuit, in the electromagnetic storage it is a relay which can be up or down, and in the paper tapes it is a "hole" or "no-hole." In order to save equipment, only four distinct digits are stored; the others are obtained as combinations. The code used is the so-called binary code: 1, 2, 4, 8. The four elements, taken one, two, or three at a time, will give all the digits from 1 to 9.

The electronic memory has capacity for eight 20-digit numbers and is closely associated with the arithmetical unit. This storage, with recovery time of less than a thousandth of a second, permits the performance of small sequences of operations independently of the electromagnetic part of the machine.

The electromagnetic storage provides for 150 20-digit numbers. These numbers are available in any order, and the selection time is equal to the time of a multiplication, or 20 milliseconds. Eight different routes are provided for transferring numbers from this storage to the electronic units to permit simultaneous transfer.

Storage for 20,000 20-digit numbers is provided in the form of punched tapes, each line on a tape corresponding to one number (Fig. 9). There are 3 punching units and 66 reading units; the punching, selection, and moving of the tapes are all controlled electromagnetically. Selection of any one of the tape readers is equivalent to selection from electromagnetic storage (0.02 second). Punching in each of these units requires about 40 milliseconds. The tapes may be moved at the rate of 20 milliseconds per line; i.e., 1,000 digits per second passing a given reader.

Thirty tape-reading units are associated with the 3 punches, so that a number recorded by the punch may be read back later as it passes the successive reading stations. Each punch is followed by 10 reading stations, or the tape from the first

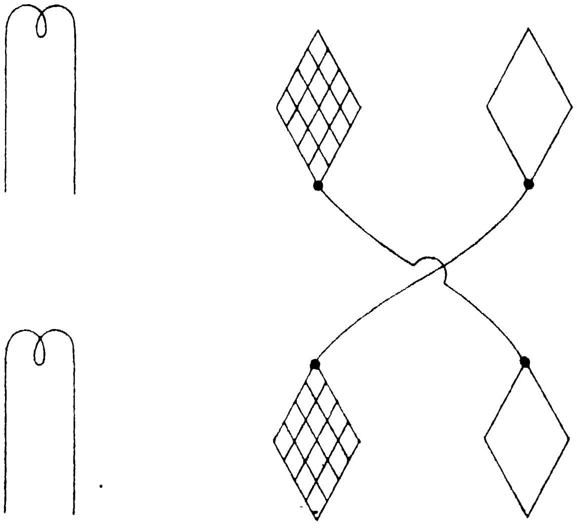


Fig. 7. Electronic "flip-flop."

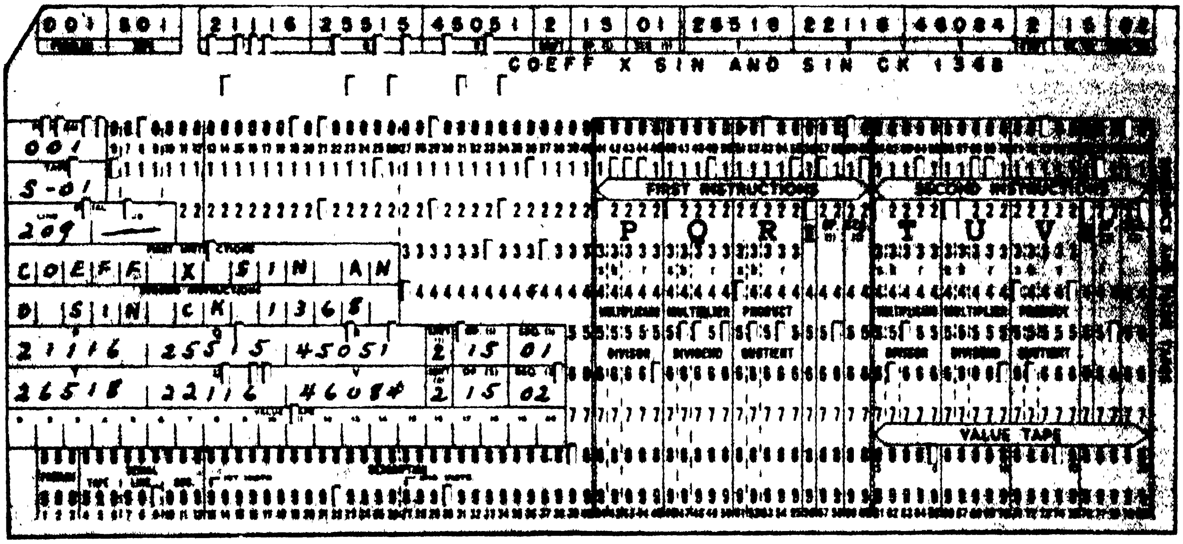


Fig. 8. Numbers that enter into computations and those that direct the operations of the Calculator are read from punched cards.

punch may pass to all 30 reading stations. Each reader can handle either continuous tape or closed loops (Fig. 10).

Thirty-six other tape readers are associated with a searching mechanism, so that any desired data may be quickly located. This unit has a capacity of 100,000 digits, and a complete search may be made in an average time of 1 second. This unit is used for consulting large mathematical tables containing such data as sines, exponentials, cube roots, etc. In all cases the machine computes a quantity for which a given entry in the table is required. In this unit a table with arbitrary intervals of the argument may be used.

We have already stated that the operations of addition, subtraction, multiplication, division, and column shift are performed electronically. These operations are accomplished by the proper combination of switching circuits and flip-flops. The flip-flop can be arranged so that successive pulses over the same wire will cause it to flip and flop alternately. A combination of such units serves as a counting device for counting a succession of pulses. The first pulse causes the first device to flip; the second pulse causes it to flop and also to flip a second unit, and so on.

Obviously if a device will count, it can add. If it can add, it can multiply by repeated additions. By proper combinations of such devices all the arithmetic operations, as well as switching and memory, are possible. All these operations are performed at great speed and with no moving parts.

The printing units have an output of 30,000 digits per minute. The records produced are for permanent reference and also for inspection as the calculation progresses. Since the machine is fast and is used for research work where it is not possible to foresee all contingencies, it is essential that the scientist be able to follow the work closely and modify his instructions in the light of the results as they appear. Recording on punched cards, at the rate of 16,000 digits per minute, supplies additional unlimited storage if the 400,000 digits in the machine are inadequate. It also provides data in a form for further processing or publication by mechanical means.

The natural questions at this stage are: "What part does such a calculator play in science?" "What is the nature of the problems which will be put on it?" and "How can the work be planned to take advantage of such great speed and capacity?"

In all fields of science, physical and social, pure and applied, it is necessary to make observations and to fit them into a theory. In order to increase his powers of perception the observer uses instruments such as telescopes, microscopes, thermometers, and galvanometers. Similarly, in order to increase the effectiveness of his reasoning powers, man requires mechanical devices. The classical example of this need is in the application of Newton's laws of mechanics and of gravitation. It is possible to state these laws in a few sentences, but their application to the various crucial cases has required the greatest efforts of mathematicians and astronomers for a century and a half, and the work already done is just a beginning. In every field of science there are similar problems.

To a great extent it is possible to substitute one for the other: mathematical theory, computation, observation. For example, in designing a new airplane three approaches may be used: predict analytically what will happen, predict computationally what will happen, build it and try it. The ability to make extensive calculations quickly and efficiently will advance all phases of science.

The following is a very simple example of how a problem can be put on the machine. This problem requires only a negligible part of the storage facilities of the machine, but it illustrates the exercise of choice at various stages. In the computation of the electron field of an atom, the solution of the necessary differential equations can be obtained by a numerical process which builds a table of numbers representing the desired quantities. The computation proceeds from one line to the next, each depending upon the previous line. In each line there will be five quantities: the number of the line, n, a precomputed number, q, for that line, and three quantities, x, y, z, which are to be computed for each line.

n	q	x	y	z
1	. . .	. . .	. . .	. . .
2	. . .		. .	. . .
3	. . .	. .	. . .	. . .
	. .	.	. .	. . .
.	. . .	.	. .	. . .
.	. . .	. .	. . .	. . .

The formulas are such that when x, y, and z for a given line have been found the value of x for the next line can be computed; when the value of x in a given line is available the values of y and z in that line can be computed. The computation is thus performed by the use of two alternate procedures. The formula for computing x is

$$x \text{ (new line)} = 2\,x \text{ (previous line)} + y \text{ (previous line)} - x \text{ (second line previous)} \qquad (1).$$

The formulas for computing y and z from x and q are

$$z = x + 1/12y \qquad (2);$$

$$y = (1/4 + q\sqrt{z})\,q\,z \qquad (3).$$

Equations (2) and (3) are solved by successive approximations: by neglecting y in equation (2) an approximate value of z is obtained; this is substituted in equation (3) and an approximate value of y is obtained; this is substituted in equation (2) to obtain an improved value of z. The process must be continued until the two equations are satisfied to the required accuracy.

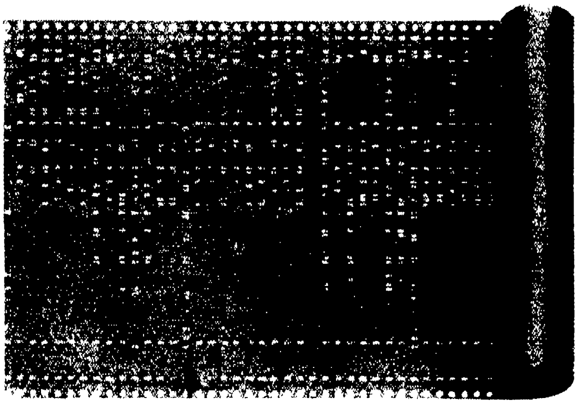


Fig. 9. **Punched tape. One line (vertical) permits storage of 19-digit number.**

When this line has been completed the same procedure is followed for the next line. The table is to be extended until the value of z satisfies one of two conditions: it becomes greater than a specified value, or two successive values bear a prescribed relation to each other.

The programming of the Calculator for this problem is as follows: the instructions for carrying out a cycle of the solution of equations (2) and (3) are contained in a loop of tape so that one revolution of the tape on the reader corresponds to one approximation. At the conclusion of a revolution of the tape the new approximation to z is compared with the previous value. If the agreement is within the prescribed tolerance the cycle is not repeated; if it is not within the required tolerance another cycle is performed.

At the completion of the sequence of approximations the resulting value of z is tested for compliance with one of the two conditions for ending the table. If the stopping point has not been reached a new value of x is computed and then another y and z.

The square root operation necessary for equation (3) was not listed as one of the "built-in" operations of the machine. A subroutine for this operation is therefore made up on a loop of control tape and operated in much the same manner as that for the solution of equations (2) and (3). This loop of tape instructs the machine to take an approximate value of the square root and improve it, the first approximation being the value used in the last previous computation. At the completion of each cycle the result is compared with the previous value and the process repeated until the required precision is obtained.

The above procedures usually required about 60 lines of the table, involving about 7,000 lines of instructions, of which 24 were written out; 5,000 multiplications; 500 divisions; and 9,000 additions and subtractions. They are completed in 5 minutes. A considerable number of integrations were performed for different starting values, the new starting values in each case being determined by a quick inspection of the previous record.

In extending the computation to the diatomic molecule it will be necessary to solve a partial differential equation which will require the full capacity of the machine. In the above problem it was necessary to operate on only a few lines of the table at a time; in the next case it will be necessary to consider a two-dimensional array of numbers which will run into the thousands.

Another problem that has been put on the machine is that of computing the position of the moon for any time, past or future. The equations are read into the machine, and when a given date is supplied the machine computes and checks the required results. For this computation an eight-place sine table is used. At one stage of the computation a small table is read into the electromagnetic memory, used 800 times in 3 minutes, and erased to permit use of the memory for other parts of the calculation. A summary of the data and operations involved in computing and checking one position of the moon is given in Table 1.

TABLE 1

Digits in basic input data and instructions	165,000
Additions and subtractions	10,710
Multiplications	8,680
Table look-up operations	1,870
Lines of sequence instructions required	1,170
Lines of sequence performed by calculator	10,350
Time required	7 minutes

Calculating devices are usually classified as "digital" and "nondigital," according to whether the basic operation depends upon the counting of discrete units or the measuring of physical quantities. The machine we have just discussed is of the digital variety.

Familiar nondigital devices include the slide rule, the planimeter, and computing gun sights. Some of the latter devices are large and elaborate. Among the large fixed computers are the "Network Analyzers" used in computing performance of power networks, and the "Differential Analyzers" at Massachusetts Institute of Technology and other places, which are used in the solution of differential equations. Digital calculators include adding machines, desk calculators, keyboard accounting machines, and punched-card computing and accounting machines.

Until the past few years practically all digital calculators operated by means of the adding wheel. In all cases except one these wheels are actuated mechanically; in IBM machines they are operated by means of electromagnets. The IBM Automatic Sequence Controlled Calculator at Harvard University (1944) contains nearly 2,000 such wheels.

During the past decade a number of large calculators have been constructed using the relay as a basic unit. These include several built by Bell Telephone Laboratories and by IBM; another has just been completed at Harvard. The largest previous electronic digital calculator, known as the ENIAC, was built by the University of Pennsylvania. This calculator has an internal memory capacity of about 200 digits, and its operation is controlled by means of pluggable connections.

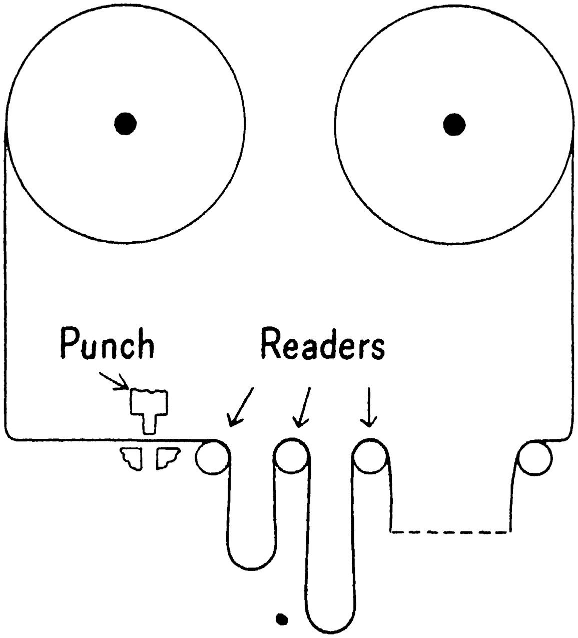


Fig. 10. Schematic diagram of tape memory unit. Numbers are punched in tape and read back at successive readers.

At the present time a great deal of research is being devoted to the further development of electronic calculators, particularly in the development of the memory element. Various devices have been proposed for this purpose, such as the supersonic delay line; the electrostatic tube; magnetic drums, tapes, and wires; and photographic film.

The supersonic delay line usually consists of a column of mercury so arranged that vibrations set up in one end will travel to the other end and be detected. As they are received by the detector they can be used or recirculated repeatedly until needed. The electrostatic memory device is somewhat analogous to the projection tube in a television set. A stream of electrons impinges on a surface and produces small charged spots that can be detected later. Magnetic drums, tapes, and wires employ small magnetized spots on a metal surface. The method is similar to that used in magnetic sound recording. Small spots on a photographic film with photoelectric detection is another type.

We have seen how the Selective Sequence Calculator was designed to save equipment by coding each decimal digit as a combination of the binary digits, 1, 2, 4, 8. Some further saving would be achieved by coding larger numbers in the same manner: 1, 2, 4, 8, 16, 32, . . . This saving in equipment must be considered in relation to the difficulties involved in converting large numbers from the decimal to the binary notation and vice versa.

* * *

PHYSICIST'S WINTER

Abruptly evening shrinks the capillary
Metal in its threadline, narrows sky
To pinpoints; ice-light races time
From source-eternal. Footprints in the slush—
Eutectic harden into molds with
Crisp and crumbling edges for our nightsteps,
Nearer to the door. No meadow wanderer
Seeks out a known-green world in such
Decreasing entropy; ice-wands
Cling brittle to a board, then stolidly
Clitter with falling, this being season
Blessed by a parallel-vectored wind
And notably devoid of calories.

HENRY A. HOFFMAN

WHAT IS NATURAL SELECTION?

S. J. HOLMES

Dr. Holmes (Ph.D., Chicago, 1897) is professor emeritus of zoology at the University of California, Berkeley. Well known for his studies in genetics and eugenics, he has been called "one of the deans of American biology." His latest book, Organic Form and Related Biological Problems, *was published this year by the University of California Press.*

IT MAY seem a strange anachronism in these days to be discussing the meaning of the term "natural selection." Everyone, of course, now knows what natural selection is—it is the survival of the fittest. Organisms vary. Some variations are better adapted to their conditions of life than others, and, on the average, the favorable ones are preserved and the others perish. This is natural selection. It is all very simple. Why, then, should anyone dwell further on so trite a topic?

It requires, however, no great familiarity with the vast literature on natural selection to realize that different people have interpreted this process in very different ways. Since the term natural selection was first employed by Charles Darwin, it would seem proper to accept the meaning originally given to it. In the fourth chapter of the first edition of the *Origin of Species,* Darwin stated that "This preservation of favorable variations and the rejection of injurious variations, I call Natural Selection." In later editions he added the phrase "or the Survival of the Fittest," which he adopted from Herbert Spencer. As thus defined, natural selection includes a twofold process or, rather, two sets of processes: those concerned with the preservation of certain variant individuals and those responsible for the destruction of others. And an essential feature of the whole series of activities is that the fate of the individuals involved is to a significant degree dependent upon what they inherit. Mortality having no relation to endowments, such as the destruction of all the inhabitants of an island that sank into the sea, would not come under the head of natural selection in the Darwinian sense.

As will be pointed out later, it makes a deal of difference in one's evaluation of natural selection as an evolutionary factor whether its preservative phase is included in the process. It has too often been asserted that natural selection is merely a negative factor, creating nothing, but only destroying those organisms that fail to meet the requirements of their environment. St. George Mivart, for instance, refers to natural selection as "an apparently positive name for a really negative effect, and is therefore an eminently misleading term. By 'Natural Selection' is meant the result of all the destructive agencies of Nature," thus ignoring completely its preservative influence, which is clearly stated in the original definition of the term. And E. D. Cope states that "nothing ever originated by natural selection. It is to the great causative forces as are the gutters and channels which conduct the water in comparison with the pump and the man who pumps it." A similar conception is not uncommon among the opponents of Darwin's theory, to whom it may have appealed, consciously or unconsciously, because it makes destructive criticism so very much easier. The real creative force in evolution, it is asserted, is afforded by those causes that give rise to new favorable variations without which it would not be possible for natural selection to make any advance.

But if we grant this contention it does not follow that natural selection is not a causal factor. Samuel Butler takes Darwin to task because he spoke of natural selection "as if it were an active cause (which he manifestly does)." Any kind of selection, natural, artificial, or sexual, is obviously a process or, rather, a series of processes unavoidably including a lot of sequences which can hardly escape being active causes of something. Accepting Darwin's definition that natural selection is the preservation of favorable variations and the destruction of unfavorable ones, it will prove instructive, I think, to carry out its logical implications and see where they land us. We shall find that we will be led to some conclusions that many writers on the subject have apparently never thought about before. The implications of this simple word "preservation" are really tremendous. Both the processes of preservation and destruction include the operation of numerous causes, internal and external. On its preservative side we cannot stop short of concluding that natural selection embraces the manifold physiological activities that preserve the life of the organism, and also the numerous formative processes of embryonic development. It may also include useful behavior reactions, resistance to disease, and the effects of many environmental conditions without which the organism could not persist. The preservation of favored variations necessarily includes all these activities, and more.

Nothing could be simpler than the general formula of natural selection. But when once we begin to inquire as to what is included in these processes of preservation and destruction in which natural selection consists, we encounter a vast number of factors whose complete analysis is a hopeless task. There can be no doubt that natural selection in the sense defined is actually operative in the organic world. It gives us a very plausible explanation, so far as it goes, of why certain peculiarities of organisms came to be as they are. This is an important achievement, even if it represents only a first step in the process of causal analysis. It profoundly affects our interpretation of the phenomena of adaptation, which is so conspicuously exemplified in the world of life. It has severely shaken the general confidence in the argument from design, which has long been defended for the consolation of the faithful and the discomfiture of the skeptic. Its theoretical bearing is especially important if the largely unknown causes in terms of which its explanations are made can be assumed to be natural as this term is commonly used. Darwin assumed that these causes are natural; his theory, he held, could get along under that assumption, and, as a matter of fact, we know that many of them are. It may be contended, of course, that some of them are not, and that they require the action of purposive agencies of some kind, but the burden of proof, as in other scientific problems, lies with the proponents of such a view.

If we include under natural selection the numerous causes that lead to the preservation of favorable variations, how far back should our series of events extend? Should it include the production of new variations? On this point Darwin makes the following remark: "Some have even imagined that natural selection induces variability, whereas it implies only the preservation of such variations as arise and are beneficial to the being under its conditions of life." We may assume, then, that the processes concerned with preserving or destroying a given individual, which are properly included under natural selection, are those that follow the origination of the new variation. As to why variations arise, Darwin had little to offer, but he regarded them as quite as natural ("given by nature") as any other event.

It has been customary to consider variation and selection as different processes that cooperate in effecting evolutionary changes. A closer scrutiny of these processes, however, gives rise to some interesting questions concerning the distinction, which may have to be decided in an arbitrary manner. Apparently the production of a new variation is something quite different from its survival. As we commonly picture the process, different variations present themselves somehow and some of them are preserved and others destroyed. But if a variation appears it obviously must be produced. If it is produced, it must have been preserved, at least up to a certain point. If it continues to survive while others perish, the formative activities that lead to its being different (a new variation) are but a part of the preservative activities that enable it to live. The common conception of a new variation as something given in its entirety independently of natural selection, and upon which natural selection may work, is quite indefensible, because without the preservative activities that constitute natural selection the new variant organism could not appear at all. Even a variation that is eliminated must be preserved by natural selection for a time at least. If on account of some lethal factor it is destroyed at an early embryonic stage, the integrative activities which are at first successful are overcome by destructive factors which later prevail. Natural selection is both Vishnu, the Preserver, and Siva, the Destroyer. Some organisms may develop no further than a germ cell. From the standpoint of the species the fortunate ones are those that are able to persist through the reproductive period of life. Every new variation that gets beyond the stage of a modified germ cell, therefore, requires the aid of Vishnu if it reaches any particular period of development.

The new variations presented for the operation of natural selection are those in which a germinal change of some sort is but one factor in its formation among many others essential for its preservation. If a germinal modification is preserved to following generations, it takes its place among the complex of conservative genetic factors that perpetuate the successful stock. Then its function comes under the category of natural selection, and its influence helps to determine whether a subsequent new variation will survive or perish.

When we attempt to abstract and set apart the processes of variation and selection we find that they are very intimately associated. The only way in which we can hope to make a sharp distinction between them is by limiting the former to the germinal change which, working with other influences, eventuates in a different kind of organism. Throughout evolutionary literature, from Darwin down, variations have been assumed to constitute the precondition for selection. But it is only the germinal change that antedates selection. This change may, of course, be essential to survival, or, again, it may lead to a fatal result.

In the process of abstraction by which variation has been sharply set apart from selection, a variation has commonly been conceived as a more or less completed product, such as an Ancon sheep or a runnerless strawberry. In this procedure the fact that has been generally overlooked is that what has been abstracted from selection (the production of a variant organism) is in large part the effect of selection. The abstraction is only partial, but many of the arguments based on it have proceeded on the assumption that it is complete. As commonly conceived, what is presented somehow for natural selection to work on is a definitely formed organism, such as a long-bodied sheep, without reflecting that without the preservative action of natural selection such a variation could not appear.

For a long time (until the time of Weismann) germinal variations were, as a rule, not clearly distinguished from those occurring in the soma. Darwin was much less explicit on this matter than he might have been, but even after this distinction was clearly recognized, writers continued to speak of the production of a variant organism as quite different from the processes of selection by which it is preserved.

We should always bear in mind that natural selection is a name for a group of processes that make for the preservation of an organism or its destruction as a result of its inherited constitution. It is not mere survival or extinction considered as a terminal event. The preservation of favored races necessarily includes very numerous activities involving a multitude of causes. And it is important to consider it from this dynamic standpoint and what it implies in any discussion of what natural selection can and cannot do. Natural selection is just as creative as the work of the selective breeder. Both attain their results through the preservation of variations that are good enough to appear; and natural selection must cooperate with the breeder if he creates a desired product. If his chosen candidates for parenthood fail to maintain themselves or do not behave in the normal, expected way, he can accomplish nothing. The breeder is just one environmental factor which, like many others, determines the kinds of variations that survive.

The activities embraced under the term preservation are to a large extent the work of genes which at some time arose through mutation, or they are the product of some other kind of a germinal change. The new mutants that are perpetuated must be, in a sense, selected by the conservative outfit of factors with which they have to get along effectively if they become incorporated as integral parts of the modified aggregate. We are thus led to consider an aspect of the action of natural selection which has been generally overlooked or ignored, especially in the older literature. It is the fact that selection has operated through the preservation of those variations in genetic factors which fit them for acting in harmonious social relationships. As I have elsewhere contended (*Organic Form and Related Biological Problems.* Berkeley: Univ. of Calif. Press, 1948, 11, 27, 28, 33, 34), the fact that natural selection has been so much concerned with the reaction modes involved in formative processes that concur in the production of a living organism helps us to understand many apparently purposive features of organic behavior which have frequently been adduced in support of various brands of vitalistic theory. From this viewpoint the ability of organisms to make appropriate direct responses to varied situations becomes a much less formidable difficulty to the naturalistic biologist than it formerly appeared.

A very large proportion of organisms are eliminated because they do not possess sufficient intrinsic viability to survive under the most favorable of all possible environments. The large number of mutations which are lethal at one stage or another affords strong evidence for this conclusion. The elimination of an amaurotic idiot or a homozygous yellow mouse is not a consequence of any failure to respond properly to some environmental influence, but is a consequence of the disharmonies of its inner formative mechanisms. Life is not only the continuous adjustment of internal relations to external relations, as it was defined by Spencer, but the adjustment of internal relations to other internal relations. If any organism is to survive, therefore, its genes must be selected in a very special manner.

Since the new germinal variations that are added have to fit into the existing complex of genetic factors, this complex may be said to select its members after the fashion of an exclusive club. And the club to which the genom may be compared blackballs the great majority of its candidates for admission. The survival of the organism must depend primarily on the aptitude of its members for getting on well together. The groups in which the constituents behave at cross-purposes quickly go into the discard. The novelties that result in improvement are, as we must expect, relatively rare. The self-perpetuating assemblage of genetic factors is mostly a well-orderd body whose members for the most part cooperate most admirably to promote the common weal. Government, as in societies of insects, seems, on the whole, to be on a democratic basis, which, after all, is the organismic method of regulation.

The origin of new germinal variations includes, of course, other happenings besides changes in individual genes. There are variations due to an increase or decrease of chromosome numbers, duplications of parts, translocations, and various other unusual procedures that give rise to transmissible modifications which may result in elimination or, much more rarely, in the production of a superior strain. The causes of these changes, like those of gene mutations, are multifarious, but it is a significant fact that a number of causes, including X-rays, etc., are productive alike of both classes of hereditary variations.

As before stated, Darwin distinguished sharply between the production of variations and the selective processes which decide whether a given variation survives or perishes. This distinction seems so very obvious because new variations appeared as unaccountable happenings which afford the condition for subsequent change. The new light which has been thrown upon the nature and the different kinds of variations, on the one hand, and what is known of the numerous procedures involved in the preservation of favorable varieties and the destruction of misfits, on the other, give us a concept of the relation between variation and selection that in some respects differs from that in the mind of Darwin.

To the extent that one is a Lamarckian, as Darwin was to a mild degree, he would not be justified in concluding that natural selection is unable to cause variability. This is because somatic modifications, according to Darwin's definition, must fall under natural selection, if they tend to preserve the organism. So far as these modifications are transmitted to following generations, thus far natural selection must be regarded as a cause of variability.

But, when we consider the topic from the standpoint of the orthodox neo-Darwinian, the distinction between variation and selection becomes less obvious than it was formerly held to be. Among the happenings that take place in the normal life of an organism, there are marked changes in gene activity, changes in the number of genes, and changes in plastids and other possible gene derivatives. Presumably we should include under variation the occurrence of mosaics, bud variations, and mutable genes, but there are other happenings concerning which there is more uncertainty as to the heading under which they should be classed. Whether the origin of a malignant cell is due to a somatic mutation is still a matter of dispute. There is, as yet, no unanimity of opinion on the nature of viruses. It has been conjectured that they are free genes, and again that they are modified descendants of genes, or perhaps in some other way products of the organism's own metabolism Then there are the Dauermodifications, which may give rise to different competing strains. If these should turn out to be dependent on cytoplasmic changes similar to those occurring in the specification of cells in normal development, the distinction between variation and selection would perhaps have to be drawn on a purely arbitrary basis. There are plenty of discrete, mutationlike changes in the organism during its formation. To be consistent, we should have to class all true gene mutations or chromosome aberrations as variations even if they affected no more than a single cell. Whether such variations play at times an indispensable role in normal development, we cannot be certain. Quite evidently, however, they may cause the elimination of the organisms in which they occur.

In the stream of events which constitute the process of organic evolution there are, in addition to the preservation or destruction involved in selection and the origination of new germinal variations, a third category of phenomena, which are indifferent in that they have no relation to survival or extinction. Their production, therefore, is not directly attributable to natural selection, although they may often be incidental consequences of the formation of useful characteristics. They have often been adduced as constituting a serious difficulty for Darwin's theory. But inasmuch as most heritable variations are bad or indifferent, their accumulation within limits is nothing to be wondered at. And where species produce many times as many progeny as can possibly survive, they may be able to carry a considerable burden of useless characteristics.

When we consider the ways in which writers on evolution evaluate the relative importance of selection and the causation of variability, we encounter a great range of opinion. As to the causes of variation, the divergencies have been wide. There have been appeals to variation divinely initiated, to a perfecting principle, to an entelechy, to an *élan vital,* or some other factor working orthogenetically, to Lamarckian inheritance, and to various other alleged causes. There have been also very different degrees of importance ascribed to selection, from its all-sufficiency to a virtually negligible role, or even none. In the light of the greatly increased knowledge of variation and its different kinds which has resulted from the advances in genetics, it can be truly said, I think, that most of these views on the causes of variation have received no support. Those variations due to Mendelian segregation follow the same rules of chance exhibited in tossing pennies. Those caused by X-rays, ultraviolet radiation, heat, or bombardment with neutrons have the appearance of being almost equally fortuitous, although with a vastly greater range of possibilities. One cannot state, however, that they are completely indeterminate as to direction. But the whole picture of variability gained from observations of mutations arising spontaneously and those experimentally caused goes far toward justifying the application of the term "fortuitous" employed by Darwin.

Looking upon selection in the way I have described, it is evident that once a germinal variation has occurred it affects the perpetuation of all the genetic factors that produce the organism. Generation after generation new or modified genetic factors are added to the stirp, which builds up its own membership in ways that enable it to meet two different classes of requirements. One of these, for want of a better name, we may term "intrinsic viability." It requires a great deal of mutual adaptation of genetic factors to produce a living organism even under the most favorable conditions, and defects of genetic factors lead to an enormous amount of elimination. The other class of variations is, of course, those affecting, for better or for worse, the organism's reaction to its environment. These two classes are not entirely distinct, but are associated in various degrees of intimacy. The action of some formative factors may be almost completely independent of all environmental influence. A modified response to the environment, however, cannot be independent of formative factors for the simple reason that it is dependent upon them for its existence. The structure and reaction modes of such diverse creatures as an ant and an anteater are presumably the outcome of accumulating genetic variations which not only fitted the ancestors to meet the demands of their conditions of life, but also the intrinsic viability requirements of morphogenesis.

It may seem to many that I have employed the term natural selection in too wide a sense because I have included in it the multitudinous phenomena of development, the physiological activities of all parts of the organism, its behavior in relation to external objects and conditions, and the effects of the environment on the life of the individual. We do not usually think of selection as including all these elements. We look upon it, much as Darwin did, after the analogy of the breeder who picks out certain individuals and rejects others. But when we substitute nature for the selective breeder, we must take the logical consequences of our procedure. The term nature has a *very* comprehensive denotation. When we speak of the preservation of a favorable variation, we cannot limit this process to a single act comparable to the choice of a breeder. We are logically driven to include all the stages involved in its production and all the processes responsible for each. When, therefore, we accept Darwin's definition of natural selection, we are using the term to cover a great multitude of causes in so far as they may properly be defined as natural.

In setting forth some rather obvious though not frequently discussed implications of the theory of natural selection as defined by Darwin, I have not, I hope, been dealing with a topic devoid of any theoretical bearing. If we include under selection the various natural processes I have indicated, it is impossible to maintain that natural selection is a purely negative influence. Without variation it may not create a new species. Neither can variation without the preservative action of natural selection. The preservative activities of morphogenesis are certainly creative even when they do not create anything new. But within limits they create new forms and new adaptive forms, although they may not be new genetically. All the adaptive activities of any organism after it has received its complement of genetic factors must be credited to the function of preservation. The nature of these adaptive responses may be looked upon as the outcome of previous genetic variations. Variations condition the direction followed by evolution; the organism determines what variation fits in with it in a way that leads to survival. Out of the multitude of different possible variations it selects those which are congruous with its own constitution not only in relation to intrinsic viability, but in relation to making adjustments to the environment. A tendency for variations to accumulate in the same general direction would seem to be a natural consequence of this fact.

It is no reproach to Darwin's theory that it does not account for the origin of variability. That variations occur in abundance, that their origin is presumably natural, and that they occur in very numerous directions and can be accumulated by selective breeding were facts that afforded a sufficient basis for his theory. Darwin believed that variability might be induced by the environment (nutrition, temperature, etc.) and to a certain extent as a result of Lamarckian inheritance, but for the chief purposes of his theory he could afford to take variations as "given by nature."

The attempt to throw light upon the mechanisms concerned in natural selection brings us at once into the fields of biochemistry, physiology, experimental embryology, and other branches of physical and biological science, and at times into psychology and the social sciences. We thus become confronted with a great variety of problems requiring special applications of principles belonging to many disciplines. Natural selection is confessedly a figurative expression based upon an analogy with the work of a selective breeder, but with the breeder replaced by nature in all her manifold complexity.

It is true that when we apply the simple selection formula to explain any feature of evolutionary change, we have usually done little more than to attribute it to natural causes. In Darwin's time this was an important achievement, for it substituted explanation in terms of natural law for supernatural intervention. If natural selection makes only the first step in the direction of a complete explanation, it directs the efforts at further analysis into two fields, the search for the causes of variability, and the study of the forces that lead to the preservation or destruction of variant types. In both these fields investigation has yielded a large body of significant facts. The discoveries that have been made concerning the nature of different kinds of variation and the agencies by which new variations may be produced have been of particular importance for our views on the method of evolution. By many writers the causes of variation have been stressed as the all-important problem for evolutionary theory. The idea that variations occur with little regard to direction, as if they were mostly fortuitous happenings, has met with vigorous and widespread opposition. That variations can arise by chance or, in other words, that they result from so large a number of causes that their nature and direction cannot be predicted, has been looked upon as an especially shocking conclusion. Out of such a chaos of hit-or-miss happenings an orderly, progressive evolution, it was alleged, could not possibly occur. Hence, the many efforts to find some internal source of variability that proceeds consistently along definite lines, such as Nägeli's perfecting principle, Fechner's stability principle, the *élan vital*, and various others. These alleged causes of variability are little more than names for what it is that causes variation to be produced in definite directions. As to the specific mode of producing their effect, they are peculiarly unenlightening. They are generalizations drawn almost exclusively, not from experiments on the actual production of variations, or from observations on the ways in which variations occur, but from inferences on the ways in which they must have occurred to produce the line of development they have been assumed to follow. From the standpoint of these theories the direction of evolution has been determined mainly by whatever it is that causes variation to follow consistently a particular course, natural selection being relegated to the role of eliminating the misfits. According to the Darwinian viewpoint, since variations occur in a large number of directions, the course of evolution is guided chiefly by natural selection. So far as experiments have thus far thrown light on the causes of hereditary variations, and so far as observations have gone as to the many kinds of variations that appear, the preponderance of evidence at present available is, I believe, distinctly in favor of this view. If there are any forces slowly acting to produce evolutionary changes along predetermined lines, their existence has not yet been revealed.

As I have pictured the intimately related processes of variation and selection, using the term variation as a change in the genetic factors that results in a modified organism, it is obvious that such changes are essential for the production of new types by natural selection. At the same time, natural selection is essential if the modified germ plasm produces an organism at all. Probably natural selection has not usually been looked upon in this way simply because people have not reflected upon what is involved in the "preservation of favorable individuals or variations." After all, the importance of germinal changes in evolution is not so much how they are caused as what they do afterward. When they lead to survival and become more or less permanent constituents of the stirp, their functioning becomes an integral part of the preservative activities that constitute natural selection. In fact, a mutant gene becomes an instrument of natural selection as soon as it begins to act.

For the purposes of Darwin's theory, variations, whatever the means may have been by which they were "given by nature," could always be made use of by natural selection. Where many different kinds of germinal variations are produced, evolution is free to follow a large number of different lines. The almost inexhaustible varieties of structural pattern that have been formed and the multitudinous adaptations that occur in relation to specific external conditions, both inorganic and organic, afford convincing evidence that the course of evolution has ever followed the beck and call of opportunity. For this to have been possible a great diversity of germinal variations must have been repeatedly produced, however they may have been caused.

◆

LEAF HOPPER STYLETS IN SUGAR-BEET PETIOLE

THIS INTERESTING PHOTOGRAPH WON HONORABLE MENTION FOR CHARLES F. LACKEY, PLANT PATHOLOGIST, USDA, RIVERSIDE, CALIFORNIA, IN THE BLACK-AND-WHITE DIVISION, 1948 INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON.

EPIDEMICS AND BACTERIOLOGICAL WARFARE

R. L. MAYER

A native of Colmar, France, Dr. Mayer studied medicine and chemistry at the Universities of Strasbourg and Freiburg. Prior to coming to the United States, he was engaged in research in allergy and chemotherapy. He is at present chief microbiologist in the Research Department, Ciba Pharmaceutical Products, Inc., Summit, New Jersey.

SCIENTIFIC research has two primary objectives: one highly theoretical, the other highly practical. The theoretical objective concerns man's constant efforts to penetrate the mysteries of life and to understand nature; the practical objective is directed toward the improvement of our living conditions on this earth. Scientific research approaches both objectives at ever-increasing speed. The extent of scientific progress and the improvement in our standard of living and social structure in the past few hundred years as a result of the understanding of the laws of nature, have truly been amazing. Unfortunately, besides these highly beneficial, social implications of science, many negative implications follow or even precede progress—and these too occur with increasing speed. Indeed, science continually discloses not only many beneficial results, but also many others that are dangerous for mankind. Such results, when applied by the wrong people or in the wrong way, may disturb or even disrupt our social structure.

The obvious example of a scientific advance which constitutes a great achievement but which also has destructive social implications is the atomic bomb. The average person is convinced that this newest scientific discovery, the domestication of atomic energy, will be used mainly for destruction, although many scientists maintain that in the near future atomic energy will pay considerable social dividends to mankind. With the introduction of atomic warfare we have suddenly realized that the social implications of science are indeed very complex; today they have become more or less synonymous with disastrous consequences.

Another branch of science which will soon be contributing a further development with destructive social implications is bacteriology, the science of the minute organisms, billions of which live within and upon us. Many of these single-celled organisms are indispensable for the maintenance of our very lives, and others are our greatest enemies. In the past 80 or 90 years mankind has profited tremendously from research in the field of bacteriology, as is apparent from a few examples. Smallpox, which not so very long ago was one of the major calamities of the world and which all but destroyed entire populations, has been almost entirely eradicated since the introduction of universal vaccination. Malaria and yellow fever, both of which prohibited the settlement of many parts of the world and once were serious threats in our own South, have almost disappeared from many regions. Because of our victory over malaria, many subtropical and tropical regions are now habitable and have become important agricultural and industrial centers.

The most spectacular result of modern medicine is the prolongation of human life during the past 45 years. In 1900, life expectancy in the United States was only forty-nine years; in 1945, it had become sixty-six years. This great achievement has been made possible mainly through cooperation in the various fields of medicine, among which bacteriology and hygiene, a child of bacteriology, have been important. Yet in spite of its brilliant record of achievements, bacteriology is on its way to furnish, as has atomic energy, a means of mass destruction and thus a major threat to humanity. Along with atomic warfare, we are on the eve of bacterial warfare.

Atomic warfare has already demonstrated its potentialities, but bacterial warfare—that is, war using pathogenic bacteria as major weapons—has never been waged on a grand scale. We therefore can judge its practical importance and danger only from theoretical considerations and to a very large extent from our own imagination. During our nearly 100 years of bacteriologic research, however, many facts have been discovered which, in the opinion of many scientists, indicate that bacterial warfare, when properly developed, can become a weapon of scope and potentiality comparable only to the weapons of atomic warfare. There are, on the contrary, other scientists who disagree with this opinion and who do not believe that bacterial warfare will ever become a practical weapon. Let us hope that we shall never have to decide who is correct—the optimist or the pessimist.

The concept of global bacterial warfare is of very recent date. It was first discussed in the European literature in the late nineteen twenties—about 10 years before World War II started, and it is not surprising that the first articles appeared principally in the French medical literature. At that time it became clear to many that sooner or later Germany would initiate another war. Since the Germans had always prepared for each of their wars with the greatest scientific precision and for each of their wars had invented new and more powerful weapons, the French were fearful that this time they would become victims of new devices, perhaps of bacterial weapons. Actually, Germany never used bacterial weapons, but this is no reason to assume that she did not have them.

In these earlier French articles bacterial warfare was clearly defined and many theoretical possibilities discussed. In the meantime, systematic research, especially in this country, has been directed toward the problem of bacterial war, and much specific knowledge has been collected. Many vague ideas, opinions, and theories have taken definitive shape in the past 10 years.

Bacterial warfare, as with atomic warfare, was a secret military or government project in all combatant nations during the last war, and it still is; so far, very little factual material has been released. This discussion on bacterial warfare, therefore, is based solely on my personal opinion and on published material available to everyone.

The weapons of bacterial warfare are pathogenic, that is, disease-producing bacteria or other agents of infectious diseases not only of man, but also of animals and plants. Included in this category are the toxic products, or toxins, of these bacteria and certain synthetic plant poisons, especially plant hormones and their derivatives. All these microorganisms or poisonous substances are employed for the sole purpose of producing diseases that will incapacitate human beings or useful animals, or destroy or damage food crops and other useful plants. In bacterial warfare we make subservient microorganisms that in their own natural way have waged war against mankind; we master them in the same way we have harnessed the electricity which, before Edison, razed our buildings, forests, and crops.

Although the microbial armies that attack man, animals, or plants are composed of microscopic organisms, their small size is compensated by their astronomical numbers and their great virulence. The effectiveness of any war weapon is measured in terms of its energy and destructive power. The explosion of an atomic bomb liberates fantastic amounts of energy; it produces millions of degrees of heat. Its destructive power can be measured by the extent of the demolition of Hiroshima or Nagasaki. But how great is the potential energy and destructive power of the biologic weapon, the bacterium or the virus? We know that certain bacilli cause diseases such as pneumonia, septicemia, tuberculosis, plague, cholera, and others and that they are capable of destroying our lives along with those of animals and plants. The magnitude of the power of microorganisms can be estimated by the epidemics and pandemics they bring about.

Let us illustrate this power with a few examples. Influenza is caused by a specific virus—a very minute microorganism. Influenza epidemics of varying intensities occur almost every winter, but every 30 or 40 years this disease assumes tremendous and catastrophic proportions. Many of us recall the great influenza pandemic of 1918–19, which probably had its start in an army training camp near Boston and which twice encircled the globe, missing only two small islands. This epidemic overtook 700,000,000 people, of whom 20,000,000 died. Within the fall months of 1918, the flu had killed more victims than died in the 4 years of World War I, in all armies put together. Few of us realize that the influenza epidemic of 1918 ranks with the Justinian Plague of the sixth century and the famous Black Death of the fourteenth century as one of the three most costly wars that microorganisms have waged against the human race.

The most impressive of all epidemics in history was the Black Death of the fourteenth century. This was an epidemic of plague, an acute infectious disease caused by the plague bacillus, carried by fleas that transfer the bacilli from rats to man. Plague occurs in two forms: the pneumonic plague, a pneumonialike disease; and the bubonic plague, the principal symptoms of which are inflammation and suppuration of the various lymph nodes. The course of both forms is extremely rapid and the mortality very high. Death occurs within three to five days.

The Black Death came to Europe from India or China, where it had been endemic for centuries, causing in these two countries, year after year, a constant but not too-alarming number of infections, with a relatively low percentage of deaths. The population of these regions had become immunized by the constant presence of the disease. When the plague spread for the first time to Europe, it found an entirely fertile terrain among the unimmunized and highly susceptible population. To speak in terms of war, plague fell upon an unprepared and untrained population. The mantle of Black Death first enveloped Europe in 1348; two thirds of the entire population were afflicted and almost all died. Exhausted for lack of victims, the disease smoldered endemically for 12 years until a new crop of victims had grown up. In 1361, in 1371, and again in 1382 the plague flared up, and by the time it finally died out in Europe it had destroyed one fourth of the population—at least 25,000,000 souls. In later centuries the plague returned many times to many countries, in 1665 destroying 100,000 people in England, 200,000 in Brandenburg, and 300,000 in Austria in 1711. More recently the plague waged its battle in India, where it killed in the 40 years between 1896 and 1936 more than 12,000,000 persons.

The Black Death was one of the greatest shocks mankind ever experienced. A few economists believe that the modern world has not yet completely recovered from the profound sociological changes produced by this epidemic. We have many accounts of the plague by eyewitnesses. One of them is De Chauliac, a famous surgeon of Montpellier, France, who, referring to the plague of 1348 in Avignon, wrote:

> The great pestilence was so contagious that men died without attendants and were buried without priests. The father did not visit his son, nor the son his father. Charity was dead and hope crushed. The plague was great because it covered the entire world and left scarcely a fourth-part of the people.

Another epidemic disease that has battled mankind in well-characterized campaigns is cholera. As a world disease, cholera seems to belong to the nineteenth century. In six separate epidemics it attacked various countries of Europe, and about 270,000 victims perished. In 1947, there was a new outbreak of cholera, thus far confined to Egypt, with 8,000 cases and about 3,500 deaths as of the end of 1947.

There have been many other famous epidemics, such as, for example, the English Sweat, which prevailed in England, France, and Germany during the sixteenth century, or the syphilis outbreak, which came to Europe in the sixteenth century as an acute, widespread epidemic.

For many years the causes of all these great epidemics remained mysteries, and it was only toward the end of the nineteenth century that bacteriologists discovered the bacilli responsible for epidemics, and hygienists explained their spread. In spite of intensive studies and world-wide research in epidemiology, many factors are still unknown, the study of which constitutes major problems in sanitation and hygiene. The penetration of the mysteries of the sudden outbreaks of a mass infection and of the development of epidemics and pandemics is not only an important task for the hygienist, who watches over the health of the whole population, but is one of the most basic problems of bacterial warfare. Should anyone consider the use of bacteria as weapons and the production at will of a mass infection, he must be familiar with all the facts governing the onset, spread, and extinction of epidemics. He must be able to master the epidemic, to guide it, to foresee its course, and to delimit its field of action. It is no small wonder that epidemiologists are among the key personnel in the research on bacterial warfare.

One of the mysteries of epidemics is what the French refer to as the *génie épidémique*, the soul of the epidemic. Many of the bacteria capable of causing infectious diseases and epidemics are ubiquitous. They are found everywhere—around us, on us, and even within us. The influenza virus, for instance, is present normally in the nasal passages of about 60 percent of all persons. Many of us are carriers of meningococci and pneumococci, or we harbor hemolytic streptococci in our bodies. In spite of this fact, "carriers" are quite healthy and epidemics do not develop.

Similarly, plague bacilli have been found occasionally in rats or in fleas infesting rats in certain large cities. A few years ago a small, circumscribed plague endemic, which soon subsided, was reported from Paris. Sporadic cases of typhus are constantly reported in this and many other countries, and yet no major epidemic of typhus arises. That major epidemics do not always result is not because our hygienic measures are so nearly perfect (although this certainly is a major factor), but mostly because the *génie épidémique*, the ensemble of the specific conditions necessary for the onset and persistence of an epidemic, is not at hand—the environment, time, or the climatic conditions are not propitious.

Indeed, many factors must simultaneously collaborate to bring about and nourish an epidemic. One very important factor favoring certain epidemics is the lack of resistance of the population, that is, an increase in their susceptibility to infections. The general resistance of the population is, for example, lowered during the winter months, and it is a current observation that flu or diphtheria epidemics often have their start in January, February, or March. Great heat and humidity, rain, and cold are other atmospheric conditions that decrease resistance and favor the development of diseases in any given community. The economic conditions of a population often facilitate (or check) the development of certain epidemics; plague, for instance, is a disease of poor and undernourished people living in squalor. One of the most important factors favoring the spread of many infections is the disruption of good hygiene.

Our understanding of bacterial warfare can be greatly facilitated by recognizing that many of the important factors favoring the onset and spread of any epidemic are fulfilled during wars. In these periods, personal and general hygiene are at a low level, and great masses of military personnel and civilians are concentrated in besieged cities or in regions to which they have fled from the enemy. Their housing is bad, food supplies are rapidly depleted, famines are common, and the general resistance of the population vanishes.

No wonder, then, that epidemics are automatically the companions of wars. One can say that in the past the story of war was not so much a tale of the destruction of life or property by bullets and fire as the story of epidemics and disease that followed in the wake of the armies like a great shadow. In many wars, epidemics and disease have accounted for more destruction than actual warfare, and very often it was not the sword or the guns that decided the outcome of a battle or a campaign, but disease.

The forward march of Roman power was interrupted by the only force that was capable of opposing its mighty legions—the great Justinian Plague of the sixth century, which lasted fifty years and which, like its successor, the Black Death, felled about one fourth of the world population.

Another historical period of war epidemics is that of the Crusades, the story of which reads like the chronicle of an uninterrupted series of epidemics, pestilences, and famines. When, in 1098, the Christian army of 300,000 men besieged Antioch, disease killed so many and in such short time that the dead could not be buried. In the few weeks between September and November, 100,000 died; the victims were principally women and children who had accompanied the soldiers.

One of the most destructive wars of history, accompanied by famine, disease, and epidemics, especially among the civilian population, was the Thirty Years' War that from 1618 to 1648 ravaged Germany, Austria, Bohemia, the Netherlands, and parts of France. Armies from the various countries invariably carried epidemics from one place to another, and, wherever they appeared, one quarter of the civil population died. Like the story of the Crusades, the story of the Thirty Years' War is one long tale of panic-stricken populations fleeing the approaching armies in wild despair and dying from disease rather than at the hands of murdering soldiers. The population of Bohemia fell from 3,000,000 to 780,000, and that of Germany from 16,000,000 to 4,000,000—all this following, within 300 years, the plague that had itself destroyed as many lives and blighted the existing civilization.

The records for North America are no better. In 1520 when the Spaniards came to Mexico, one of the Negro slaves accompanying the army transmitted smallpox to the Mexican Indians. Within a few years more than 3,000,000 Indians died of smallpox, and the once mighty Aztec population was reduced to a small group of shaken beggars.

The American Civil War was one of the most murderous in history. Of 850,000 soldiers engaged in this war, 350,000 died; that is, more than 40 percent. But only 100,000 of them died from battle wounds; more than twice this number (60 percent of all who perished) died of diseases such as typhus fever, dysentery, and malaria.

I have mentioned some of the gruesome details of epidemics, and especially their dependence upon wars, in order to make two points clear: That we know of no greater destructive force on this earth directed against human life than that of pathogenic microorganisms, and that there are no better opportunities for pathogenic microorganisms to thrive and to start widespread epidemics than those presented by wars.

It is most surprising that only a very few attempts have been made to produce deliberately, in wartime, epidemics or infectious diseases in the camps of the enemy with the intention of weakening the adversary. During the siege of Caffa in 1346, the year when the Black Death first smote Europe, the plague broke out among the invading Tartars. The besiegers are said to have infected their foe by throwing over the walls, by means of engines of bombardment, corpses of persons who had died from plague.

Much later, in 1763, the English General Amherst, Governor of Nova Scotia, tried to destroy the Indians with smallpox by means of infected blankets. In 1917 the Germans are said to have tried to contaminate army horses in France with glanders. And, finally, Wickham Steed has reported that in 1940, in a certain part of China, the Japanese dumped from airplanes rice kernels contaminated with plague bacilli, with the intention of producing a plague epidemic there, far behind the lines.

As in the times of the Tartars, so may modern warriors consider the spread of infections behind the lines of the enemy as one of their principal objectives. But, in contrast to the crude attempts of the Tartars, in modern bacteriologic warfare we no longer need rely upon the uncontrolled spread of disease. Modern bacteriologic warfare strives for two well-controlled effects which govern the use of any weapon: optimal efficiency against the enemy and maximal protection of one's own army and civilian population. Consequently, the study of bacterial warfare is concerned with a number of major problems: the selection and mass cultivation of the most suitable microorganisms, or the mass preparation of toxins; the investigation of natural and artificial epidemiologic conditions; the technology of the bacterial weapon; and the protection of one's own army and civilian population.

Upon careful consideration of the conditions by which the various great epidemics develop, it seems quite possible that one could produce certain of these epidemics at will either in the ranks of the enemy army or among his civilian population. But it is also probable that it would be very difficult under most circumstances to control the spread of many artificially induced human epidemics. They would threaten not only the enemy's army and civilian population, but also those of the aggressor. For this reason none of the authors who have written about bacterial warfare considers as useful for such purposes the classic microorganisms of plague or cholera bacilli, flu or typhus virus, which have produced the great epidemics in the past, in peace as well as in war. I personally share this opinion and believe that a regard for the security of its own army and civilian population may prevent any responsible nation from making use of these microorganisms except in remote jungles or on islands. But the bacteriologist knows of other pathogenic microorganisms that so far have not produced epidemics of widespread character. It may be quite possible to deliberately produce with these germs incapacitating mass infections that might not spread by themselves and that would not endanger the aggressor's army.

The practical use of these organisms is possible since we are able to disperse and unload upon the enemy infectious microorganisms in such huge quantities that the resulting epidemics will no longer follow the pattern and the laws governing the spread of normal epidemics. Man-made artificial conditions may change the epidemiology of any disease in such a way that germs which hitherto have not produced large epidemics become useful war weapons.

Bacteriologic warfare—and this is the contribution of the modern age of biologic war—creates its own conditions for the dangerous spread of various diseases; it does not rely on the natural pathways taken by the classical epidemics, that is, spread by means of water droplets, by contact with infected material, or through diseased animals or foods.

Fortunately for us, many technical problems complicate the realization of bacterial warfare and may prevent its practical application, or at least render it more difficult. Many organisms, theoretically useful for bacterial warfare, are difficult to cultivate; in other cases, the bacterial preparations are either unstable or cannot retain their virulence over a sufficient period of time. Furthermore, certain microorganisms are unable to produce infections directly but require vectors such as insects, which in themselves are difficult to handle and which impose additional problems. Nevertheless, the principal difficulties with bacterial warfare are not these technicalities alone nor the problem of its power and impact. There is no question regarding the efficacy of many bacterial weapons; the real problem is that of self-protection.

Protecting the troops of the aggressor nation and its civilian population in the rear is of major importance. The soldiers who personally handle the propagation and dissemination of dangerous microorganisms can be protected to a certain extent by means of special uniforms or masks. But such mechanical protection is not feasible for all individuals in the army and civilian population, and other means are necessary.

One may consider prophylactic chemotherapy as being similar to malaria prophylaxis with quinine, or syphilis prophylaxis with bismuth therapy. Other preventive agents against infections are specific sera or vaccines; chemotherapy and antisera have the same importance for bacteriologic warfare as has the gas mask for chemical warfare. Treatment with antiserum and vaccine produces immunity, that is, protection from those microorganisms against which the serum or vaccine are directed. We possess excellent chemotherapeutic agents and antisera for certain diseases, but not for all. Antisera are highly specific in that they are effective only against certain pathogenic microorganisms and do not confer immunity against all other pathogenic microbes.

Unfortunately, we know of many infectious diseases in which immunization with specific sera or vaccines has had little or no beneficial effect. This applies especially to many infections which otherwise would lend themselves well to bacterial warfare. But even specific antisera do not give protection under all circumstances. Treatment with the best serum becomes useless should the enemy succeed in cultivating an antigenic variant of a pathogenic microbe, or detect a new virulent microbe against which no antiserum exists. Bacterial warfare is much more versatile than atomic warfare.

Considering the pros and cons of the various infectious diseases of man, there can be no doubt that the number of organisms that can be used effectively and safely in bacterial warfare aginst humans is greatly restricted. This, however, does not exclude the possibility, or even the probability, that bacterial warfare directed against human life may be used in certain phases of future wars. Whatever the scope and the effectiveness of such future attempts may be, whether or not they will be as colossal and dangerous as the effects of atomic bombs, one fact stands out: The human imagination, the fear of disease, or the feeling of helplessness will create much terror and despair. Demoralization may be even more important than the actual number of diseased persons.

Besides attacks against human beings, bacterial warfare is also concerned with the destruction of the enemy's livestock. Since epidemics of animal diseases generally follow the same pattern as human epidemics, many considerations already discussed also apply to them. The destruction of the enemy's livestock would exert a tremendous influence on the outcome of any war, since it would jeopardize the entire food supply. It must therefore be recognized that bacterial warfare directed against animals may constitute a serious threat. Properly conducted, such bacterial warfare may be highly effective over large areas; an example is the extent to which epidemics of foot-and-mouth disease may spread. The practical effectiveness of warfare against domesticated animals may be limited because of the relative slowness of the spread of many animal epidemics. For this reason, the destruction of useful animals probably cannot be considered as a general or major weapon, but it may be a helpful adjuvant.

Another aspect of bacteriological warfare is the destruction of useful plants. The potential extent of this type of biologic destruction is very wide, at least as broad as that affecting human life and animals. Moreover, it is more practical and has a less odious and emotional connotation. Each of our cultivated plants is threatened by one or more specific diseases that in peacetime often totally destroy a crop. Besides insects, there are many varieties of molds, bacilli, and viruses, all of which specifically attack and destroy the fruits, leaves, or roots of many crop plants, producing wilts, rusts, rots, blights, and smuts of fruits, vegetables, and grains. It is possible to cultivate many of the microorganisms causing these plant diseases, just as we do those microorganisms afflicting man or animals, and we can contaminate entire fields experimentally and deliberately.

The economic importance of the destruction of a wheat or a potato crop, for instance, is self-evident, especially if an enemy population is dependent entirely upon home-grown crops. The danger of spread from field to field can be greatly diminished by proper prophylactic treatment of the plants, and this danger of contagion does not exist when chemical substances rather than living organisms are used for the destruction of crops. Although the use of chemicals as weapons belongs to the realm of chemical warfare, certain plant-destroying substances are generally included in any discussion of biologic warfare. Chemicals have been developed which rapidly destroy many plants. An example of a plant poison is the new weed-killer, 2,4-D, which, incidentally, is a useful by-product of the research on bacteriologic warfare. Without question, bacterial warfare waged against useful plants is a type of biologic war that has the fewest pitfalls. It is the least dangerous for the aggressor, but very effective against the victims. Plants can be rapidly destroyed over large areas, with immediate results.

Thus far this discussion has dealt only with the naturally occurring epidemics, the possibility of deliberately producing epidemics in man, animals, and plants, and the difficulties encountered in the proper choice of a bacteriologic weapon. Several other technical problems should be mentioned, together with an analysis of certain differences between bacterial and atomic warfare.

Atomic warfare is a very complicated, elaborate, and costly enterprise, and only those nations endowed with great industrial and technical resources can produce atomic bombs. Bacterial warfare, on the contrary, is a cheap and comparatively simple method of warfare. It can be developed by any nation, large or small, rich or poor, to which the methods of modern bacteriology are available. One might truthfully say that bacteriologic warfare is the poor nation's atomic bomb.

Any bacteriology laboratory, whether in a hospital, research institute, university, industrial plant, or in a properly equipped private laboratory, can produce huge amounts of the bacteriological weapons. The equipment to cultivate and prepare bacilli is simple, and hundreds of liters of most powerful bacilli or virus suspensions are easily prepared. For instance, a simply prepared suspension of the dangerous psittacosis virus, which produces a very serious pneumonialike disease, contains a million deadly human doses in a single drop; in other words, one drop properly dispersed is theoretically capable of killing a million people, and, consequently, half a cup of such a preparation could, again theoretically, eradicate all mankind. Equally simple of preparation are the "bacterial bombs," the containers and missiles in which the infectious material can be delivered to the enemy.

There is no comparison between the technology of bacterial warfare and that of an atomic bomb or any other modern weapon. Complicated devices are not necessary to transport "bacterial energy;" a cargo of microorganisms can be carried by plane, balloon, rocket, in a suitcase, or even in the pockets of spies and agents behind the enemy lines. Drinking water and food supplies can be contaminated from small bottles or vials; planes can spray the prepared cultures exactly as they now spray insect-killing chemicals over truck farms or infested woodlands. The microorganisms can be disseminated as artificial rain, or dispersed in exploding shells.

It may be true that bacterial warfare is not just around the corner; nevertheless, there is no choice but to accept, for general guidance, the statement recently made in an official Anglo-American government release that the possibility of bacteriologic warfare cannot be discounted. It is certain that any responsible government would refrain from using some of the possible bacterial weapons; but we must anticipate those irresponsible war lords and madmen, who, before their final defeat, and *in extremis*, throw overboard all conventions, treaties, and moral considerations. Might not a future Hitler or Mussolini try to draw his conquerors and even his associates with him into a cataclysm of deliberately produced pandemics of psittacosis or plague? It is, therefore, our first and most urgent task to prevent and control bacterial warfare just as eagerly as we try to prevent atomic warfare.

There can be no question of any unilateral action, as there can be no question of the unilateral control of atomic warfare. Recently, David Lilienthal, Chairman of the Atomic Energy Committee, wrote:

> The Atomic Energy Commission is today designing, developing and producing weapons of unimaginably destructive power. It is the firm resolve of the Atomic Energy Committee to maintain pre-eminence in these weapons until effective international safeguards are in force.

Similar considerations apply to bacterial warfare. It would certainly be unwise to neglect the development of its technological possibilities and the defenses against it.

Simultaneously with the intensive research and development of bacterial warfare that is now going on, we must arrive at an international agreement. Actually, bacterial warfare, together with chemical warfare, was condemned by a convention signed in Geneva in 1927 and ratified by eight nations. Many never believed that such a convention had any more practical value than similar previous conventions which failed to prevent the use of newly discovered weapons. In the thirteenth century the Lateran Council forbade the use of the arbalest, or crossbow. In the fifteenth century Bayard, in France, asked the interdiction of the use of the shotgun. Only recently—on February 9, 1948—Pope Pius XII, calling the atomic bomb the most terrible arm which the human mind has thus far conceived (his predecessor, 700 years ago, used the same term for the crossbow), urged that the use of atomic energy for warlike purposes be outlawed. All these interdictions have been without effect, since man has always used, and will use again, those agents of warfare that have proved to be most powerful and most efficacious. It is useless to outlaw arms or to discuss their legitimacy. Pascal realistically stated. "Since it was not possible to make strong everything which is legitimate, one has arranged to make legitimate that which is strong." If bacterial warfare can prove its strength, then bacterial warfare will be declared legitimate. It is not likely, therefore, that condemnations and interdictions will solve the problem of this type of warfare.

Any control of atomic, as well as of bacterial, warfare must start with manufacturing. In the manufacture of atomic bombs, the weakest link is the production of the explosive ingredient, plutonium. Unlike the atomic-energy plants, bacteriology laboratories are normal components of modern research and are easily established; this is precisely what renders any control of bacterial warfare so utterly difficult. Bacteriology laboratories can easily and unnoticeably be converted to noncivilian activity. Moreover, there are no major weak links in the production of the weapons of bacterial warfare; the necessary cultures and containers can be produced in small rooms anywhere, and it would therefore be necessary to maintain a spy system of dictatorial dimensions. One could perhaps consider the control of the vectors, or carriers, of bacterial warfare, the balloons, the guided missiles, and even airplanes. Eliminating these vectors would be an effective precaution, but who would or could now renounce airplanes? I personally do not see any other way than to resort to, and rely upon, political agreements.

POPULATION PHENOMENA AND COMMON KNOWLEDGE

LAMONT C. COLE

Dr. Cole (Ph.D., Chicago, 1944) served during World War II as an officer in the U. S. Public Health Service, where he studied populations of rodents and their ectoparasites in relation to typhus epidemiology. For the past two years Dr. Cole has taught ecology and entomology at Indiana University. This fall he went to Cornell University as assistant professor of zoology

ALL organisms as we know them, as they are available for present-day study, have behind them a long record of survival both as individuals and as types. Thus the diagnosis of the conditions determining survival, and of those which have determined survival in the past, becomes a central theme of ecology—the study of the interrelationships between living organisms and their total environment, both living and inorganic.

Part of the environment of any organism consists of other organisms, and when we examine interrelationships within a group of individuals belonging to a single species, we quickly become aware that the population has essential properties, not visible in the individual organism, which may tip the scale between survival and failure to survive. So far as the individuals are concerned, they compete with each other for food, space, and other essentials that are available in limited quantities, they may cooperate in exploiting their surroundings or in defense, and they are altered physiologically and behaviorally by propinquity. These and similar interactions may increase or decrease the probability of individual survival, and an immense amount of research has been devoted to evaluating these effects.

A slightly different emphasis is obtained by examining the properties of populations as these affect, not mere individual survival, but the survival of types of organisms. This long-time survival implies that the organisms are equipped for effective and certain reproduction. If individual reproductive success is uncertain, as it necessarily must always be, survival depends upon maintaining a sufficiently large breeding stock that adequate reproduction of the population approaches statistical certainty. The failure of one generation may mean extinction, a fate which has overtaken countless species known only from the geological record, but one which every living type of organism has somehow managed to avoid.

The emergencies that have been met and overcome by their ancestors during the long struggle for survival have left impressions on all modern organisms in the form of adaptive characteristics for increased efficiency in coping with their environments. Random variations that reduce efficiency have been eliminated and so have specialized adaptations to niches (opportunities for special ways of life) which at some time in the earth's varied history have ceased to exist. Similarly, the luxury of maintaining inefficient populations has been suppressed.

Our understanding of population phenomena has made impressive gains both in the field and in the laboratory following the development of new techniques to supplement the ordinary physiological methods suitable for studying individual survival. Outstanding among techniques for population study are the statistical approach, the selection of populations amenable to laboratory study under approximately natural conditions, and the formulation of theoretical mathematical models to deal with the subtle, unusual, and slow-acting environmental factors which exert their influence over the course of many generations. Studies employing these techniques continue to reveal new and ever more essential relationships between the population phenomena of growth, composition, organization, size limitation, and mass physiology on the one hand and population survival on the other.

As more or less a by-product of analytical population studies, there has emerged a great deal of information with a potential bearing on human welfare. Specialists have translated some of this information into practice in the population fields of demography, epidemiology, agriculture, game management, and pest control, but, unfortunately, common knowledge, as revealed by the popular press, seems scarcely to have advanced beyond

---

* Contribution No. 390, Department of Zoology, Indiana University.

LOW REPRODUCTIVE POTENTIAL

THE EXTINCT PASSENGER PIGEON PROBABLY PRODUCED ONLY ONE EGG PER BROOD; HENCE SURVIVAL DEMANDED A LARGE BREEDING STOCK AND A SOMEWHAT EXTENDED INDIVIDUAL LIFE EXPECTANCY.

blind acceptance of uncritical and often irresponsible statements by various vocal but uninformed persons or groups.

Experience shows that there is a type of man, common in all nations, who will hasten to support any environmental manipulation which, on grounds however slender, is proposed for improvement of those populations represented by his lawn, his favorite game animal, or his favorite stock of fish. He will, without asking why, join in the persecution of nearly any predator, whether it be a spider, house centipede, snake, hawk, or coyote; and, if control is effective, he may then attempt to personally substitute for the absent predator. He is easily alarmed by what is told him regarding human populations. He may join controversies over birth control, believing that an increased birth rate is the answer to all his country's problems while at the same time he is concerned over a shortage of food and *Lebensraum*. He is shocked if the death rate in his community begins to approach the birth rate and he is edified by someone's remark that a "normal" human life span should approximate one hundred and twenty years.

Until the average nonscientist can be brought to think critically and skeptically about loose statements of population problems and phenomena, there seems little hope of incorporating the potentially useful results of modern research into common knowledge. Many of the things which the public must learn to disbelieve scarcely require experimental demonstration. A little logic and some skepticism toward dogmatic generalizations coupled with a very little empirical information can clear the way for an understanding of the complex which ecologists are seeking to unravel. Thus, in the interests of common knowledge I shall attempt in the following paragraphs to clarify some of the simpler but most abused conceptions of population phenomena.

Some of the common fallacies about populations may be resolved by simply remembering that any population must be finite in size. The reproductive potentialities of all organisms are such that, if unimpeded, populations would grow in a manner very like a sum of money placed at compound interest. It has been estimated by Hodge that one pair of houseflies reproducing at their maximum rate could in five months produce enough descendants to cover the earth to a depth of 47 feet. By a slight extension of the computation it may be shown that under the postulated conditions about one year would suffice to make the number of houseflies equal to the number of electrons estimated by astronomers to be present in the visible universe. This is a dramatic but not an extreme case, as many organisms exceed houseflies in reproductive potential.

Any species in which two individuals, at the time of their deaths, consistently left behind more than exactly two offspring like themselves would grow at compound interest and, if unchecked, would eventually overflow the earth. Obviously, then, population increase cannot be indefinitely continued, and indefinite decrease clearly leads to extinction. Consequently, at some intermediate size all populations must attain an equilibrium where they neither grow nor shrink. The population may either remain at this stable level or may fluctuate about this level as an average. Sometimes such fluctuations are extreme, and the upward part of the cycle, in the form of "mouse plagues," "locust plagues," and "deer irruptions," attracts great attention. The point to be noted here, however, is simply that under any set of environmental conditions, there is for each species and population some stable or average equilibrium size toward which the population is trending. A population that is growing or shrinking is in a temporary and unstable state. If we wish to understand population phenomena we must consider the population in its equilibrium condition.

If we consider a population which has attained a constant stable level in equilibrium with its environment, it is obvious that for each individual entering the population exactly one individual must leave. For a cyclically fluctuating population this condition will pertain over a long-time average, whereas for an absolutely constant population it will be true at all times. Additions to the population can come only from birth and immigration, and losses come only from death and emigration. For a generalized population picture we may consider a closed population or an entire species where changes result only from births and deaths and not from migration. Migratory movements do not change the basic population picture but merely obscure the vital statistics by counting a single individual as a birth with no corresponding death in one population and as a death with no corresponding birth in another. This, of course, tends to alter the birth and death rates recorded for the two populations without altering the average rate or in any way upsetting the correspondence of births and deaths if a larger group is considered.

Thus we see that in any population that has passed its temporary stage of growth and attained a stable condition there must be exactly one death for every birth, and, accordingly: birth rate = death rate. This could not be otherwise because all organisms are mortal and each one that is born must die. (Throughout this discussion we shall neglect the situation in organisms which reproduce solely by fission.) Any imbalance between birth rates and death rates can only be maintained while a population is changing in size in a particular direction and this, of course, is a temporary condition. Perhaps this seems too obvious to be worthy of mention, but the fact is that very many persons never view what they read and hear in this light. One constantly encounters references to communities and regions in which the human birth rate exceeds the death rate, presented with the implication that such a condition indicates a particularly healthful environment or even especially good medical care! Actually, there are only two possible interpretations of the situation; either the local population is in a temporary state of growth, or many individuals born in the locality move away to die elsewhere. The only permanent way of reducing the death rate is to reduce the birth rate.

It is usual for death rates to increase with increasing age. Therefore, a rapidly growing population where births exceed deaths tends to have a high frequency of young individuals, whereas, with the slowing of population growth, a shift is noted in the direction of increased average age and increased frequency of older individuals. Anyone can observe this shift in age structure by following the history of a particular population such as a growth of timber or the human population of the United States, but many persons fail to recognize

Ernest P. Walker

A POTENTIALLY IRRUPTIVE SPECIES

IN 1927 A LEADING MAMMALOGIST FOUND A POPULATION DENSITY OF SEVENTEEN HOUSE MICE PER SQUARE YARD, OR MORE THAN 82,000 PER ACRE, AT THE CENTER OF AN OUTBREAK IN CALIFORNIA.

its significance. When man is systematically exploiting a population, as for example in the fisheries industry, a shift toward increased age is a warning that reproduction has fallen below its maximum, whereas in pest control such a shift may be an encouraging sign that control is becoming effective.

The responses and behavior of a population are greatly influenced by the fact that it is not homogeneous but ordinarily consists of several types of individuals differing in their biological importance. In general, the number of females limits population reproduction and survival to a more significant extent than does the number of males. It is this fact that makes it possible to annually remove large numbers of male deer or pheasants (which are promiscuous, or at least polygamous) from a range without appreciable effect on the next year's production of young. Because this is true, it is not surprising that in their struggle for survival many animals have increased their reproductive potential by various mechanisms which make the most of the limited supply of environmental necessities available to females. Bee colonies may kill off excess males when food is short; female spiders frequently eat their mates after their eggs are fertilized; various mollusks maintain extremely unequal sex ratios by the teleologically beautiful expedient of having the excess males turn into females; and aphids and water fleas (Cladocera) which expand their populations rapidly in summer dispense with males entirely at this time, the summer generations of young all developing from unfertilized eggs and all becoming females. Parthenogenesis is the usual type of reproduction for many types of animals, and males of some species of insects have never been found, this sex apparently having entirely disappeared.

In addition to sex heterogeneity, most populations are divisible into about three age groups, which differ in population significance. The life span of many animals falls into three distinct periods: an immature or prereproductive period of growth and development, a period of potential reproduction, and a postreproductive period of sterility. The relative lengths of these periods vary greatly for different animals and may even be highly variable within a single species. The postreproductive period does not exist for May flies, salmon, and certain butterflies, which die immediately following reproduction; it amounts to some 50 percent of the usual life span in brown rats; and in populations of the Western mosquito fish recently described by Krumholz it varied with the season of maturity, amounting to some 20 percent of the life span of overwintering females and to about 55 percent for those passing their entire life span in a single summer. For human females in the United States, the reproductive period effectively extends from about age fifteen to about age forty (97.43 percent of the births recorded in the U. S. for 1943 were to mothers aged fifteen to thirty-nine). Thus in man the postreproductive period makes up something over 25 percent of the usual life span.

THE CLADOCERA, OR WATER FLEA

SHOWN HERE WITH YOUNG DEVELOPING IN THE BROOD POUCH, THIS FLEA DISPENSES WITH THE MALE SEX WHEN ENVIRONMENTAL CONDITIONS ARE FAVORABLE.

The capacity of a population to increase at any given moment is clearly dependent not only upon total population size and individual reproductive potential but also upon the number of individuals in the reproductive period of life, which in turn depends upon population size and length of the reproductive period relative to the other parts of the life span. Any cause of death or sterility which removes individuals from the reproductive portion of the population means fewer births and a decrease in the reproductive and growth potentials unless the population can in some way compensate for the loss. Direct compensation leading to immediate expansion of the reproductive class has been suggested for rat populations where the increased food and perhaps shelter made available result in earlier maturity and a longer span of reproductive life. However, such direct loss compensation achieved by altering the relative lengths of the reproductive and sterile portions of the life span is probably of much less general importance than the more delayed compensatory trends resulting from larger litters, more frequent reproduction, and, above all, increased survival of offspring.

The chief significance of the immature or prereproductive portion of the population stems from the fact that (barring immigration) this class is the only source of new reproductive individuals. Increased deaths in this group may produce a misleading apparent rise in birth rates because population size is decreased with no immediate loss of population fecundity. This, of course, is only temporary. Any decrease in the number of immatures means an eventual decrease in the number of reproductives unless compensation occurs in the form of increased individual reproductive performance or, as is common, improved survival of the remaining prereproductive individuals.

From the foregoing, it should be evident that the size of the prereproductive part of the population tends to be a simple multiple of the size of the reproductive class. Any permanent change in the relative sizes of the two classes presupposes that the population can be kept permanently in a condition of rapid growth (a possibility to be discussed later) or that the relative length of time spent by each individual in the two classes can be altered. Changes in the birth rate or differential mortality in the two groups can produce only temporary changes in the balance unless such changes are extreme enough to lead to extinction. For most species the relative lengths of the developmental and reproductive stages of life seem to be fixed and beyond human control. This points to a common fallacy of thought about population problems. In man the ages for physical labor and military efficiency correspond closely to the ages for reproduction, and it has often been proposed to expand this portion of a national population by tampering with the birth rates. It is no accident that such proposals are most frequent in countries that are already worrying about overpopulation—where the decreased rate of population growth has shifted the age structure in the direction of increased age. The fallacy of such proposals lies in neglecting the necessary corollaries of an increased birth rate: a general population increase in all three age classes and/or an increased death rate.

Losses from the postreproductive portion of a population are unique in biological significance. Such losses reduce population size without altering its fecundity and hence lead to an increase in population growth rate with a corresponding shift toward a youthful age structure. A dictator willing to risk the obvious consequences might eventually succeed in enlarging his armies by taking steps to reduce the number of postreproductives, but the matter is unworthy of discussion in connection with the control of human populations. It is, however, a very important subject in the management of game and commercial organisms.

By harvesting postreproductives, additional food, space, and other limited necessities are made available to the reproductive and potentially reproductive individuals. Also, the average reproductive rate per member of the population is increased, with a consequent more rapid production of individuals in all three age classes. Thus, the selective catching of the oldest fish in a population, which are also likely to be the largest and least fecund fish, has a positive stimulating effect on the growth of all age classes and also frequently reduces a mortality pressure resulting from cannibalism. It has been suggested and denied that predators serve a valuable function in nature by selectively removing the aged, unhealthy, and therefore most accessible members of prey populations, thus maintaining a progressive population structure.

Selective mortality of the postreproductives, with death rate accelerating with age, is a common phenomenon among organisms under usual conditions, and this serves as a natural control which maintains population fecundity by not allowing too large a proportion to slip over into the postreproductive class. In the case of man a shift to older population structure is reflected by new emphases in medicine and public health as the causes of death such as cancer and cardiovascular conditions, which are highly selective for older individuals, naturally gain increased prominence.

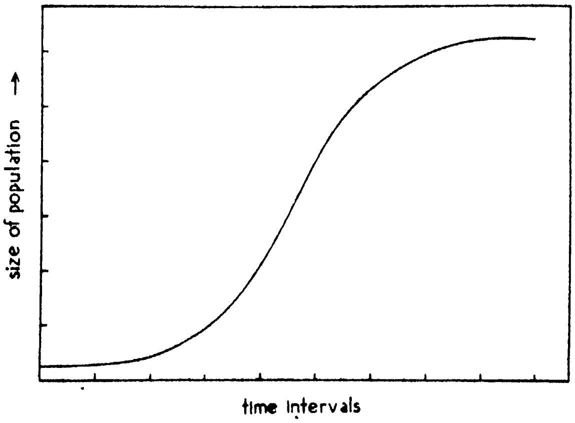
We now hear frequent comments on the extent to which human longevity has been increased and we are made aware that the search for controls for cancer and heart disease is in full swing. Without deprecating any of these efforts it may be well to glance at human longevity from the population standpoint to anticipate the effects of gains in this phase of medicine. We may first note that the effective maximum span of human life is somewhere near one hundred years and has changed very little if at all in historic times. If we compare United States life tables for 1941 with those for 1858, we find that in this period a newborn child has gained in life expectancy by some twenty-two years, whereas a person of sixty can expect to live less than two years longer than his 1858 counterpart. The gains in longevity have come through increased survival in early life and not through lengthening of the life span. If the life span were to be much increased, it would necessarily mean that eventually a much lower proportion of the population would fall into the reproductive age class and that some new problems would have to be met. With lower death rates a smaller reproductive class might adequately supply the number of births necessary to maintain the population; but if man is going to continue heavy physical labor and warfare under such conditions, human vigor will have to be greatly extended into later ages.

If a few reproductive organisms are transported into a favorable and unoccupied environment and there begin building up a population, this population will, as already noted, tend to grow after the manner of money placed at compound interest. If we make a graph plotting the size of such a population against time, we find that, like money at compound interest, the rate of growth is at first slow but that it accelerates with time. This cannot be indefinitely continued, however. Sooner or later the rate of growth begins to taper off as food or space or some other resource of the environment is not produced rapidly enough to support the accelerated population growth. As the population continues to grow, it then grows more slowly until finally growth practically ceases and the population tends to remain at a rather constant level as long as environmental conditions are unchanged. The history of population growth thus describes a rough S-shaped ("sigmoid") curve. This type of growth is very general, and curves of this shape have been found to fit such diverse material as the growth in weight of vegetables, the number of victims of a communicable disease ("growth of an epidemic"), the growth of amount of lumber in woodlands, and the growth of populations of an extremely wide variety of organisms from bacteria to man. The sigmoid shape of the growth curve is usual no matter in what units the population is measured. For example, in fish populations a sigmoid growth curve is obtained whether the population is measured in numbers of individuals or in pounds of fish per acre of water surface.

A great deal is now known which enables us to rationalize the shape of the growth curve and a great deal is known about its mathematical properties, although some slight controversy still exists regarding the best-fitting mathematical model. For our present purposes it is enough that we merely accept the empirically established sigmoid shape of the curve, at the same time noting that the damping off of growth rate indicates some inadequacy of the environment to further support an increasing growth rate or an indefinitely large population.

Regarding this limitation of growth rate by limited environmental resources, many common misunderstandings might be noted. Perhaps the most common is the tacit assumption that limitation is brought about by inadequate reproductive potential. The construction of game-fish and game-bird hatcheries and other expensive means of adding additional immature members to a population should be undertaken only when the need for enhanced fecundity has been indicated by much more critical study than has usually preceded such endeavors.

If one traces the shape of the growth curve, paying attention to the rate at which the curve is rising for each unit of time, it will be noted that the maximum rate of growth and the maximum increase in number of individuals occur at some intermediate population size, with slower increase being characteristic of both large and small populations. This is the crux of a fundamental problem of population management which has come to be widely known as the "optimum-yield problem." If few individuals are removed from a population per unit time, the population remains large and adds new individuals slowly. If the rate of harvest is increased, the smaller population responds by increasing more rapidly; behaving exactly as if the population were returned to a previous state, moving further to the left on the curve in the figure. Thus by "fishing" harder and catching more "fish," you induce the population to produce fish more



A TYPICAL POPULATION GROWTH CURVE

rapidly. If one could know enough about a population to keep it at the size corresponding to the most rapid rise in the growth curve, one would then obtain the optimum yield, the largest possible sustained harvest from the population. Note, however, that any harvest greater than the optimum yield pushes the population back into a size range where it grows more slowly. If a constant rate of harvest were maintained under such conditions, the population would be progressively decimated and finally exterminated; but this seldom happens because the effort required to reap the last of the crop becomes prohibitive. When a commercially valuable population has been pushed back to this state, increased efforts to harvest a crop yield progressively diminishing returns. Regarding the situation in overexploited fish populations, Graham has written as follows: "All of the data lead to the same conclusion—that a certain proportion of the time and money of the fishermen is at present devoted to reducing their catch—or is at least wasted." Statements like the above appear anomalous to the average nonecologist and fisherman. It is extremely hard to explain that if he will fish less, he will catch more fish. Here again it seems that man might profit if a little more of known population phenomena could become a part of common knowledge.

There is another aspect of the optimum-yield problem. Efforts to remove pests by killing are often very like harvesting a population for commercial purposes. A rat population, for example, responds very rapidly to reduction of numbers by increasing the production of rats. Thus a sustained yield on a high level may be obtained without appreciably abating the rat problem. It would doubtless be a very impressive figure if one could collect information as to the total annual expenditure in the United States which goes to increase the harvest of "pests." Of course, a sufficiently vigorous and sustained campaign could push the population back beyond its level of maximum growth rate and make it possible thereafter to keep it at a low level without undue effort, but this is not the usual result of our antipest programs.

A similar phenomenon becomes visible when one carefully scrutinizes the activities of the much-maligned and persecuted predatory animals. It has too long been accepted as self-evident that predation is deleterious to a population exposed to it, whereas a stronger case can be made for regarding predation in general as analogous to the yield phenomena described above. Reproductive potentials being what they are, almost all animals typically produce more offspring than can possibly survive. Ordinarily, a portion of the population entrenches itself and gains the use of the environmental resources while the surplus individuals are doomed to death whether by predation or some other factor. As with pest control, the predator reaps diminishing returns per unit effort as the prey population is pushed back into smaller size classes, and it is unusual for any animal other than man to exploit a population obliteratively. Predation reduces the size of the prey population, which responds by more rapid growth, increased survival of offspring, and increased individual reproductive performance—responses which in this connection are often called "intercompensation."

On the other side of the ledger, the value of predators for controlling noxious species has doubtless been overestimated in the popular mind. Predators are typically larger than their prey and, being larger, they typically have lower reproductive potentials. When, as happens in connection with population irruptions, the environmental situation is such that a population approximately achieves its potential rate of increase, it typically outbreeds any predators that may increase along with it in response to the unlimited food supply. When both predator and prey increase at compound interest, the prey species may ordinarily be expected to exhibit the higher rate of interest. The disparity between numbers of prey and predators thus becomes greater the longer the unimpeded increase continues. Under such circumstances, predation becomes effective in population control only after environmental resistance has retarded the growth rate of the prey population.

A great many additional examples of known population phenomena which merit a place in common knowledge might be presented. Nonecologists are generally aware of the deleterious effects of overpopulation but generally unaware that underpopulation may be as deleterious, or even more so. Population size has important influences on the course of evolution, populations (at least of vertebrates) frequently exhibit internal structure on the basis of social dominance and/or the defense of individual territories, populations may differ in breeding structure with consequent differences in their potentialities, and they may exhibit extreme degrees of division of labor between individuals so that whole populations survive or perish according to the efficiency of particular classes of individuals.

Finally, populations of different species interact with unilateral or mutual benefit to produce larger systems, which stand in relationship to the population about as that unit does to the individual organism. These larger units are a dominant part of the environment of man (a part often referred to mystically as "the balance of nature"). They are complex and they are relatively new as objects of serious investigation. However, with the background information now available on the populations which are the building stones of these larger associations, ecological knowledge is advancing rapidly. Man will benefit when sound information on populations becomes a part of general knowledge; sometime in the future sound knowledge of biotic communities will aid man in improving his relationship to his own environment.

REFERENCES

ALLEE, W. C. *Animal Aggregations. A Study in General Sociology.* Chicago: Univ. of Chicago Press, 1931, 421 pp.

DONALDSON, H. H. On the Control of the Rat Population. *Science*, 1925, **61**, 305–6.

ELTON, C. *Voles, Mice and Lemmings; Problems in Population Dynamics.* New York: Oxford Univ. Press, 1942, 496 pp.

ERRINGTON, P. L. Predation and Vertebrate Populations. *Quart. Rev. Biol.*, 1946, **21**, 144–77, 221–45.

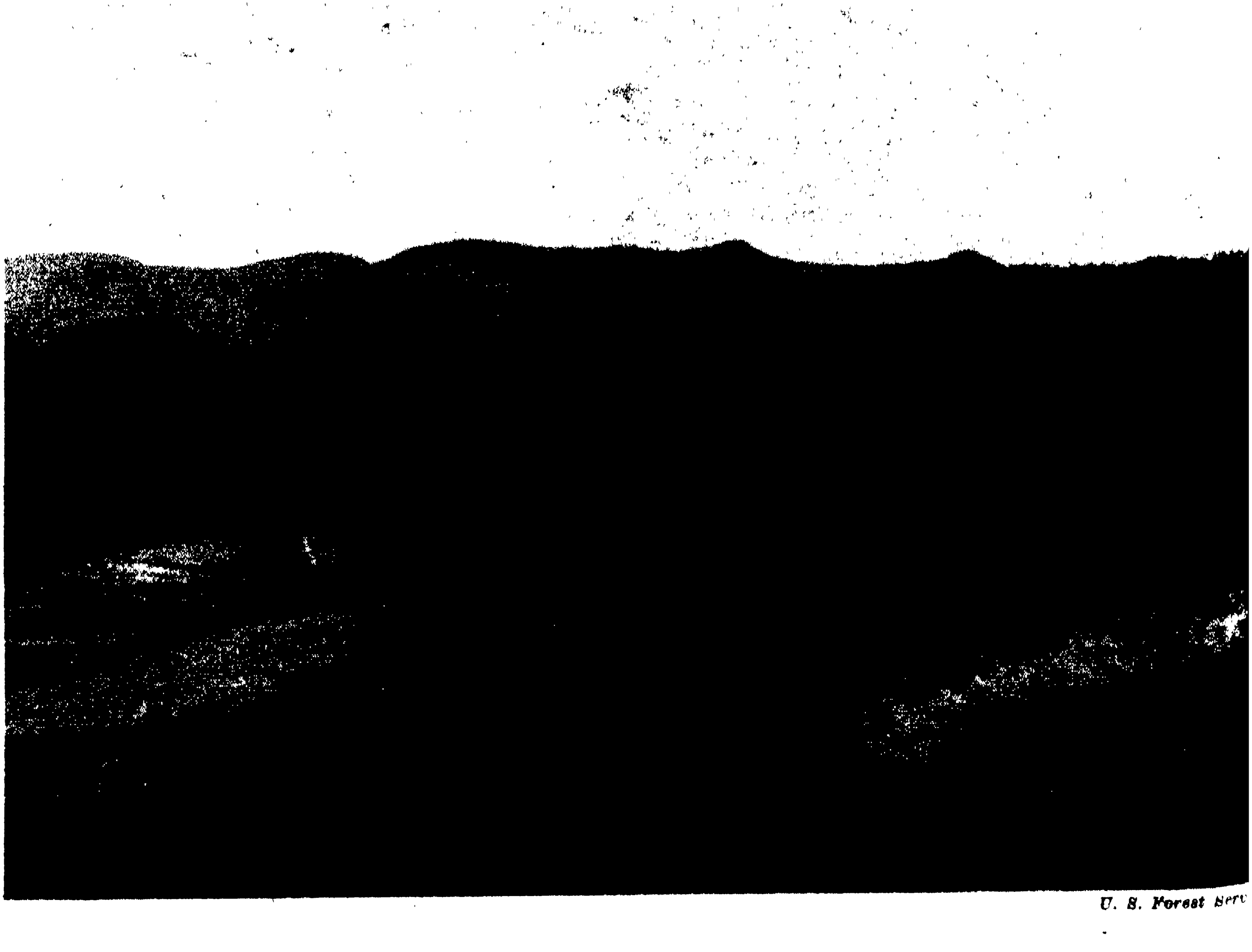
PARK, T. Analytical Population Studies in Relation to General Ecology. *Amer. Midland Naturalist*, 1939, **21**, 235–55.

PEARL, R. *Man the Animal.* Bloomington, Ind.: Principia Press, 1946, 128 pp.

REED, L. J. Changing Problems Growing out of the Change in Composition of the Population. *Amer. J. Publ. Hlth.*, 1948, **38**, 160–3.

Symposium on Predatory Animal Control. May 21–24, 1930. *J. Mammal.*, 1930, **11**, 325–89.

◆



U. S. Forest Serv

OUR FORESTS AND WATERSHEDS

EDWARD N. MUNNS

Mr. Munns, a University of Michigan graduate, began his career as first assistant on Angeles National Forest. After World War I he did forest research throughout the California region of the Forest Service. Later he was transferred to Washington, D. C., and put in charge of silvicultural research. He is now Chief of the Division of Forest Influences, U. S. Department of Agriculture, which includes the forestry phases of watershed surveys under flood control legislation.

MAN can live without shelter or clothing for months, or at least with improvised makeshifts; he can live without food for days; but life without water is measured in hours. Water is the most vital physical resource of a community. Among all the natural forces basic to our economy, its behavior is one of the most obscure. The product of climate, it is closely related to the soil and its cover, and it has for thousands of years affected the life of man and the evolution of his civilization.

Hydrology, strangely enough, cannot be considered by itself, but must always be related to the land—to climate, to soil, to vegetation. Solomon and the philosophers of ancient Greece explained that springs were fed by subterranean channels from the sea. Even during the Middle Ages this idea persisted. It was not until around 1650 that it was learned that the amount of water in the streams was related to the rainfall of the catchment basin. From then on, the relation of water to the land became more and more apparent, and as time went on it was realized that water could not be studied without taking into account land as well. Various advances were made in the next two centuries. The source of artesian water was discovered, a way was found to measure the velocity of streams, and, finally, it was learned that soils and rock may be permeable.

The natural processes involved in the hydrologic cycle are complex. Some, like evaporation and transpiration, cannot be measured accurately; others, like deep seepage, cannot be measured at all. Man does know, however, that there has developed a harmonious adjustment of the physical forces that relate to vegetation, soil, and water. Consequently, as our knowledge of the behavior of water increases, especially in its relation to vegetative life, we shall be able to exert a perceptible influence on the water regimen.

COWEETA EXPERIMENTAL FOREST

TWO PANORAMIC VIEWS OF U. S. FOREST SERVICE EXPERIMENTAL AREA NEAR FRANKLIN, NORTH CAROLINA. A TREATED AREA FACES THE CAMERA JUST ABOVE AND TO THE RIGHT OF THE HEADQUARTERS BUILDINGS.

It has long been known that good forests, good soils, and good water go hand in hand. Contrariwise, when man uses the land improperly and seriously upsets the ecological balance between soil, water, and vegetation, a reaction sets in which may be disastrous to civilization. Thus, fire following logging on steep mountain slopes invites erosion; erosion reduces the rate at which water can enter the soil; water that cannot enter the soil must run off; an excessive amount of surface runoff means flashier streams and an increasing number of floods—one reason why these appear with sickening monotony in all parts of the United States.

So far the cause-and-effect relations of various kinds of land use and water are not too clearly established. Although in the past quarter century many valuable records of the amount of water carried in the various streams of the country have been compiled, these data cannot be related to the complex watershed conditions over a large drainage area. Consequently, they do not tell us much about the effects of any specific type of land use on the water resource. To get a clear picture of such relations, records from small watershed units are required, where the influence of land clearing, timber cutting, grazing, tillage, etc. can be closely observed.

The United States Forest Service is particularly concerned with discovering the principles of beneficial watershed management, since it is responsible for the protection and administration of many important watershed and headwater areas. For a number of years, the Forest Service has been carrying on investigations in the relations of forests and water in outdoor laboratories scattered throughout the United States. One of its major laboratories is the Coweeta Experimental Forest, in the Nantahala Mountains of western North Carolina, about 80 miles southwest of Asheville. Here, in an area of 5,400 acres, are conditions highly suitable for hydrologic research.

The Coweeta area lies in a zone of heavy rainfall. Precipitation averages 65–90 inches a year and comes from many storms—sometimes 100 a year—of all kinds, heavy, light, intense, very gentle. Although no one year's rainfall pattern is exactly like another, there is enough comparability in the storms and their distribution to permit collection of an abundance of data in a relatively short span of years. This area was selected as a hydrologic laboratory for other reasons also. The geology is uncomplicated, and the topography is particularly suited to watershed observations. Lying at elevations of 2,200–5,200 feet, the deeply weathered Archean rock gives rise to a wonderfully deep and well-drained soil. The slopes are steep, and the sharp-crested ridges form natural boundaries for the many small watersheds into which the Coweeta Forest is divided. Each experimental watershed is a miniature valley, complete within itself, and ranging from 20 to 200 acres.

Cover conditions on the Coweeta Creek watershed are excellent. A dense mixed hardwood forest is dominant, with scattered pines only on old fields or on the ridge tops at lower elevations. Although about 60 percent of the area had been cut over a quarter century or more before the Federal government acquired ownership, this part of the Coweeta now supports a second-growth forest, with the remainder still in old growth. Before being killed out by the blight, chestnut was the major species. Now about 80 percent of the Experimental Forest supports oak-hickory stands, 15 percent cove hardwoods (yellow poplar, northern red oak, hemlock), 5 percent Northern hardwoods (sugar maple, yellow birch, beech), and conifers. The forest is three-storied: large trees form an upper layer, small trees the second, and shrubs or herbs cover the ground.

In the Southeastern states rapid industrial growth and the expansion of many cities have created a rising demand for water supplies. Ground waters are believed to be progressively decreasing, and stream flow is becoming less uniform and dependable, while the quality of surface water drawn on for domestic use and industrial processes is deteriorating because of sediment and pollution.

We may well ask, To what extent is this situation due to land conditions, and what can be done to improve and conserve water as a basic resource? Specifically, what types of land use are most favorable to water resource conservation? Are we managing our municipal and industrial watersheds so as to obtain the maximum quality and quantity of water? These are the questions to which the Forest Service is attempting to find answers at Coweeta.

The long-range plan of research is directed toward four related fields:

1. To establish fundamental forest and water relations.
2. To furnish coefficients of runoff, infiltration, and water storage for different types of land use such as grazing, logging, and "mountain farming."
3. To develop principles for silvicultural management that will provide for maximum supplies of usable water of highest quality.
4. To work out feasible and practical methods of stabilizing the soil in logging operations, on road banks, and along streams and reservoir shore lines.

Up to now, the investigations have been mainly concerned with the first two fields.

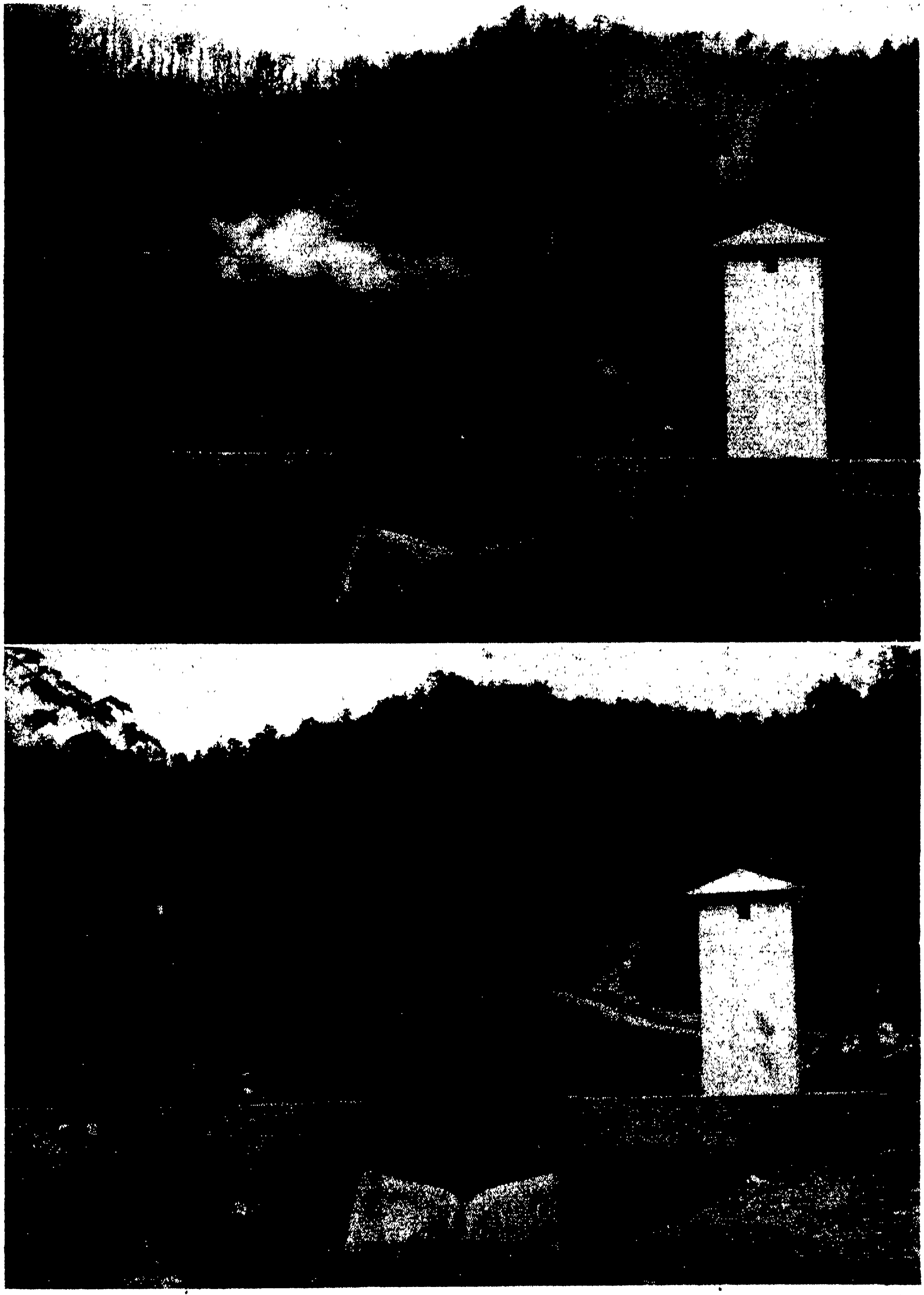
At present, the water behavior of 28 watersheds is being studied. Of these, 16 are being treated experimentally and 12 are held as control units and for future experimentation. On each watershed hydrologic measurements are taken by self-recording instruments, so that the amount and intensity of the rainfall in each watershed can be synchronized with stream behavior at the gauging station, with changes in the level of the ground-water table in observation wells, and with temperature, evaporation, and other climatic factors. Soil-moisture determinations are made periodically. Water temperatures and turbidity data are also taken.

The behavior of ground water is being studied by means of observation wells in which movements of the water table are recorded by automatic instruments. These records have demonstrated that water tables close to the surface may be quite sensitive to individual storms throughout the year, but deeper wells show significant replenishment normally during the late winter months.

Several treatments on different watersheds are being studied. It had been originally thought that a standardization period of ten or more years

CLEARING THE FOREST

PART OF THE WATERSHED AREA IS TO BE TURNED INTO A TYPICAL "MOUNTAIN FARM." FENCE ON RIGHT MARKS BOUNDARY OF WATERSHED. GAUGING STATION IN FOREGROUND. LOWER VIEW SHOWS SAME AREA IN SPRING FOLLOWING CLEARING. UNCUT TREES IN BACKGROUND INDICATE CHARACTER OF FOREST BEFORE CLEARING. RAIN GAUGES, GROUND-WATER WELLS, AND SELF-RECORDING METEOROLOGICAL EQUIPMENT ARE IN PLACE.

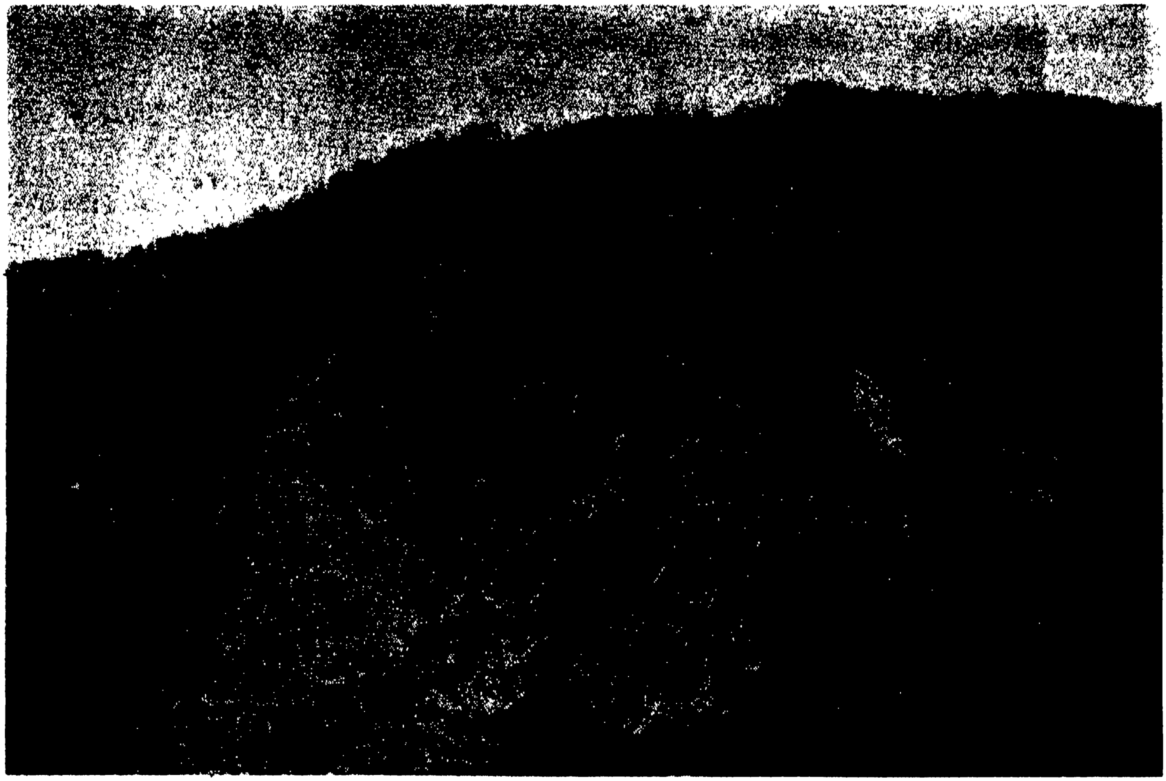


U. S. Forest Service

would be required before any treatment could be applied. It was soon discovered, however, that because of the pattern of precipitation and the large number of storms per year the stream behavior was fairly uniform basin by basin, storm by storm, and season by season. This, plus the areas used as a check, permitted investigators to compare each treated stream with itself and so cut in half the time required for standardization.

The results of the treatments have thrown a good deal of light on how the vegetative cover influences the production of water as well as the behavior and rate of discharge in the streams. With this kind of information at hand, it may be possible to suggest land use practices to attain different goals—such as where flood control is the primary objective, where water yield is the main consideration, or where regularity of stream flow is chiefly desirable.

The first watershed studies carried out on the Coweeta dealt with the effects of temporary cutting of all major vegetation. On two small watersheds (33 and 40 acres), the trees and understory down to broomstick size were cut to the ground early in 1941. All the cut material was left where it fell in order not to disturb the litter and humus. Before cutting, sufficient stream flow and climatic records were obtained so that the normal relations between rainfall and runoff could be established. On one watershed, the trees were allowed to sprout up again as they would following a normal cutting operation. On the other, the sprouts were cut back each year so that they never attained any appreciable size. A similar drainage area was left uncut to serve as a control unit.



SMALL EXPERIMENTAL WATERSHED AT COWEETA

ALL FOREST GROWTH WAS CUT DOWN TO BROOM-HANDLE SIZE AND ALLOWED TO REMAIN WHERE IT FELL. SPROUTS HAVE BEEN KEPT CUT BACK TO THE GROUND SINCE, WITH NO DISTURBANCE. AS A RESULT, THERE IS A WEALTH OF FAUNA AND FLORA, AND THE SOIL IS EXCEEDINGLY POROUS AND OF A BEAUTIFUL CRUMBLIKE STRUCTURE.

On stream flow, the effect of removing the vegetation was spectacular. In the first year after cutting, the total runoff was increased by 17 area inches, or 65 percent, without any indication that storm flow was augmented or the quality of the water impaired. The largest augmentation of flow occurred in late summer and fall. On the watershed where the sprouts have been kept down, this phenomenon has been repeated each year. On the area where the regrowth of vegetation has been permitted, the higher water yields have persisted, although becoming gradually smaller in amount.

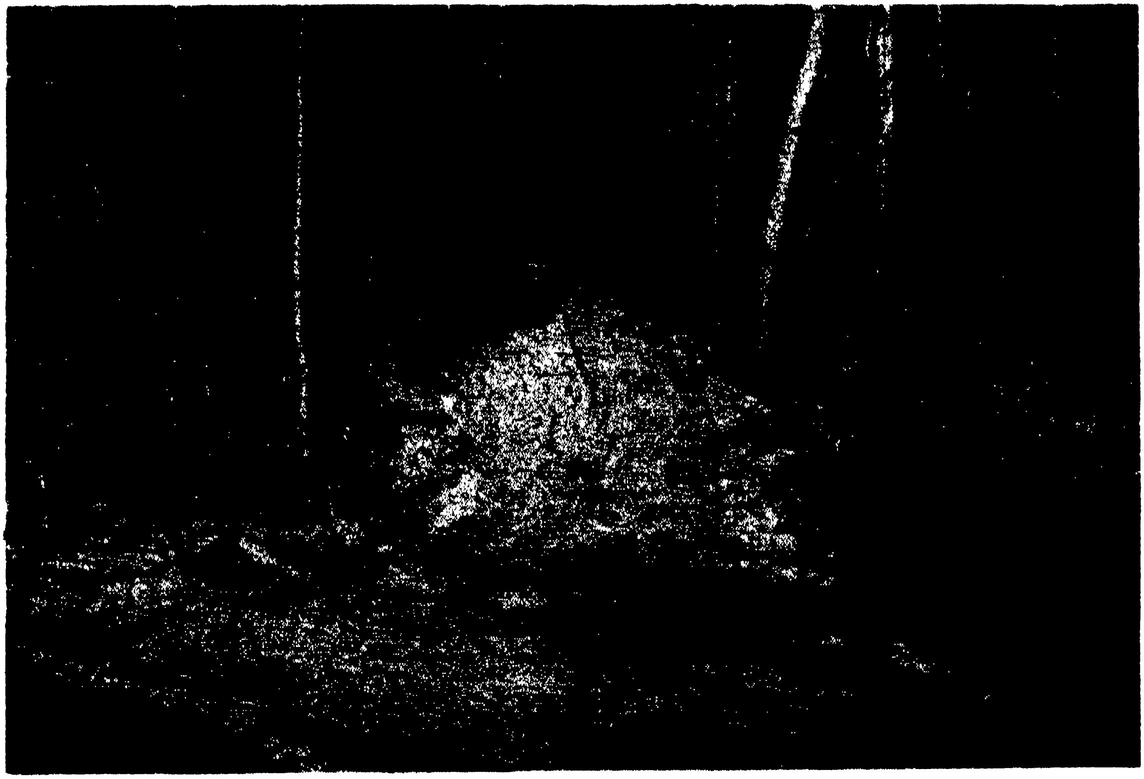
On another basin of about 22 acres, only the riparian forest was cut. Again no disturbance of the forest soil was permitted. Here, though the cutting was limited to a narrow strip of trees with their feet in the ground water and amounting to only 12 percent of the drainage area, there was a 20 percent rise in stream flow. Again the quality of the water was unaffected.

Foresters have long tried to get a fairly definitive statistical answer to the question of what effect the customary logging and skidding practices have on the quantity and quality of the water yield in our mountainous regions.

To get logs from the tree stump to the sawmill is usually difficult and laborious. The logs must be skidded to a point where they can be taken out of the woods. In the Eastern hardwood forest, they are usually loaded on trucks and hauled over roads that wind through the woods. In building and using these skid trails and truck roads, a great deal of soil is normally disturbed—litter and even humus are destroyed—and conditions favorable to erosion are created. Here is a common source of watershed deterioration that results, among other things, in the muddying of streams and fish depopulation.

At Coweeta a 212-acre watershed was cut and logged in 1942–43 by a contractor who was permitted to use the methods common in the Southern mountains. He cut whatever trees he wanted and got them out of the woods in any manner that appealed to him. Logging was resumed in 1945. A 2.3-mile truck road was bulldozed into the area in a location chosen by the contractor. Most of the profitable trees have since been removed from this watershed.

The effects of logging were soon perceptible in the quality of water flow, which deteriorated markedly. It was found that the logged watershed yielded an average turbidity of 95 parts per million, compared with 4 parts per million for the uncut control unit. The flow from the logged area ran as high as 6,000 parts per million in contrast to a maximum of 120 parts per million for the control unit. Moreover, the water from the logged water-



TYPICAL ROAD CONSTRUCTION ON COWEETA EXPERIMENTAL FOREST

THIS IS THE TYPE USED BY LOGGERS GENERALLY THROUGHOUT THE EASTERN MOUNTAINS. ONE SMALL STRETCH OF ROAD SUCH AS THIS PROBABLY RESULTS IN THE DESTRUCTION OF MORE FISH IN A WEEK THAN ALL THE FISHERMEN IN A COUNTY COULD DESTROY IN A SEASON.

shed was always laden with debris and never met the standard of 10 parts per million set by municipalities for drinking water. In addition to these adverse results, the mud flowing into the stream indicated that erosion was occurring on the logged forest. Most of this originated on the skid trails and the poorly built and maintained truck road. Erosion continued even after logging was shut down. The full effect of logging on stream flow, as distinguished from water quality, has not been completely analyzed, but there are indications that the peak flow has been somewhat increased.

These results seem to bear out the conviction, based on observations in other parts of the country, that logging is a primary cause of erosion and sedimentation of mountain streams. So far, all the advances in logging methods have been for the benefit of the operator, not for that of the forest or of the water. Those involved in studying water problems believe that the efficient planning of a logging job will result in savings to the operator as well as to water users.

Perhaps the most interesting small drainage area in the Coweeta Experimental Forest is that cleared for agricultural use in 1941. It has long been thought that mountain farming is harmful to the watershed. The experiments at Coweeta have provided supporting data.

A local farmer was induced to operate the 23-acre mountain farm as he saw fit. Conforming to the local pattern, on the more gentle, 30 percent slopes he grew corn and on the steeper slopes grazed cattle. For the first two years there was an increase of water production on the cleared watershed without much damage to the quality. By the third year erosion was in progress; in fact, by then the organic material in the cornfield was largely gone and the structure of the soil was breaking down so that infiltration began to decrease. Surface runoff then occurred with every rain. The peak discharge hit a high of 398 cubic feet per second per square mile during the rain of July 30, 1943, compared with a recorded maximum of 48 cubic feet per second per square mile before clearing.

Erosion on the tilled area became so prevalent that the main creek was discolored for a considerable distance downstream. Thus, before clearing, the rate of sediment movement based on accumulation in the catchment basin was 9.4 pounds per day. After clearing, it shot up to an average of 767.6 pounds per day in the period May 4–September 8, 1943.

After two years of tillage no corn was planted, and the fields were allowed to grow up to natural weeds; many seedlings and sprouts of yellow poplar and locust became established. The sediment flow was decreased but still was four to five times the volume noted before the area had been cleared and farmed. Corn was again planted in 1946, but the yields fell off greatly. They were increased in 1947 with the application of fertilizer. The damage to the watershed, however, persisted.

Grazing on a 145-acre forested watershed also produced proof of the damage that livestock inflict upon the soil. Compaction was prominent wherever there was any nutritious forage, and the undergrowth of palatable tree seedlings was practically wiped out. The soil became less porous and hence less able to absorb and store water. With the loss of the understory, wind was able to get under the crowns of the tall trees and blow away the litter.

A drainage basin of 39 acres was selected for a fire experiment. This watershed had never been cleared but had been cut over many years prior to the time the land came into Federal ownership. A new forest of oaks and hickories had come in, covering the area uniformly. No sign of fire could be found in the area, even on the old holdover trees.

The experimental area was burned over on April 14, 1942. The blaze consumed about four fifths of the litter on the ground; most trees less than 4 inches in diameter were killed and branches below 14 feet scorched. The larger trees suffered considerable damage when sizable areas of the cambium layer were killed.

So far as the watershed was concerned, the results of this experiment were inconclusive. Stream flow was seemingly not affected because, as is normal at this elevation in the spring, the soil under the litter was quite moist; consequently, the blaze did not alter the structure nor affect biological activity in the soil. Soon after the burn, sprouts and new herbaceous growth appeared. These served to protect the soil from the impact of subsequent hard rains. Inspection revealed, however, that the soil had been dangerously exposed by the reduction of litter and humus and that repeated fires would create unfavorable conditions for the percolation and storage of ground water. This area will be burned again, and probably another area will be chosen for a fall fire.

What have the results of the watershed treatments at Coweeta added to our knowledge of the behavior of water and its relation to the forest?

First, we have gained much information about basic hydrology, particularly a more realistic interpretation of forest and stream-flow relations in terms of watershed storage. It is now possible to explain the behavior of a stream on the basis of the source of its water; that is, (1) channel precipitation and overland storm runoff, (2) water detained in temporary subsurface channels, and (3) ground-water storage. As we have seen, different types of land use affect these three elements in varying measure.

Of especial significance from the standpoint of fundamental hydrology are the findings that relate to the balanced water economy of small watersheds. The results obtained substantiate the theory that in humid climates the precipitation recharge for any one year should tend to equal the discharge from the watershed in the form of runoff, transpiration, and evaporation, together with the changes in soil moisture and ground-water storage.

On the practical side, the Coweeta studies have suggested some of the measures to be used in beneficial watershed management. First, we have discovered that the mixed hardwood forest of the Appalachian area uses much more water, especially during the growing season, than had been suspected. Simply by removing the vegetation much more water became available for runoff. And, as long as the soil and litter mantle are not disturbed or dissipated, the additional water does not create flood hazards.

Second, Coweeta has demonstrated in a clear-cut fashion that any disturbance to the soil will change the watershed picture. Logging causes compacted skid trails, which become focal points for surface runoff and induce erosion. Livestock constantly trailing through the woods in search of forage tend to compact the soil and dissipate the leaf litter, thus stimulating quick runoff and attendant erosion.

PLOWING THE "MOUNTAIN FARM" WATERSHED AT COWEETA

The study of mountain agriculture showed that tillage rather quickly breaks down the structure of the soil. The crop yields tend to diminish as fertility declines unless large amounts of fertilizer are applied to the soil. The Forest Service plans to continue the farming of the tilled area at Coweeta until some kind of uniform runoff and erosion pattern is obtained, after which an effort will be made to determine how long it takes to re-establish a good forest soil.

The Coweeta investigations also suggest that a dense forest of a type that uses a great deal of water may be highly desirable for flood control purposes. Thus the litter and humus permit maximum infiltration in the soil, and during the growing season the forest tends to use up a large part of the precipitation that comes in the form of heavy storms. By drying out the soil, the forest provides storage space for subsequent storms so that the soil goes into the winter relatively dry.

The high water use by a dense forest, on the other hand, suggests that we can increase the supply of water for man's purposes by cutting out the heavy water users. This kind of treatment could be recommended to communities and industries that wish to augment their water supply.

The Coweeta experiments have clearly demonstrated that timber cutting and grazing seem to cause more damage than is usually imagined. The increase in stream turbidity alone justifies the elimination of these practices on highly critical Appalachian watersheds.

Then, too, erosion from any cause results in sedimentation and in discoloration of the stream. As a result of the discoloration, water temperatures are increased and the amount of light reaching the bottom is decreased. Higher temperatures decrease the amount of oxygen in the stream. Both the higher temperatures and the decreased oxygen result in fish, especially trout, moving out or, under extreme conditions, dying. Decreased light and oxygen react unfavorably upon the aquatic life upon which fish feed, so that a stream may become sterile. Mud and sand settling on the bottom also bury fish food and, in the spawning period, may ruin the nest or smother the eggs. Thus abuse of watershed lands affects the fish population.

In the final analysis, the adverse effects on the water supply resulting from forest land use are due to indifference and carelessness. The experiments at Coweeta suggest that our water problems, though staggering in some regions and localities, can be solved and that forest lands can be managed in the interest of the water resource. Though many questions remain to be answered, Coweeta has, so to speak, opened the hydrologic door.

◆

SOCIAL ASPECTS OF HOUSING

PAUL C. GLICK

Dr. Glick (Ph.D., Wisconsin, 1938) has been in charge of family statistics in the Population Division of the Bureau of the Census since 1940, except for the war period, when he was in the Research Branch, Information and Education Division, War Department. His article is the fourth in the series of sociological articles now appearing in* THE SCIENTIFIC MONTHLY.

THE first comprehensive inventory of housing in the United States was made by the Bureau of the Census in 1940. This census of housing provided information on the amount and quality of shelter in use or available for use by the American people. At the same time, this Bureau collected and tabulated the most detailed information ever obtained on charcteristics of the population of the nation. The results of the population census most directly related to housing are those on the number and types of persons who were living together as households. The facts established through the censuses of housing and population in 1940 have served as valuable bench marks with which sociologists and economists could compare the findings of current surveys made since that time.

In addition, historical series have been prepared by the Bureau of Labor Statistics on the number of new nonfarm dwelling units constructed in the United States and by the Bureau of the Census on the number and characteristics of households (household: a family or other group of persons, or a single person, occupying one dwelling unit). These series, extending back several decades, help to explain how the present housing situation has arisen. Furthermore, the series on number of households has been projected several years into the future, mainly on the basis of (*a*) the forecasts of population by age and sex prepared by the Bureau of the Census and the Scripps Foundation for Research in Population Problems, and (*b*) past trends in the tendency for persons of each age and sex to maintain a household. These projections have been widely used by analysts who were forecasting long-time needs in housing and in other fields where households are more appropriate units of measurement than persons. The Federal Reserve Board has also sponsored surveys of consumer finances on a nation-wide basis during 1946, 1947, and 1948, from which were obtained estimates of contemplated housing purchases during the year.

The number of dwelling units at a given time includes both occupied and vacant units. Thus, in April 1947, there were about 41.7 million dwelling units, of which 39.1 million were occupied and 2.6 million vacant.

Furthermore, within one household there may be more than one family group. In April 1947, for instance, there were about 39.1 million households, but at that time there were close to 4 million family groups (mostly married couples and parent-and-child groups) that were sharing the living quarters of someone else.

Trends in household growth and housing construction. During the 58 years since 1890 the increase in number of households, 221 percent, has been at a rate more than half again as high as the increase in population, 130 percent. The difference between these percentages of increase is a result of the factors that have caused the average size of household to decline sharply. Furthermore, the rate of household growth will continue to exceed the rate of population growth as long as the decline in household size continues (Table 1).

The lowering of the birth rate and the consequent decline in the number of young children living at home account largely for the diminution in size of household. The underlying reasons for the decline in the birth rate have been changing attitudes toward the value of having a large number of children, as our culture has been transformed from one that was predominantly agricultural to one that was predominantly nonagricultural and as standards of living have risen. A secondary factor that has generally been overlooked is the decline in death rates; more and more persons are surviving past the time when their children have left home, thus leaving a larger proportion of small households comprising one or two elderly persons. A third factor that cannot now be documented is a possible tendency toward more widowed and older single persons maintaining households of their own rather than living with relatives.

---

* This article was not prepared in the official capacity of the author and should not be interpreted as representing official estimates of the Bureau of the Census.

TABLE 1

POPULATION, HOUSEHOLDS, AND POPULATION PER HOUSEHOLD, FOR THE UNITED STATES: 1890–1960

CENSUS, SURVEY, OR FORECAST	POPULATION (thousands)	HOUSEHOLDS (thousands)	PERCENTAGE INCREASE SINCE PRECEDING CENSUS OR SURVEY		POPULATION PER HOUSEHOLD
			Population	Households	
1890[1]	62,948	12,690			4.9
1900[1]	75,995	15,964	20.7	25.8	4.8
1910[1]	91,972	20,556	21.0	26.9	4.5
1920[1]	105,711	24,352	14.9	20.2	4.3
1930[1]	122,775	29,905	16.1	22.8	4.1
1940[1]	131,669	34,949	7.2	16.9	3.8
1948[2]	145,085	40,720	10.2	16.5	3.6
1960[3]	159,909	45,100	10.2	10.8	3.5

[1] Decennial census.
[2] Sample survey of civilian population as of April 1948.
[3] Forecast for July 1960.

Another trend of central importance has been the cumulative difference between the net growth in number of households and the number of new dwelling units constructed. Figures for nonfarm areas in the United States for the period since 1930 are shown in Table 2. (Data on housing construction are not available for farm areas.) These figures show that the increase in nonfarm households during the 1930s was nearly 2 million greater than the number of new nonfarm dwelling units constructed. This difference is, in part, a consequence of the fact that the number of dwelling units built in the 1920s was greater than the increase in households by well over a million; but a considerable amount of the apparent excess in construction during the 1920s was absorbed by those who were short of housing in 1920. From 1940 to 1948 the number of new housing units constructed was less than the increase in households by another 1.8 million, making a difference of about 3.7 million for the period 1930–48. The latter figure is approximately one tenth of the 34 million nonfarm households in April 1948.

The main way by which the growth in number of households could thus have exceeded the number of new housing units constructed was more intensive use of previously existing living quarters. This more intensive use took such forms as the subdivision of units into a greater number of smaller units, the continued habitation of units that would ordinarily have dropped out of the housing market, and a sharp reduction in the number of vacant dwelling units. These types of adjustment to the current housing shortage will be discussed at greater length below. An additional factor, which probably explains only a small part of the excess in nonfarm household growth, is the continued use of units in the open country that were formerly classified as farm units but that are now classified as nonfarm units because agricultural operations are no longer carried out on the land.

Population growth. In settled times the greatest source of demand for new housing in this country is population growth. In such times, there is an increase each year in the number of households that desire and can afford separate housing space. During the 1940s, for instance, the expected increase in the number of households in the United States would have been about 500,000 per year if the employment level and the marriage rates had not been unusually high. Actually, during this decade the average annual increase in number of households will likely be half again the "normal" number.

During the 1950s, however, the expected increase in number of households on account of population growth is somewhat below 500,000 per year because the number of young people reaching the age for establishing new homes will reflect the low level of the birth rates during the 1930s. This source of housing demand during the 1950s could readily be more than offset, however,

TABLE 2

NET INCREASE IN NUMBER OF NONFARM HOUSEHOLDS AND NUMBER OF NEW NONFARM DWELLING UNITS CONSTRUCTED, FOR THE UNITED STATES: 1930–48
(Numbers in thousands)

PERIOD[1]	NET INCREASE IN NUMBER OF NONFARM HOUSEHOLDS		NUMBER OF NEW NONFARM DWELLING UNITS CONSTRUCTED		DIFFERENCE	
	During Period	Cumulative	During Period	Cumulative	During Period	Cumulative
1930 to 1940	4,646	4,646	2,734	2,734	1,912	1,912
1940 to 1945	3,205	7,851	2,334	5,068	871	2,783
1945 to 1948	2,766	10,617	1,861	6,929	905	3,688

[1] For households, dates are as of midyear for 1930, 1940, and 1945 and April for 1948; for dwelling units, all dates are as of January 1.

by demand arising from the large number of families that are now inadequately housed.

Practice of sharing dwelling units. In addition to the normal growth in population, the practice of sharing other persons' living quarters by family groups that would prefer, and could pay for, separate quarters must be taken into account. In 1930 there were about 1.5 million married couples not maintaining their own households; most of these couples were members of relatives' households or were living as lodgers. By 1940 this figure had increased to nearly 2 million and by 1947 to nearly 3 million. By way of caution it should be noted that, on the basis of surveys of housing intentions conducted by the Bureau of the Census, it is known that a substantial proportion of married couples living with others do so voluntarily, for such reasons as financial inability to maintain a separate home and convenience in the care of young children or of aged persons. Yet the same surveys lead one to assume that about one half of the nearly 3 million couples not maintaining a home of their own constitute active housing demand.

In this connection mention should be made of the more than 1 million family groups, other than married couples, that were not maintaining separate households in 1947. A substantial majority of these groups consisted of parent-and-child groups, most of which were living with relatives. With few exceptions these groups were not financially independent and consequently did not constitute an important factor in the demand for new housing.

Practice of subdividing dwelling units. In addition to family groups that are known to be living temporarily with others only because suitable housing is not locally available, there is a large number of family groups who have chosen to live in small "converted" units because these quarters generally provide more privacy and the facilities for preparing meals at home. These units satisfy the criteria of separate households, but the occupants in many cases desire, and are financially capable of paying for, more adequate living space.

A converted structure is one that contains a larger or smaller number of housing units than the number for which it was originally built. The most common type of conversion is that of subdividing a structure into a greater number of smaller units, but in some cases, of course, the opposite process of combining two or more smaller units into one larger unit does take place. Many structures that have been converted had been built several years ago when families were much larger, on the average, than they are today. The units into which they have been divided often combine, therefore, the disagreeable features of cramped quarters and obsolescence. The decline in the lodger population since 1940 is indirect evidence that during recent years rooms or suites of rooms in many structures once used as lodging houses for unattached individuals have been equipped with plumbing and cooking equipment and rented as apartments. Furthermore, some structures not originally built for residential purposes have been converted into living quarters, but the number is probably inconsequential.

It would be unfair to leave the impression that all converted units are substandard; some of them provide ample and comfortable quarters for a small family group and therefore constitute a permanent addition to the supply of separate housing units. In many cases existing structures have been enlarged in order that each of the converted units might have adequate room. Moreover, once a household has supplemented its income by renting a part of its living quarters to another family, there is a reluctance to discontinue this additional source of income. But in so far as the current occupants of converted quarters desire and can afford more ample and modern quarters, they are a factor in the active housing market.

No attempt on a national scale to measure the net effect of "conversions" (units gained minus units lost) has been very satisfactory. An approximation of the net gain during recent years in number of dwelling units attributable to conversions can be made, however, by subtracting from the increase in the number of households the number of new dwelling units constructed and the amount by which the number of vacant dwelling units has decreased. A calculation of this type yields an estimate of about 1 million converted units added in nonfarm areas during the seven years between 1940 and 1947. Since the 1947 data are based on a sample survey, however, the estimate is subject to sampling variability. A similar calculation cannot be made from census data for the 1930–40 period because the 1930 census did not show the number of vacant units. However, an estimate of 2,392,000 nonfarm vacant units in 1930 was reported by M. H. Naigles, of the Bureau of Labor Statistics, in the April 1942 issue of the *Monthly Labor Review*. A comparison of this estimate with the 1,903,000 nonfarm vacant units as shown by the 1940 census indicates a decrease of about half a million in the number of vacancies in nonfarm areas between 1930 and 1940—a period when construction of new dwelling units fell nearly 2 million below the increase in number of households. The same article contains an estimate of 1,060,000 units added by conversion (by remodeling of structures or in unremodeled housekeeping units) during the 1930s.

Continued use of substandard housing. Far more numerous than either the married couples living doubled up or the households living in converted units are the households living in substandard dwellings that are defective because they are temporary, dilapidated, hazardous, or too small. In the following paragraphs are presented averages of estimates, made by housing authorities in seven private and public agencies, of the number of substandard units in the United States today plus those units expected to enter this category during the next 10 or 15 years. The estimates of these and other authorities are presented in a report entitled *The Housing Situation—the Factual Background,* prepared by the Housing and Home Finance Agency for the National Conference on Family Life held in Washington, D. C., in May 1948.

Many of these estimates are for nonfarm areas only. One reason why estimates for farm areas are often neglected is the fact that measures by government to improve farm housing would be complicated by numerous administrative difficulties. The main type of housing needed in farm areas is new replacements for the most seriously substandard units in existence today, since population growth in farm areas is at a standstill. This type of need, however, is relatively great.

An average of the most conservative estimates made by the authorities referred to above who indicated needs for replacing substandard farm units was about 1.5 million. In addition, about 4.5 million dwelling units in nonfarm areas, again according to an average of the most conservative estimates presented by the authorities referred to above, are now substandard or are expected to become substandard during the 1960s. Where several estimates were given by one authority, the lowest one was selected. This was done because the higher estimates may not have taken full account of the possibilities of renovating many dwelling units that are now substandard. The progress already made during the 1940s in the reduction of the number of dwelling units with characteristics often used as indexes of substandard housing can be seen from the results of sample surveys conducted by the Bureau of the Census; many, if not most, of the authorities whose estimates were averaged in arriving at the above figures did not have the benefit of those results when they made their estimates. It should be emphasized, however, that the estimates of the number of substandard units just cited do not represent official estimates made by the Bureau of the Census.

Estimates of substandard housing naturally reflect opinions regarding what constitutes adequate ends toward which to aim. The estimates are correspondingly varied. Most people would probably agree that the average estimates represent worthy objectives for achievement by 1960, provided certain contingencies are met. The act of adding 6 million new dwelling units to the supply over and above the number needed to meet the basic needs arising from population growth, current doubling, and the current vacancy shortage, would require provisions for effective removal from the market, probably by demolition, of most of the units they were intended to replace. Although this and many other contingencies could probably be met if a sense of urgency in the matter were felt by those who would have to implement the program required to provide that much new housing, it is beyond the scope of this paper to speculate as to whether that sense of urgency will actually develop.

It is relevant, however, to point out a few of the criteria available for use in arriving at an estimate of substandard housing in the nation as a whole. Such criteria are listed in Table 3, together with the number and percentage of all dwelling units in the United States in the category in 1947, according to the results of a national sample survey conducted by the Bureau of the Census:

TABLE 3

Criterion	Number of dwelling units	Percent of all dwelling units
In need of major repairs	4,120,000	9.9
In need of major repairs or (in urban areas) lacking both private bath and flush toilet	6,070,000	14.6
With 1.51 or more persons per room	2,367,000	6.1
With 1 or more married couples sharing the living quarters of the head of the household	2,513,000	6.4
With rental under $20 (nonfarm tenant-occupied units only)	4,140,000	9.9

Of course there is a great deal of overlapping among dwelling units with the above characteristics, which can be demonstrated by the results of cross-classifications not shown here. Furthermore, any one criterion taken alone would not be sufficient evidence that the unit should be replaced. Many units would be adequate if renovated, enlarged, or used by a smaller family. On the other hand, these criteria do not reveal the number of units with such other defects as being poorly lighted, too closely spaced, too far from one's place of employment, or situated in otherwise adverse environments.

A very promising method for evaluating the quality of housing in segments of a city of metropolitan area is described in an article by Allan A. Twichell in the June 1948 issue of the *American Sociological Review.* The method he presents was developed by the American Public Health Association and takes into account not only a series of measurable characteristics of the dwelling unit but also another series of quantifiable data regarding the environment in which the unit is located. By such an appraisal technique any single dwelling unit or any neighborhood of dwelling units can be placed at a point in the scale that includes all units surveyed. It probably would not be feasible, however, to make the necessary expenditure to obtain such data on a nation-wide basis.

Need for reasonable number of vacant units. Authorities in the field of housing maintain that 3-5 percent of the dwelling units should be vacant at a given time in order to permit free movement of families in their adjustment to changing needs for living space. The lower estimates are generally used when the types of vacancies referred to are restricted to effective vacancies, that is, vacancies that are for sale or rent and habitable throughout the year.

It should be emphasized that the total number of vacant dwelling units is usually in considerable excess of the number that is in the effective housing market. Thus, estimates based on a nationwide sample survey conducted by the Bureau of the Census indicate that, of the 2.5 million vacancies in 1947, only one out of every six was habitable, nonseasonal, and available for sale or rent. Only 1 percent of all dwelling units in 1947 were vacant and met these criteria, and four tenths of those vacancies were on farms. Thus, the quality and type, as well as the availability of vacant units to families in need of them, must be taken into account in appraising the vacancy situation.

The vacancy shortage in recent years has been aggravated by the widespread movement of the population. Only 44 percent of the heads of households in 1947 were still living in the same house they had occupied in 1940. There were sweeping migrations of people to distant war jobs, and relatively few of them returned after the war. Migration out of the South and to the West was especially heavy. The population of California increased 42 percent from 1940 to 1947. There were 37 percent fewer nonwhite households on farms in the South in 1947 than in 1940, and outside the South nonwhite households increased by 50 percent.

Because of the nature of these large-scale movements, an increasing proportion of the vacant dwelling units are on farms, and a decreasing proportion in urban areas where the demand for housing is far more intense. In addition, the population of metropolitan districts has for decades shown a continuing preference for dwellings in suburbs. This tendency has helped along the obsolescence of homes in downtown areas and kept vacancy rates low in suburbs.

To these observations we may add that nearly one half of the vacancies in 1947 were seasonal dwelling units, such as cottages in resort areas quite remote from places of employment, and one tenth were classified as uninhabitable. A substantial proportion of the remaining vacancies were assuredly substandard. The foregoing remarks should make it apparent that the consideration of local, as well as national, conditions is of utmost significance in a thorough analysis of vacancies.

If effective vacancies were defined as habitable, nonseasonal units for sale or rent, and if 3 percent of all dwelling units in urban areas of the United States as a whole in 1947 had been in the effective vacancy class, about 750,000 such vacancies would have been expected. Actually, the number was only about one fifth that large. Furthermore, if each housing market area taken separately —such as each metropolitan area—were to have an effective vacancy rate of 3 percent, the number of additional effective vacancies needed might well exceed 1 million. Furthermore, when the housing situation does improve and the number of vacancies increases, many of the vacancies coming into the effective housing market will be the substandard units that have been occupied only on a temporary basis during the most acute phases of the housing shortage.

Losses by demolitions, fires, floods, etc. This final factor to be discussed is relatively minor, since available estimates indicate that only about 40,000 dwelling units are lost per year by demolitions or catastrophes. If estimates of housing needs for a long period are being made, however, an item is generally entered for such losses. No allowance is made in this figure for large-scale demolition projects accompanying the replacement of substandard units.

In this paper the types of factual data available to sociologists and research workers in related disciplines for appraising the current housing situation have been presented. These data provide a basis for estimating the amount of additional housing needed immediately and in the long run to meet certain objectives. These needs and some of their implications may be briefly summarized as follows:

1. Persons who have made estimates of our housing requirements are in general agreement that housing construction on a scale exceeding that in our peak years (about 900,000 units) will have to be maintained for many years if the general level of housing conditions is to improve substantially. An average of the estimates of over-all housing needs, made by seven authorities referred to above in the discussion of substandard housing, is about 12 million nonfarm dwelling units and about 2 million new farm units needed between 1948 and 1960, with the estimates varying according to the degree of improvement visualized.

2. If 14 million new dwelling units were constructed during the next 12 years, provision could thereby be made for:
   - 5 million units for households added through population growth;
   - 1.5 million units for family groups now doubling up and seeking separate living quarters;
   - 6 million replacements for occupied units that are now substandard (including substandard converted units) or that will become substandard during the period;
   - 1 million increase in urban vacant units for sale or rent and habitable in all seasons; and
   - 0.5 million normal replacements for units demolished or destroyed by fire, floods, etc.

3. How nearly such an objective will be attained depends on many contingencies relating to the maintenance of active housing demand. If employment levels remain high, if prices for housing construction fall to a more favorable level in relation to family incomes, if much more rental-type housing becomes available, and if most of the substandard units to be replaced are demolished or otherwise completely removed from the housing market, the demand for housing will probably remain high. The fact remains, however, that a significant proportion of the families in substandard housing units will undoubtedly be unable by their own means to provide standard housing for themselves. The extent to which such an objective as outlined above will actually be realized, therefore, will depend on economic and political forces that no one is in a position to forecast with confidence.

4. An objective appraisal of the current housing situation should include an acknowledgment that many aspects of the problem cannot be readily quantified. The physical structure of the house or apartment and the environmental setting might well pass all the statistical tests and still be undesirable living quarters for a particular family or unattached person in need of housing space. A vacant unit may be ruled out of consideration by the family because of the ethnic composition of the neighborhood; the unit may be unsuited to the needs of the family during its present phase of the family cycle; or the unit may be for sale only, whereas the family prefers to rent during the current phase of the business cycle. It would be very easy to extend this list of real but unmeasured factors. The existence of these problems, however, makes certain elements in the housing situation, such as the number of needed vacant units, most difficult to estimate with much accuracy.

5. The universal need for good housing in desirable neighborhoods has innumerable personal, family, community, and national implications. That social adjustments are conditioned to a large degree by the quality of housing is so well accepted that it need not be elaborated. Suffice it to say that substantial improvements in housing would reduce the irritations that arise out of living under crowded conditions and would also reduce the deterioration of physical and mental health that is often associated with living in hazardous and otherwise substandard structures. In short, improved housing would lay a foundation that would make the treatment of many social problems far more simple.

◆

CHARLES BABBAGE: A SCIENTIFIC GADFLY

CHARLES F. MULLETT

Dr. Mullett (Ph.D., Columbia) has written extensively for scholarly journals on science, religion, and imperialism. After teaching in New York and Indiana, he went in 1925 to the University of Missouri, where he is still professor of history.

SCIENCE has had its heretics no less than religion. Some have disputed the basic premises; others, particular trends; still others have attacked those obstacles that restrict its potentialities. Such a one as this was Charles Babbage (1792–1871), whose frequent stinging assaults a century ago derived from the decline of science in England and the causes that gave rise to that decline. Had he stood alone, he would have deserved at most a footnote, but his convictions, if not his exasperation, were shared by many, laymen as well as eminent scientists.

The son of a banker, Babbage became one of the shrewdest mathematicians and most distinguished scientific mechanicians of his day. A sickly childhood and desultory education permitted him to indulge his "inveterate habit of contriving tools" and his passionate fondness for algebra, so that when he entered Trinity College, Cambridge, in 1811, he had already acquired a knowledge of the subject far beyond that of his tutors. With John Herschel (1792–1871) and George Peacock (1791–1858) he strongly preferred the Leibnitzian notation to the awkward Newtonian calculus, and the three founded the Analytical Society (1812), whose transactions Babbage would have published under the title "The Principles of pure D-ism in opposition to the Dot-age of the University." When, in 1816, the three translated the treatise on calculus, founded on Leibnitz, by the celebrated François Lacroix (1765–1843), Babbage also suggested periodical meetings for the propagation of *d*'s and the consigning to perdition of the *dot* heretics. He graduated B.A. from Peterhouse in 1814, and M.A. in 1817. Between 1815 and 1817 he presented three papers before the Royal Society, to which he was elected in 1816. In 1820 he produced some original work in Leibnitzian calculus and helped found the Astronomical Society; five years later he was at work on magnetism.

Meanwhile he had for years been working at the instrument that was to bring him his greatest fame, a calculating machine, much more complicated and valuable than earlier ones. Encouraged by the distinguished physicist William Wollaston (1766–1828), he had by 1819 made considerable progress. A small engine, contrived between 1820 and 1822, received high praise from many sources, and the government's interest prompted Babbage to work several years on the larger machine. Then misunderstandings over the work led to delay. During the interval Babbage evolved some new ideas, but the government did not respond, and it remained for others to apply the ideas he had ingeniously worked out. Nevertheless, in the opinion of those qualified to judge, the tools alone that Babbage had produced justified the government's expenditure of several thousand pounds.

During the same years Babbage engaged in other enterprises. While abroad in 1828 he got the materials that went into his most complete book, *On the Economy of Machinery and Manufactures* (1832, 4th ed., 1835), which was translated into four languages as well as reprinted in the United States. He shared extensively in the founding of the British Association for the Advancement of Science and the Statistical Society; he contested a parliamentary seat twice on the Whig ticket; he kept extensive workshops annexed to his house and produced some practical inventions; he was elected to many academies. All the while he carried on controversies that steadily bordered on the vituperative, whether the subject was street nuisances, the dereliction of the government, or the decline of science in England. To this last he contributed a full-length volume, *Reflections on the Decline of Science in England and on some of its causes* (1830), and chapters in his study of the economy of machinery and manufactures and in *The Exposition of 1851; or Views of the Industry, the Science, and the Government of England* (1851). No doubt he voiced the same views elsewhere, but the essential core can be found in these three.

According to the Preface of the *Reflections* Babbage was solely inspired by a desire to serve English science. In England few persons pursued science for itself. Moreover, although Continental scientists kept up with English discoveries, Englishmen were usually ignorant of Continental advances. Newspapers, magazines, reviews, and pamphlets had attacked the conduct, officers, and publications of scientific societies, but these criticisms were regarded as personal and therefore dismissed, when they were really professional and might have done much good. In consequence England fell below not only its political equals but its political inferiors. To make the situation even more appalling, the country was everywhere distinguished for utilitarian ingenuity.

Although Babbage was registering his own convictions, the *Edinburgh Review* had anticipated him by a dozen years. In 1816, in reviewing the *Principles of Fluxions,* by Archdeacon William Dealtry, F.R.S. (1775–1847), it commented sharply on the state of science, particularly mathematics, in England. The defects of the book under review went far beyond its own specific shortcomings. They consisted not in teaching falsity but in not teaching what was known. By laying down only those principles necessary for its particular problems, the book conveyed the impression of completeness and mastery of all problems to which the calculus could be applied. Unfortunately, a student, even after having mastered this whole treatise, could not grasp the first six pages of Laplace's *Celestial Mechanics.* The author mentioned no new discoveries. He set forth nothing that had not been known a century ago; more than this, he did not even relate all that was known at that time. Foreigners were omitted, and Newton received only superficial treatment.

The deficiencies of the volume became forcibly apparent on comparison

> with any elementary treatise of the same size, and on the same subject, that has appeared within the last fifty years in France, Germany or Italy. The success which the work has nevertheless had, the rapidity with which it has arrived at a second edition, and the support it seems to have met with in the University where the mathematical science of this island is supposed to be concentrated, are abundant proofs, that, in the higher mathematics, we have not of late made the same progress as the neighbouring nations. It is certainly a curious problem with respect to national genius, whence it arises, that the country in Europe most generally acknowledged to abound in men of strong intellect and sound judgment, should, for the last 70 or 80 years, have been inferior to so many of its neighbours in the cultivation of the science which requires the greatest and most steady exertions of the understanding; and that this relaxation should immediately follow the period when the greatest of all mathematical discoveries had been made in that same country. This is a paradox not altogether impossible to be explained, and to the consideration of which, having no room at present for such a discussion, we shall be glad hereafter to return.

Three years later the *Edinburgh* did return to the subject. The reviewer of Robert Woodhouse's (1773–1827) *Elementary Treatise of Astronomy* noticed the great progress of physical astronomy on the Continent and the absence of the same in England. He found the explanation in the too-great reverence for Newton, which had kept back the introduction of the calculus into England. To know the advances of neighbors it was necessary to know their language, not of speech alone, but of science. Moreover, whereas in England people promoted general knowledge and neglected technical knowledge, in France they promoted both. Pensions and honors were heaped upon their discoverers, but in England the only reward was the opportunity to read a paper to the Royal Society, which itself had held out little encouragement to mathematical learning. Perhaps the *Edinburgh* might also have noted the long shadow of Newton: men were busy filling in his lacunae and too much in awe to enter where he had not trod. Not to be disregarded also were the inadequacy of university instruction, the devotion to utility, and the character, not merely of the Royal Society, but of so many learned or professional societies in England.

Babbage himself attacked the neglect of science in the educational system. Some improvement had taken place at Cambridge in the preceding fifteen years, but not much. A university degree ought, he said, to be the pledge of a certain quantity of knowledge; the progress of society warranted that the character of that knowledge would change. Degrees should be awarded only to honors students and should keep pace with the needs of society. To supply curricular deficiencies Babbage outlined four areas of concentration in three of which science formed the backbone: Modern History, Laws of England, Civil Law; Political Economy and the Applications of Science to Arts and Manufactures; Chemistry, Mineralogy, Geology; Zoology and Botany, each including Physiology and Anatomy. A student should take only a limited number of examinations, he thought, in order to distinguish himself in those he did take. By such means Babbage hoped to augment scientific knowledge.

He hoped also to increase the inducements to individuals to cultivate science and thus attract first-rate minds. Because the pursuit of science was not in England a profession, government and public alike failed to discriminate between original and hack researchers, and amateurs were often chosen for professional tasks. Fortunately, law, in attracting the highest talents, and medicine and divinity, in attracting good talents, had some powerful allies of science. Of political encouragement there was little, a lack justified by many on the grounds that the public was the better judge and that government rewards were smaller than business profits.

Long intervals elapsed between the discovery of new basic truths in science and their successful application. Men had constantly to remember that the mathematical truths of one generation formed the bases of profound physical inquiries in the next and were of practical use to all in the third. At the time of the first discoveries, however, existing apparatus did not permit their mechanical application.

For one original discovery there were many applications. One should reward the application, yes, but certainly not less the person capable of setting forth the original truth. Unless this were done, the contriver of some invention would alone derive substantial profit, whereas he on whom the invention basically depended went unrewarded to his tomb. Great discoveries did not emerge from the universities because professorships inadequately rewarded the "pure" scientist. Moreover, they entailed drudgery, a drudgery Galileo could escape but not John Dalton, who, incidentally, was eventually granted a government pension through Babbage's own plea.

The advance of science risked some danger from government interference—the ignorance of the judges, the indifference to qualified opinion, and the subservience of the qualified opinion itself. Such a situation had permitted wasteful expenditures on certain publications through ignorance or indifference. Hence government subsidies had to be carefully sought—and carefully spent. It was a commentary on the state of science in England that an English translation of Laplace's *Celestial Mechanics* had come from America. Be it remembered also that Laplace himself had dedicated his writings to "that extraordinary man" Napoleon, during whose reign "the triumphs were as Eminent in Science as they were splendid in arms." In Continental countries savants were well rewarded. Laplace (1749–1827), Carnot (1753–1823), Chaptal (1756–1832), and Cuvier (1769–1832) had all held high office in France; similarly, Prussia had honored the two Humboldts, Alexander (1769–1859) and F. W. (1767–1835); Modena had made Rangoni (1744–1815) its Academy president and minister of finance and public instruction; Tuscany and Saxony alike had rewarded well their eminent scientists. England herself had not always been indifferent, but now to suggest an appointment such as Newton's at the Mint would have brought only smiles.

Learned societies, which should be the font of scientific rewards, were often remiss. At one time they had been a practical necessity, especially in the matter of supplying apparatus, and they had otherwise stimulated research. Election to them should be a distinction, but in England that was scarcely the case, in comparison with several other countries. France, with a population one third larger than that of England, elected only 75 members to its distinguished academy; Prussia and Italy, with populations only slightly more than half as large as England's, elected 38 and 40 members; but England elected 685 members to the Royal Society. Where one Englishman in 32,000 could be a Fellow, one Frenchman in 427,000 and one Prussian or one Italian in 300,000 could be academicians. No country equalled Prussia in bestowing encouragements on science, but others fell only slightly behind. The French awarded titles of nobility to distinguished scientists, and a French report of English parliamentary proceedings referred to the intellectual distinction of a particular speaker; an English report, had it stressed anything, would have emphasized his wealth. France gave great financial awards to science, but England allowed such independence of opinion to go unrewarded.

In considering the general state of English learned societies, Babbage found cooperation and the division of labor as practicable in science as in industry. The want of competition stultified. Many different scientists, appreciating this deficiency in the Royal Society, had seceded, despite the bitter opposition of the parent society, whose attention to their specialty was insufficient to their wants and needs. In this way the Linnaean, Geological, and Astronomical societies came into being. In the case of the last, so successful was the persecution by the Royal Society that within six years it had become widely known and respected throughout Europe. By contrast, the Zoological and Medico-Botanical societies were founded without Royal Society opposition, and the latter totally failed.

Babbage listed thirteen learned societies, their fees, and their letters. The fees ranged from £ 21 for the Medico-Botanical Society to £ 50 8*s*. for the Antiquarian Society. Should a person desire he could become a comet with a tail of over forty letters, at the average cost of £ 10 9*s*. 9*d*., and a farthing per letter. Among the societies Babbage picked out half a dozen for special comment. The Geological Society was doing pretty well, but its papers needed more finish; the Astronomical Society was a great success from any angle—excellent papers, open meetings, and rotating presidents; the Asiatic Society had done good work in translating rare works; the Horticultural Society had had a good record but now was ridden to death by its large debt. On the other hand, the Zoological Society was a source of great amusement. The Medico-Botanical Society had gone after social lights, and its quackery rendered *expulsion* its highest honor—if it would only commit suicide it would exemplify the sublimest virtue.

It was against the Royal Society that Babbage brought up his heaviest guns. Properly he peppered the quacks with satire; but the Royal Society had played traitor to learning, and sorrow as well as anger inspired his comprehensive attack. Therefore, he described the mode of becoming a member, the administrative organization, the medals and lectureships, the causes of the present state, and a proposal for reform. Informing all these topics was an intense conviction that until the Royal Society was reformed science must remain at a comparatively low level.

The mode of election made it relatively easy for any man so desiring to become a Fellow. Three members put up his name and at the end of ten weeks—if he had done nothing whatsoever—he was elected. If he had written something his chances were much slimmer, for then some Fellow's would assuredly inquire if his work showed sufficient merit to warrant his election. Blackballing, prejudiced or otherwise, was not final; neither was it the only means of keeping out the real scientist. The £ 50 fee excluded many such, though not, of course, members from the gentry.

The president and secretaries came in for sharp attack. President Davies Gilbert (1767–1839), an antiquary, was held to be, after some years as treasurer—to which office he seemed anxious to return—"by far the most fit person" available. It was true, said Babbage, that he was "a most amiable and kindhearted man," although in office, instead of raising the level of the Society's achievement or indeed of living up to his character, he had grasped despotic power and remained in the office to which he was elected as an *ad interim* figure and for which he had no competence whatever. The secretaries, Peter Mark Roget, M.D. (1779–1869), and Captain Edward Sabine (1788–1883), were incompetent. The presence of the second undoubtedly prompted the additional remarks concerning the propriety of a military man holding any office in the Society. It was not that such a person was necessarily inefficient but rather that habits of obedience and command rendered him unfit for such offices. The soldier must cover up his mistakes, the scientist must freely admit his. Babbage, however, hastened to add that no man should take criticism as personal, perhaps because Sabine was undoubtedly a superior member of the Society. The council was inadequate, and the union of several offices in one man was bad practice. The Society frequently showed a deplorable lack of integrity in learning and administration alike: it claimed other men's work as its own, and its written minutes by no means revealed the true proceedings. The funds of the Society were grossly misused.

Nowhere did these latter deficiencies appear more glaringly than in the awards of medals and lectureships. The members voted one another the medals, or else voted them to a proper recipient long after a specific achievement and in direct violation of the avowed purpose behind such medals, that is, to excite competition. The lectureships went year after year to the same men instead of being used to encourage some youngster. These criticisms undoubtedly had great validity. In particular, the Croonian Lecture, founded in 1738, deserved all the scorn that Babbage could direct at it, for from 1790 to 1829 the name of Sir Everard Home (1756–1832) appeared with monotonous repetition. In that interval only eight other persons, who between them gave twelve lectures, held the lectureship; on six occasions the lecturer and his subject were not recorded, and in one year (1828) no lecturer was appointed. All things considered, and perhaps not uninfluenced by Babbage's blast, no Croonian lecturer was appointed 1830-50. The Bakerian lectureship (founded 1775) did not show quite the same monotony, but for thirteen years, 1780-92, Tiberius Cavallo (1749–1809) was the lecturer. Afterward Samuel Vince (1749–1821) held it seven times in eleven years, and in following years the names of William Wollaston, Thomas Young, and especially Humphrey Davy appear frequently. Although no question attached to the merits of these three, the point made by Babbage had pertinence.

The cause of the low ebb of the Royal Society, then, was years of misrule, for which the responsibility must in part be laid at the door of the members who had failed to exercise their suffrages. It was of course true that they found it easy not to do so. A printed list of nominees, prepared by the president, was put in their hands, and for the most part no questions were asked. A coterie—a mutual admiration society—managed the Society. Any dissenter was silenced by the remark that either it was no concern of his, or, being his concern, he was the last who ought to raise a cry, or that he should wait until he cooled off (by which time he was told that it was too late), or that the rules were never adhered to anyway. Meanwhile the coterie surrounding the president and the beneficiaries of his patronage continued in charge.

To remedy those defects, which especially aroused his anger, Babbage printed a plan of reformation, a plan which indeed he and others, including such eminent members as Herschel, Wollaston, Thomas Young (1773–1829), and James South (1785-1867), had formulated three years before. A committee had been appointed to consider means of limiting the number of Fellows and improving the Society's prestige, but its members unfortunately had not agreed on the way of realizing these aims, and this, combined with the natural disinclination for change and the power of the administration, had rendered the committee's recommendations ineffectual. As far back as 1674 the Society had considered the ejection of useless Fellows as one means of improving scientific standards; in the subsequent interval an occasional voice had urged without effect the same policy.

Now Wollaston thought to reduce the membership to 400 by not replacing deceased Fellows. Babbage thought this process not only too slow but also discouraging to younger scientists who could add luster to the Society that they would never be able to join. He himself suggested a test of two or more printed contributions to the *Transactions*, which would reduce the membership immediately to 72; if three or more became the test, which he preferred, the number would fall to 51; and even if all contributors were retained, the Society would have only 109 fellows. At the time more than 600 members got credit for nothing. Babbage also wished to renovate the council and institute more democratic election and freer discussion of policies.

The committee report, presented late in the 1827 session, contained the core of these suggestions. Satisfied that the Fellows had increased out of proportion to the growth of population or scientific knowledge, the committee made membership, which was to be limited to 400, exclusive of royalty and foreigners, an object of competition among deserving persons. Using the contributions to the *Transactions* as a clue, the committee decided that until the number of 400 had been reached only 4 new members were to be admitted annually. These would be chosen on the basis of merit, with full knowledge of the candidates' qualifications. No one would be excluded by arbitrary blackballing, and no disgrace or recrimination would accompany failure to be elected. A new committee on finance would guarantee a more judicious expenditure of funds in the face of falling revenue. There should be more publicity to awards, more thought to council choices, and more critical selection of papers for the *Transactions*. These recommendations were to go to a committee for framing and then be jealously and zealously scrutinized by the whole Society.

Despite the eminence of the original committee, the thought that had gone into their report, and the undoubted need for reform, nothing whatever emerged. On June 25, 1827, the council recommended the report to "the most serious and early consideration of the Council for the ensuing year." The president thereupon loaded the council according to his prejudices, and the report was rejected without any discussion of its merits. No wonder that Babbage traced the decline of science to the Royal Society, a view shared by South, a moving spirit in the creation and membership of the committee, and, far more moderately, by Augustus Bozzi Granville (1783–1872). The first published a very sharp pamphlet on the matter in 1830. The second published *Science without a Head* anonymously in the same year and *The Royal Society in the Nineteenth Century* in 1836. Granville, however, in some measure sought also to refute, or at least to soften, the strictures of the other two. His works, especially the second, were abundant in facts not readily available, sparing in argument, and economical in conclusions and recommendations; nonetheless, he believed the Royal Society "the only scientific corporation in Europe . . . *en arrière* of the century we live in," and suggested some fundamental changes. All three men, plus the committee's report and the undoubted prestige of its members, no doubt influenced the reforms of the 1830s. To Babbage, however, the damage was done, and he seems never to have forgiven the Society, even though at a meeting on May 27, 1830, "the President announced, in a good tempered speech, that the discontented members, Mr. Babbage and Mr. South, were for the present appeased; and that Capt. Sabine, having been summoned to join his company in Ireland, was obliged to resign the Secretaryship" (*Gentleman's Magazine*, C, pt. i, 544).

This of course did not halt suggestions from Babbage, either in the *Reflections* or in later works, for the advancement of science in England. First of all, he maintained the Fellows of the Royal Society must express their opinions. The managing coterie triumphed, he believed, because many qualified persons expressed their opinions reluctantly and timidly, and silence was cited as approval. This reluctance no doubt resulted in part from the scholar's modesty, partly from deference to a great name. The coterie triumphed also because presidents remained long in office. Biennial presidents had proved successful in the Astronomical and Geological societies; each man was anxious to accomplish something worth while during his tenure, and no man stamped his personality on the society.

Outside influences sometimes benefited, sometimes damaged, the Royal Society. In the case of the Colleges of Physicians and of Surgeons, the tendency of their members to join the Society was a disadvantage. Although the *Transactions* occasionally contained medical papers of "very moderate merit," medical men introduced their professional jealousies. Moreover, medicine had too many aspects, and its practitioners too many interests, to contribute much. The Royal Institution had a mixed effect. Designed originally to cultivate the more popular and elementary branches of science, it had through the efforts of Davy and the decline of the Royal Society gained greater prominence than anticipated. It had a good influence in the diffusion of knowledge and a bad influence, for the Society, in that its officers and transactions did not normally meet the standard that ought to govern the Royal Society. If the situation were not controlled, wrote Babbage, the Royal Institution might ultimately dominate the Royal Society; and certainly no man should hold office in both.

Of the *Philosophical Transactions* Babbage complained that they contained no history of the Society and its work, but merely a meteorological register, a notice of medal awards, and the papers selected by the council. There was not even a list of the officers and council; (but then to include this last would rob the porter of the half crowns he received for regularly distributing a separately printed list). Among other worthy items omitted were the names of the new and the deceased members, the conditions of granting medals, and the statutes of the Society. Actually, furthermore, to print 800 copies was unnecessary since many members had no real interest in the contents. On the positive side Babbage noted the regular and accurate publication of the *Transactions*, a characteristic to be imitated by all societies, especially by the French Institute, which was woefully remiss.

Finally, in the matter of encouraging science, Babbage made a few lesser recommendations. He stressed the need for an order of merit based on quality; knighthoods he feared would go to "mere jobbers" in science. Elevation to the peerage might have value. The union of scientific societies was desirable in many ways, not least as an economy measure. One large building would house their meetings, which could be scheduled without conflicts, and one library, which would prove a great advantage.

In a chapter not quite so germane to his central theme, Babbage concerned himself with "observations," in which he emphasized the value of minute precision, described the art of observing, warned against frauds, and passed some shrewd judgments. The last advances toward precision, he pointed out, required greater labor, but the first steps toward discovery added most to knowledge. Some men held that no experiments had value except the measurements be most minute; thus they diverted attention from points of greater importance. Genius was marked not by observations perceptible only to the most acute, but rather by achievements obvious to the common observer. In observing, one might safely trust the principles of common sense. He should find the limits within which an instrument could "measure the same object under the same circumstances." An elaborate instrument was worthless if it could not be used dependably; and its findings should be checked constantly. This was essential because observers sometimes put out fraudulent conclusions: the deliberate hoax—deception without wit—a vicious offense in that the products of nature were so various that a hoax might be convincing; forgery; clipping off bits from some observations, which differed most in excess from the mean, and sticking them on others; and finally the selection of observations which most agreed and the use of different formulas as if they were the same.

In conclusion, Babbage eulogized Wollaston and Davy, stressing especially the modesty of the first and the impulsiveness of the second. He also printed three appendices. The first described a scientific meeting at Berlin in 1828, reprinted the eloquent address of Alexander von Humboldt, and indicated the character of the scientists present; the second showed how personal favor often determined election to the Royal Society; the third listed the Fellows who had contributed to the *Transactions* or had been on the council.

In his incisive essay on machinery and manufactures, "perhaps the first adequate exposition of the economic bearings of the machine industry," Babbage gave only one chapter to science; the remainder dealt with the history of English manufactures, taxes, and combinations of masters and workmen. He could not forbear, however, pressing his scientific creed, especially as related to the situation in England and the dependence of applied on pure science. The connection of the latter should be intimate, he reiterated, and the discovery of theoretical truths should not only be protected by patents but, because of the expense, subsidized. In countries where science was a profession it was properly rewarded. Not so in England! The only high place to which a scientist might aspire—the presidency of the Royal Society—was held by the Duke of Sussex (1773–1843), who took office despite his narrow majority of eight over John Herschel. This clearly illustrated the evil days on which English science had fallen.

Happily, the British Association promised after only two meetings to remedy the deficiencies of the Royal Society. The benefits to science of such an association, properly run, were varied and significant. Chief among them was the intercourse between pure and applied scientists. In consequence, the sons of wealthy manufacturers might prove a hopeful source of future scientists; the economy of some "pure" discoveries to "applied" production might be more fully appreciated, especially the potential value of chemistry and physics; and the future prospects of neglected regions of the world might be emphasized.

Early in the Association's career Babbage contributed to its meetings. He recommended a collection of tables of all those facts which could be expressed by numbers in the various sciences and arts, and which he denominated "the Constants of Nature and Art" (*Report of the First and Second Meetings,* 2nd ed., 1835, 613). In some remarks on the vital statistics of an Irish parish, he declared that "to discover those principles which will enable the greatest number of people by their combined exertions to exist in a state of physical comfort and of moral and intellectual happiness, is the legitimate object of statistical science;" the data would ameliorate "the moral and physical condition of the people." He also reported favorably on cooperative shops (*Report of the Fifth Meeting,* 1836, 118, 125).

The examination of the exposition of 1851, nearly twenty years later, prompted Babbage to narrate the origins of the Association. Although major attention in this book went to political economy, science received considerable space. Because learned societies had long proved useful in resolving doubts and clarifying discoveries, the increasing technical complexity of science was begetting more specialized societies. One such, organized by Professor Oken (1779–1851), of Munich, was an annual assemblage of German naturalists meeting in different cities. Gradually this society had admitted representatives from other sciences, as well as foreigners. The generous welcome and the imitators of it testified to its success. At first, for instance, Austria was an intellectual desert, her rulers even refusing passports to her philosophers: that now was remedied, and Vienna was soon to entertain a meeting. Moreover, when Englishmen appreciated the success of the whole project, they sought to form a similar institution. At first grave difficulties arose because of the unfavorable position of science in England. Since the government recognized no obligation to science, it was indifferent to scientific progress and incapable of distinguishing the charlatan from the savant. Once founded, however, the British Association conspicuously reflected Oken's ideal in many respects.

Unfortunately the Association, now twenty years old, had not altogether realized its opportunity. Few qualified people attended the sessions, but Babbage thought that if it would meet in different cities and then convene in London, and if new sections and exhibitions were encouraged, its prospects would brighten. Under the inspiration of Adolph Quetelet (1796–1874), who once came to address the Association and found no provision for his "valuable budget" of documents, a statistical section had quickly gained recognition, and soon after the Statistical Society had been born. Properly supported, the Association would have greatly improved the Exhibition of 1851. Such societies needed encouragement, for many were created by popular demand, but they did not flourish in England. France had gone much further. Her Conservatoire des Arts and Métiers (1795), comparable to Gresham College, was very successful; and many other countries had followed suit. In England, rewards often went to persons broad in interests rather than original in contributions, and to those with an aptitude for figures. The pecuniary returns were poor, and in eleven years, in the midst of a veritable shower of party creations, only one baronetcy and six knighthoods had rewarded scientists. Babbage felt that many government posts should go to scientists, and pure science should be steadily honored. "It is of the very nature of knowledge that the recondite and apparently useless acquisition of today, becomes part of the popular food of a succeeding generation."

How far and how unfairly Babbage clung to his earlier prejudices appears in his references to the Royal Society. He harshly condemned its intrigues, and, despite the steady improvements of the preceding years, he declared it to be so hopelessly mismanaged that all efforts at reform had failed. Thereupon he repeated his criticisms of twenty years before. In France, only men of the highest intellectual attainment were the electors of the Institute, but in the Royal Society any one of the 800 Fellows, the greater part of whom knew no science, equalled any other in voting power. In contrast to the Paris Academy the secretaryships of the Royal Society went to third-raters and its presidency to men of exalted rank, whose other interests took first place. The Society also awarded its medals with great irregularity; prejudice, deliberate malice, and disparagement almost invariably took precedence over merit. Since pure science was inadequately recognized, its university chairs often went to men appointed not as scientists but as divines. Although much of this attack was no doubt literally true, Babbage made no allowance for a changing spirit in the Royal Society and the universities.

His autobiography, *Passages from the Life of a Philosopher* (1864), related his troubles with the calculating machine, his success abroad, his failure at home (to secure several learned posts for which he thought himself qualified), and his abomination of street music. To the eradication of this last he devoted as much energy as to the reform of the Royal Society and the elevation of science. He calculated the damage to learning, health, and music itself; he listed the instruments of torture—organs, bands, fiddles, harps, hurdy-gurdies, whistles, trumpets, and, not least, the human voice whether in advertising, canting, or singing; he found taverns, children, servants, country visitors, and "ladies of elastic virtue" the chief encouragers. Because of his efforts, however, noises redoubled outside his window, mobs hooted at him, dead cats were heaved into his areaway, his windows were broken, and he himself was struck. Occasionally he reverted to his earlier complaints about science, as when he characterized presidents of scientific societies as the "demimonde" of science, and when he derided the "erudite Professor Ponderdunder," a member of all existing academies, author of the remarkable metaphysical treatise, "On the Entity of Space," and promoter of "small science and low art."

He concluded by listing the personal "peculiarities" which enabled him to accomplish "what even the most instructed in their own sciences deemed impossible." During his waking hours he invariably followed some train of inquiry, trivial or profound; to avoid insomnia he pursued a trivial subject until he went to sleep; to accomplish something important he worked up the minor accessories during the day and between 2:00 and 6:00 A.M. tackled the essential problem. Early perceiving the immense power of science, he became convinced "that the highest object a reasonable being could pursue was to endeavour to discover those laws of mind by which man's intellect passes from the known to the discovery of the unknown."

When Babbage died the Royal Society printed no obituary. Perhaps it no longer regarded him a member, although in 1864 he had called himself such and in 1859 he had contributed "Observations on the Discovery in various Localities of the Remains of Human Art mixed with the Bones of Extinct Races of Animals" (*Proceedings,* **X,** 59–72). On the other hand, the president of the Statistical Society hailed Babbage as more than any other the founder of the Society, for it began at his house. Babbage, he went on, "took broad views of science" and explored many branches of knowledge; in 1839 he had read an informative paper on the statistics of the clearinghouse, and his study of machinery and manufactures was "a masterly analysis." Interestingly enough, the president referred in no way to the *Reflections* or the *Exposition of 1851* (*Journal of the Statistical Society,* **XXXIV,** 411–17).

Babbage also figured variously in less specialized periodicals. Some statements in the *Reflections* (pp. 62–65) led to an acrimonious controversy in the *Philosophical Magazine* (1830). The president and secretary of the Royal Society denied the charges concerning alterations of the minutes. Babbage replied; the secretary answered; and Babbage rejoined with the last word. Captain Edward Sabine also defended himself against the criticism in the *Reflections* (pp. 76–100), and *Socius* accused Babbage of pettiness, especially since he had contributed to the *Transactions* and held office in the Society. At the same time *Socius* recognized the need for the reforms suggested by Babbage (ser. ii, **VII,** 446–48; **VIII,** 44–50, 72–74, 153–59). Two years later, on the appearance of the essay on machinery and manufactures, the *Philosophical Magazine* (ser. iii, **I,** 208–13) put him in a much better light. Although specifying that the book was outside the sphere of a scientific journal, the reviewer insisted upon the need for becoming acquainted with so remarkable a work. The scientific reflections were "equally fitted for the perusal of the philosopher and the general reader." Babbage had clothed "the stores of his highly endowed mind with the richest drapery of language"—in description, perspicuous, in argument, concise.

The reaction of lay periodicals to Babbage possesses greater interest, and whatever they discussed, the opinions varied. *Blackwoods* (**XXXII,** 1832, 694) was "sick of the very name of this coxcomb Babbage. Such a cold dry, prig it is! So intensely ignorant, too . . . Babbage rhymes to cabbage;" the man writes "as pompously as the Pandects." The *Quarterly* (**XLIII,** 1830, 305–42) alone reviewed the *Reflections,* strongly complimenting Babbage's "varied and profound acquirements." As was customary, the reviewer actually examined the subject as a whole, and in so doing was "unceremoniously exposed" by Granville for "his *diatribe* on the scientific men of his own country." The review especially underscored the contrast between England and every thriving country on the Continent, and insisted upon the need for more radical changes than those suggested by Babbage.

In sharp contrast *Fraser's* (**VIII**, 1833, 167–75) found the essay on machinery and manufactures deficient in every required condition of such a work. What was wanted was "not a treatise for a lady's boudoir . . . or a confused miscellany . . . but a treatise of solid and substantial merit," from a man keenly aware of existing needs. The deficiency of the work was the more obvious in that Babbage "stood second in British science at the time of its appearance," and had taken such a dim view of the prospects of science in Great Britain. Actually the large sale of this rubbish proved his contentions concerning the decline of science in England. Nevertheless, in considering the chapter on science, the review not only admitted the validity of Babbage's criticisms but actually, as had the *Quarterly* earlier, went farther. It did, however, belittle his opposition to Sussex, on the ground that no one paid any attention to the Royal Society anyway, that the Duke might rise above the trivial politics of the Society, and that the Society needed much more fundamental reform.

The *Edinburgh* fell between the extremes of applause and denunciation; on the whole it paid Babbage the high compliment of solid comment and lengthy attention. It discussed the history of the calculating machine (**LIX**, 1834, 263–327); and its disregard of Babbage's strictures on the Royal Society did not, as already noticed, arise from dissent. Not uncharacteristically, in reviewing his less controversial books, it adopted a down-the-nose attitude, but here as elsewhere "this won't do" symbolized an insistence on first-rate work. The reviewer of *A Comparative View of the Various Institutions for the Assurance of Lives* (1827) advised Babbage to confine his "comprehensive mind" to the "researches of abstract science." Here he had made "too bold a descent into the details of ordinary business," and he could not be congratulated on the success of his undertaking. The errors in his reasoning showed clearly that he was not at home with his materials (**XLV**, 1826–7, 482–513). In the essay on machinery and manufactures, a reviewer found "within a small compass a great deal of novel and interesting information, presented in a striking manner;" he also found it sometimes superficial, unsatisfactory, and incomplete (**LVI**, 1832–3, 313–32).

The conditions of science which aroused Babbage had already had a long history (Henry G. Lyons. *The Royal Society 1660–1940,* Cambridge, 1944, chap. vii; *The Record of the Royal Society,* 4th ed., London, 1940, 54–74). For decades the Royal Society had faced ridicule and secession. The increase in the number of Fellows during the eighteenth century, the relative diminution of the number of scientific members, and the Society's failure to promote science actively were widely deplored, as were the self-perpetuating council and the secrecy attending finances. Furthermore, the long presidency of Sir Joseph Banks (1778–1820), despite his positive beneficial achievements, had stereotyped the outlook; and at his death there was widespread feeling that science needed more emphasis, a conviction to which Banks himself had subscribed.

Although Banks was followed first by Wollaston (June to November 1820) and then by Davy (1820–27), and the new council contained 12 instead of 7 or 8 scientists among its 21 members, no revolution occurred. Davies Gilbert and the Duke of Sussex presided from 1827 to 1838. Davy himself soon emphasized social rather than scientific distinction; in addition he proved a tactless, arbitrary, and impulsive administrator. Moreover, his chagrin over the failure of a recommendation to the Admiralty restricted his activity after 1825. This permitted the dominance of Gilbert, a persistent opponent of reform throughout his entire connection. Several years elapsed before genuine scientists regularly controlled the council, for some new members after 1820 were scarcely more than "courtesy" scientists.

If the scientists did not take over the Society in the 1820s, they were sufficiently influential to open the door to such a development a generation later. Upon Davy's resignation in 1827 Gilbert's effort to elect Sir Robert Peel, then Home Secretary, aroused so much hostility that the matter dropped, but Gilbert himself took the chair and appointed a council—including the reactionary Lord Colchester (1759–1827) in case legal assistance was needed—which suppressed the reform report at its first meeting. In 1830 Gilbert further showed his hand by picking the Duke of Sussex as his successor. This, with his earlier activity and his appointment of the Bridgewater lecturers, aroused criticism, but not enough to unseat him or destroy his influence. Nevertheless, Society affairs received such an airing that better prospects were at once apparent. Sussex showed some sympathy with reform, although as time went on he left affairs more and more in the hands of reactionaries until 1838. Thereafter improvement occurred rapidly. Whereas in 1830 nobility comprised over 10 percent of the membership, in 1860 the proportion had fallen to 4.5 percent and was steadily diminishing.

Equally important was the growing emphasis upon scientific, productive Fellows. The breakdown of the membership in 1830 into its component parts reveals how much such emphasis was needed if the Society was to improve natural knowledge. The 10 episcopal members had contributed 9 papers to the *Transactions,* all by one of their number; 63 noble members had contributed nothing to the *Transactions*; 66 military and naval officers had contributed 35 papers, but 25 came from 2 military members; 74 lesser clergy had contributed 8 papers, and 63 legal members 28 papers, 16 by one man; 100 physicians and surgeons had contributed 203 papers, 109 by Sir Everard Home. The 286 other members, who included "pure" scientists and men about town, had contributed 205 papers. Out of 488 papers in the *Transactions*, 408 had come from "pure" and applied scientists, and 50 more had come from 4 other men.

How far this situation prompted a change remains obscure, but henceforth, although any candidate could be elected, six instead of three supporters of his certificate were required. Moreover, lists of the council members and the officers were made available before the anniversary meeting, and the treasurer's accounts and the council's report were published. In no sense revolutionary, these changes at least suggest some triumph for the critics. Other reforms followed; and in 1840 the election of W. R. Grove, though he was a nonscientist, added a powerful recruit to the science faction on the council. Scientists still formed scarcely more than a third of the Society, but after 1842 the number of new Fellows elected each year fell off significantly. Scientists completely dominated the council in 1845–46, and in 1847 came some drastic changes along lines sought twenty years before Babbage. By 1860, the Society had become scientific.

Interestingly enough, the sort of criticism heaped on the Royal Society found expression in other spheres. Thomas Wakley (1795–1862) saw favoritism and nepotism rampant within the medical profession and founded the *Lancet* in 1823 in protest against abuses in the College of Surgeons and elsewhere. In 1846, he as a Member of Parliament introduced a bill for reforming medical practice. It failed, but its essence went into a law in 1858. At the same time the universities, especially Cambridge, were waking from a long sleep and facing the new demands.

To cast up the accounts of Charles Babbage is not difficult. Notwithstanding the fundamental validity of his contentions, the solid comment of Granville and the testimony of history refuted his more extreme affirmations. He was, for his own purpose, too petulant and incidental. His disappointments led him into misrepresentation if not downright error. Science *was* a profession and so rewarded; the Duke of Sussex, whether he should have been elected or not, turned out better than some "scientific" presidents; the derelictions of the universities and the government had always existed; British science, never on a par with Continental in discovery and interpretation, had fallen no farther behind; the stress upon the number of contributions to the *Transactions* was invidious.

In tracing "the gradual decline of mathematical, and with it of the highest departments of physical science," Babbage too closely identified the Royal Society with science. Although for one hundred fifty years they had seemed one, they were, after all, not the same. The newer societies belied his pessimism, for by his own admission several exhibited the liveliest interest in science and appeared the likeliest agents for its advancement. Moreover, the contemporary intellectual depression and administrative oligarchy of the Royal Society only marked a stage through which every learned society goes some time or another. In the century since his fulminations, many societies have irritated their more scholarly members. The piddling raffle of politics, the perennial committeeman, favoritism and snobbery, and the awarding, on the basis of age or distinctions other than intellectual merit, of honors to which the scholar alone may aspire flourish as they did in Babbage's day.

More generally, Babbage failed to appreciate that each country's ideals are shaped by its history and rooted in its environment; in their origin and growth they are as social as institutions. Science in England was made and supported by Englishmen in their own way. Admittedly they stressed application, wherein they surpassed the Continent as far as the Continent outstripped them in discovery and philosophical interpretation. That stress had values beyond utility. No one will deny the benefits of qualitative selectivity; at the same time no one can disregard the consequence of divorcing the laboratory from the larger social scene. The English scientist knew fewer formulas; perhaps he knew more humanity. By keeping his feet on the ground he did not reach the stars, but neither did he pursue hypotheses irresponsibly. Any scientist appreciates his moral responsibility most directly when he considers his researches in terms of their social application. This he must do when he submits his discoveries to the judgment of an informed laity as well as to his fellow-technicians. The investigator who has not scrutinized his hypotheses and tenets through a glass brightly, who has not related one scientific assumption to others, who has not checked them against each other and against established truths outside science, may become a thriving gadgeteer, but he will certainly make a sorry citizen and a sorry scientist as well.

Whether Charles Babbage comprehended matters of this sort is not clear, but, for all his arrogance, he was a humanitarian, and no mere doctrinaire. What he sought so ardently was not less application but more original discovery. No one familiar with the history of any branch of knowledge can doubt the essential soundness of his position; nor, so far as we are concerned here, can one doubt that he successfully needled his learned contemporaries into general agreement with him.

ITINERARY

SECOND (1948) ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

Cranbrook Institute Bloomfield Hills, Michigan	October 1–30
Buhl Planetarium and Institute of Popular Science Pittsburgh, Pennsylvania	November 6–30
Franklin institute Philadelphia, Pennsylvania	December 6–31
Science Illustrated New York City	January 6–20
General Foods Corporation Hoboken, New Jersey	February 3–24
Brown University Providence, Rhode Island	March 7–26
Gamma Sigma Epsilon University of Florida Gainesville	April 15–May 1
University of California Berkeley	May 15–30
American Osteopathic Convention St. Louis, Missouri	July 11–17
Pennsylvania State Museum Harrisburg, Pennsylvania	September 5–26
Royal Photographic Society London, England	December 1–31

PINEAPPLES IN ANCIENT AMERICA*

J. L. COLLINS

Dr. Collins (Ph.D., University of California, 1923) has been geneticist and head of the Department of Genetics at the Pineapple Research Institute of Hawaii since 1930. In 1938–39 he conducted an expedition into South America to look for wild and semi-wild species of pineapples, covering areas in Brazil, Argentina, Paraguay, and Bolivia.

IN THE early morning of Sunday, November 3, 1493, the fleet of vessels under the command of Christopher Columbus on his second voyage to the New World, made a landfall in the lesser Antilles of the West Indies. A landing was made on the first island they approached, to which Columbus gave the name "Marie-Galante." They soon returned to their ship and sailed for a larger, mountainous island visible in the distance, on which they landed Monday, November 4, 1493. Columbus gave to this island the name "Guadeloupe." According to the chronicle of Peter Martyr, here, at an Indian village, they found pineapple plants and fruits, "the flavor and fragrance of which astonished and delighted them." This is the first recorded contact of European people with the pineapple.

Although the pineapple represented a new and exotic fruit to these Europeans who sailed with Columbus, it appears to have been a common and an important element in the diet of the inhabitants of tropical America in pre-Columbian times. How widely the fruit was distributed and used by the Indians is to some degree indicated by the records left by those adventurous souls who penetrated many tropical regions during that romantic period of exploration and adventure following the discovery of the New World.

The pineapple shares the distinction accorded to all the major food plants of the civilized world of having been selected, developed, and domesticated by peoples of prehistoric times and passed on to us through one or more earlier civilizations. The pineapple, like a number of other contemporary agricultural crops such as corn, potatoes, tobacco, beans, and peanuts, originated in the Americas and was unknown to the people of the Old World before the discovery of America.

The Indians of tropical America had developed and named a number of different varieties of pineapples, selected by them or their ancestors because of their size of fruits, good quality, and absence of seeds. Wild pineapples that are probably the foundation stocks from which these domestic varieties were derived are still to be found in tropical America, but they are generally small-fruited, inferior in eating quality, and extremely seedy. None of these can be singled out now as the form or forms which gave rise to the domestic pineapples of today or even of those varieties in the possession of the Indians at the time of the discovery of America.

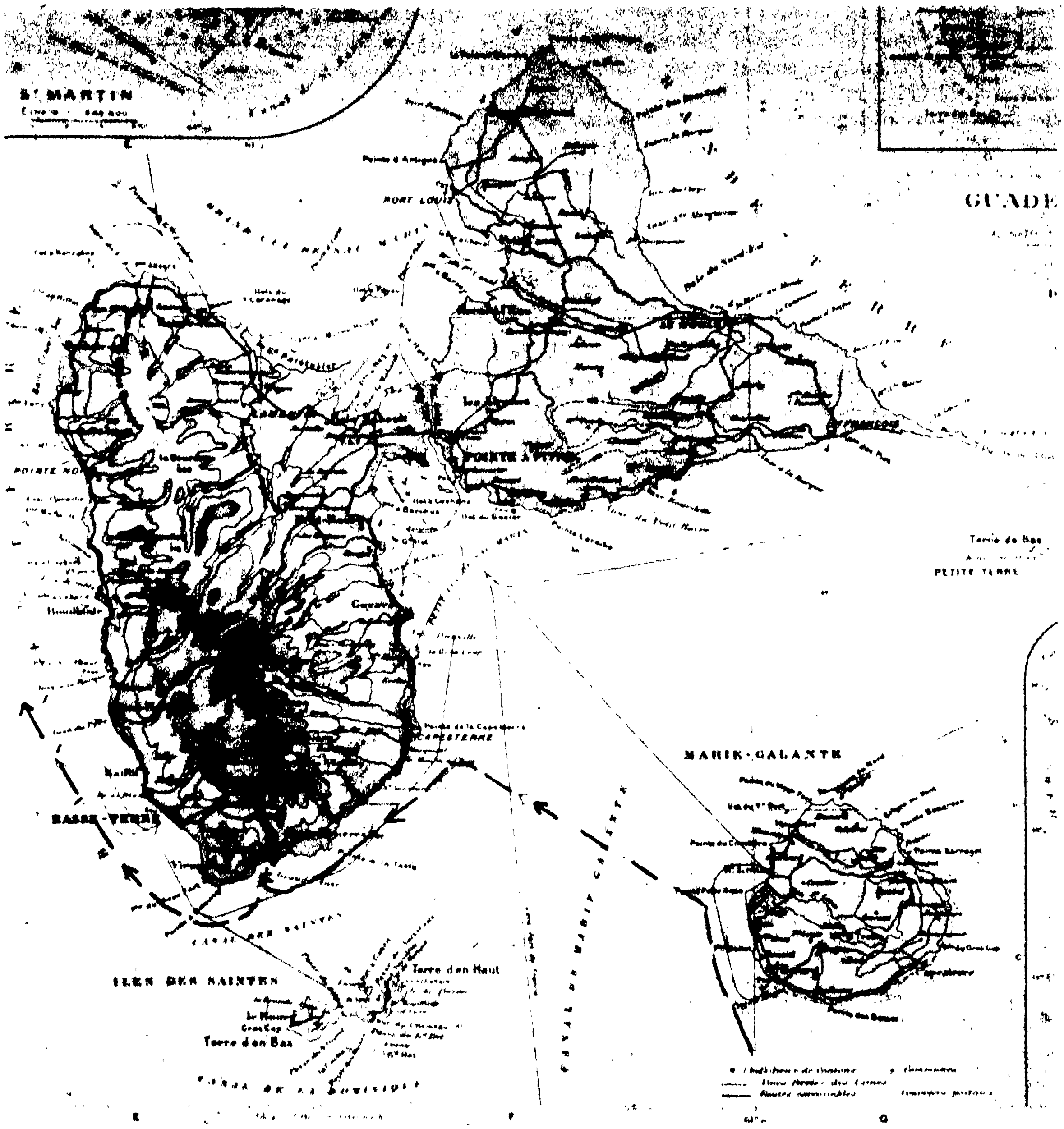
The pineapple appears to have been used very little by the native American tribes in their religious ceremonies, for which reason it has not been found as a motif for pottery designs, temple ornaments, and other decorations, as were corn and potatoes. There is some evidence, however, that it may have figured in some of the religious rites of the Indians of Mexico, for Acosta in his *History of the Indies* gives a detailed description of an Indian god which he described as holding in his left hand a white target with five pineapples made out of white feathers and set in the form of a cross. Since this god was used by the Indians previous to the introduction of Christianity, the image was probably made long before the coming of Europeans. This would then indicate that the Indians of Mexico were acquainted with pineapples in pre-Columbian times.

Thompson believed that the Maya Indians of Central America were not acquainted with the pineapple before the discovery of America by Europeans. He discusses this possibility as follows:

> The pineapple (*Ananas sativus*) possibly did not reach the Mayas until shortly after the conquest. The Prospero Indians who were entirely cut off from Spanish influence were found to be cultivating the pineapple when first visited by Europeans in 1646 (Cogolludo, Book XII, Chap. 7). This, of course, is not direct evidence that the pineapple was known to these tribes before the arrival of the Spaniards in the new world, as many of the articles introduced by them passed from tribe to tribe and were well established in remote areas long before Europeans had penetrated so far. Pineapples were certainly cultivated in the Cueva region prior to the conquest (Oviedo, Book XXIX, Chap. 29) and on the Mosquito Coast (M. W.) and the Usamacintla basin (Tozzer, 1912). . . . In view of the close relations between the Chiriqui area and the Mayas as demonstrated by the importation into Chichen Itza of gold objects from this region, it is not unlikely that food products of the two areas were interchanged.

---

* Published with the approval of the director as Miscellaneous Paper No. 46 of the Pineapple Research Institute, University of Hawaii.



The island of Guadeloupe in the West Indies, where pineapples were first seen by people from the Old World when Columbus landed here on his second voyage to the New World in 1493.

The Relacion Breve relates that Father Ponce was received during his journey to Yucatan in 1588 at many of the Maya villages with gifts of pineapples.

Thompson also points out that there is no word in the present Maya language for the pineapple and that the Spanish *piña* is used. This, he believes, does not necessarily mean that the pineapple was unknown to the Maya before the Spanish Conquest, since the Maya Indians have accepted Spanish words for other things, the earlier Maya terms being known only to a few of the older Indians at the present time. In a letter to the author in 1946, Dr. Thompson stated that he would now consider that the Maya of the peninsula of Yucatan did not have the pineapple but that the Maya of other Central American regions did have it in pre-Columbian times.

Stephens found what he believed to represent a pineapple made of stucco mounted on a circular base, used as an ornament in front of a small shrine or altar among the ancient ruins on the seacoast of Yucatan at Tuloom. His illustration, when viewed under low-power magnification, shows a scalelike surface which could represent the eyes (or individual fruitlets) of a pineapple fruit. Stephens presents arguments in favor of this town being inhabited by the Indians at the time of the discovery of America, concluding that the ruins examined in 1840–41 were not of great antiquity.

The following is quoted from Stephens' account of his visit to these ruins:

> Near the foot of the steps overgrown by the scrubby wild palm, which covers the whole cliff, is a small altar with ornaments in stucco one of which seems intended to represent a pineapple. These wanted entirely the massive character of the buildings and are so slight that they could almost be pushed over with the foot. They stand in the open air exposed to strong easterly winds and almost to the spray of the sea. It was impossible to believe that the altar had been abandoned 300 years.

The 300 years would take the date back to about 1540, approximately the time of the Spanish conquest of Yucatan. Stephens believed that the Indians had continued to use these buildings long after the conquest.

A. M. Tozzer, who has made comprehensive studies of the culture of the pre-Columbian inhabitants of Central America, stated in a personal communication that he was sure this ornament mentioned by Stephens could not have represented a pineapple.

Cook states that the pineapple was known in Peru before the arrival of the Spaniards and gives the Inca name (*Achupalla*) for it; on this he bases his belief that the Inca Indians were acquainted with this fruit. This is the only indication we have so far found that the Indians of the west side of South America knew the pineapple before 1492.

De Oviedo, who lived in the New World from 1513 to 1547, voiced his belief that the pineapple was an old and well-known fruit when he observed that "In all these islands [West Indies] it is a fruit which I hold old and very common, because they are found in all these Islands and on Tierra Firme" [mainland of Central and South America]. He described three distinct varieties grown by the Indians of Haiti and produced the first illustration of a pineapple. This is a pen drawing used to supplement his description of a pineapple, which he admitted lacked exactness.

Columbus found pineapples being cultivated in fields by the Indians in 1502 at a place he called Puerto Bello on the Atlantic coast of what is now the country of Panama. His son Ferdinand reported finding pineapples being cultivated at Veraqua in this same general region in the year 1503:

> They make another wine of the fruit we said is found in the Island of Guadeloupe, which is like a great pineapple: it is planted in great fields and the plant is a sprout growing out at the top of the fruit itself, like that which grows out of a cabbage or lettuce. One plant lasts three or four years and bears.

Again, during his last voyage in 1503, Columbus found pineapples growing at Belen, which is a short distance north of the mouth of the Panama Canal. According to Irving's account, Columbus in 1503 landed on the island of Guanaja a short distance off the coast of Honduras, where he traded with a large canoeload of Indians who were quite different in appearance and clothing and who had apparently just arrived from Yucatan. They had come intent on trading with the Indians of the island and brought cotton cloth, copper utensils, pottery vessels, cocoa, beer made from maize, and wooden swords edged with sharp pieces of flint of a type found in Mexico at a later date.

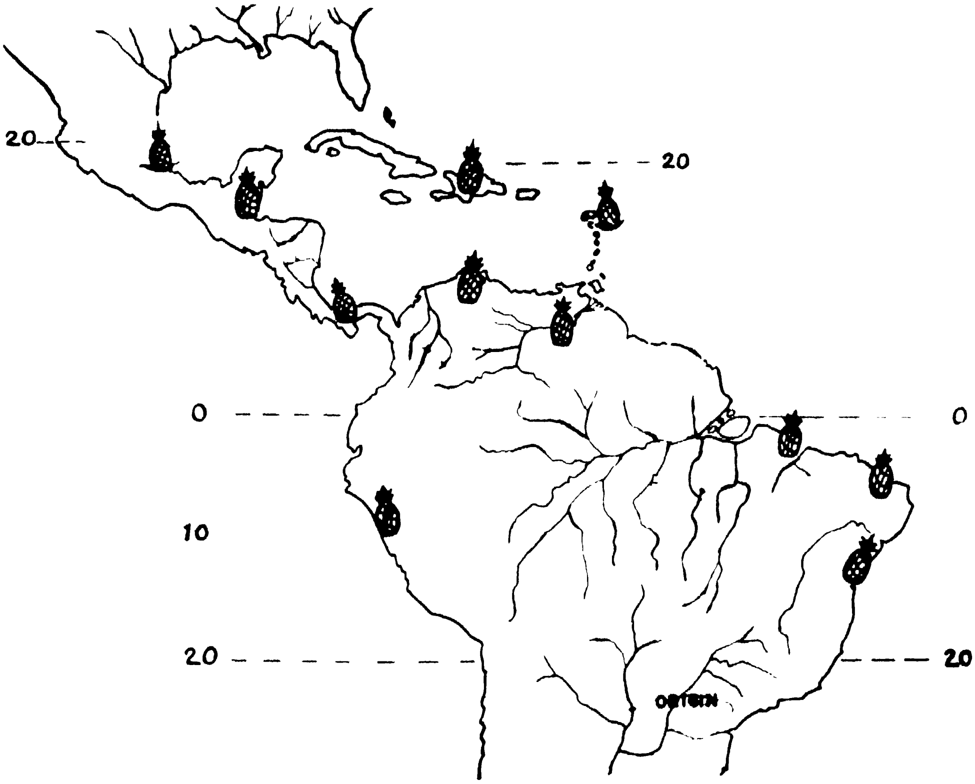
In 1503, while Columbus was exploring the coastal area of Panama, "He again heard of a nation in the interior, advanced in arts and arms, wearing clothing and being armed like the Spaniards." These incidents indicate trading between the interior and the coastal area where pineapples were found by Columbus.

In 1519, only twenty-six years after Columbus first saw the pineapples on the island of Guadeloupe, Pigafetta recorded pineapples growing in coastal areas of Brazil, which is probably the first record of pineapples on the mainland of South America.

Gerommo Benzono, who lived in Mexico from 1541 to 1555, records pineapples being grown in that country, and about this same time, Jean de Lery again records pineapples growing in Brazil. Whether this represents a new area for Brazil or whether it was the same as reported by Pigafetta some twenty-two years earlier, we do not know.

In 1565 Sir John Hawkins, sailing in his ship *Jesus of Lubeck*, stopped at a place along the coast of Venezuela known then as Sante Fe, in order to take on supplies of water and provisions. Here he received from the Carib Indians, who were described as naked savages armed with poisoned arrows, "hennes potatoes and pines." These latter were pineapples "of the bigness of two fistes." The inside of these pines was said to "eateth like an apple but is more delicious than any sweet apple suggared."

In 1595 Sir Walter Raleigh made his remarkable expedition some 400 miles up the Orinoco River in northern South America. He reports having secured quantities of pineapples from the Indians in trade at various points while ascending the river. This observation was made one hundred and two years after the discovery of America, and one might well argue that the pineapples could have been introduced into this area in the intervening years and that it does not necessarily represent a pineapple area of pre-Columbian times. Neither do we know about the amount of communication between the Indians of the coastal area of Panama, where pineapples were known to exist in pre-Columbian times, and the tribes in the interior of the country. However, the fact that Sir Walter Raleigh found them in some abundance and that the Indians also made a kind of wine from them argues for a long-time presence in a country inhabited by primitive people having limited means or inclination for travel and transportation.

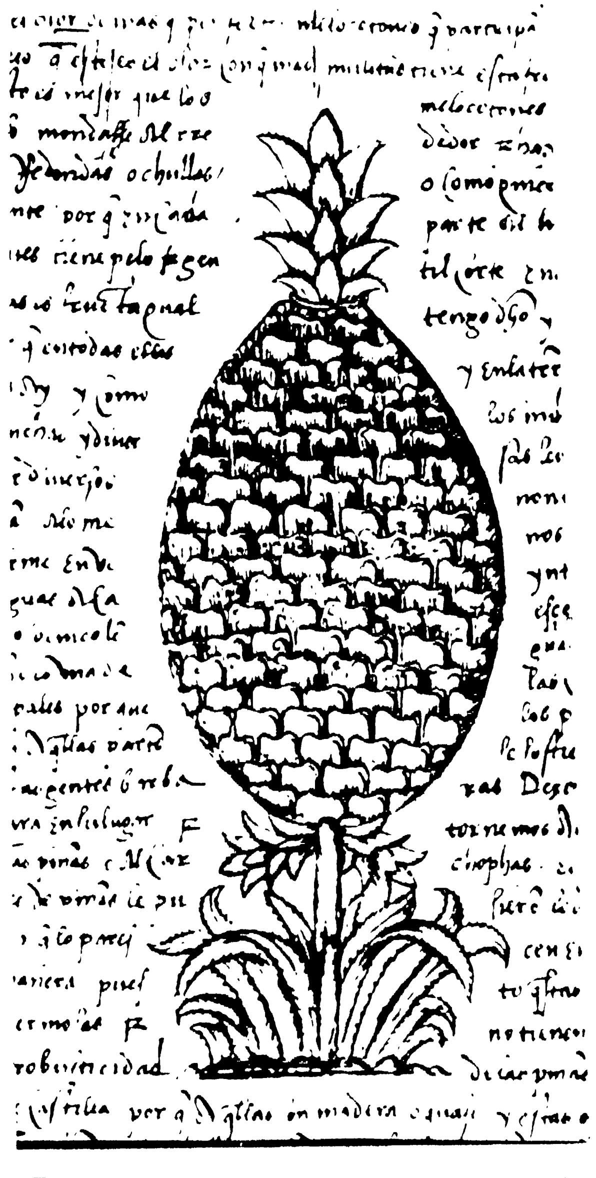


This map, showing the distribution of pineapples in tropical America at the time of the discovery, is based upon records left by the early explorers.

In 1637 Frans Post, a Dutch painter, came to Brazil as a government official while the northern part of Brazil was under the domination of the Dutch government. During his stay in Brazil, he painted a large number of landscape, seaport, and village scenes portraying the country and customs typical of Brazil at that time. Among these paintings, twelve (and possibly more) show isolated pineapple plants as a part of the native vegetation. From these paintings we may conclude that pineapples were a common feature of the north Brazilian coastal area in the early part of the seventeenth century.

Laufer, in discussing the migration of the pineapple, believed that the absence of the seeds in the cultivated species was induced by long-continued asexual reproduction and thus served to indicate a great age for the cultivated species. This belief that long-continued asexual reproduction induces or causes the loss of seed production, at one time quite current in regard to seedless fruits in general, has been shown to be erroneous. Seedlessness usually first appears in plants as a result of mutation in the chromosomes (a hereditary change) or as a consequence of hybridization, which is thereafter perpetuated by the asexual method of propagation. It is not difficult to call to mind examples in horticulture of plants long propagated asexually that still produce seeds. In the cultivated pineapple in Hawaii, new mutations (reverse mutations) have been found and perpetuated by asexual propagation which permit the development of seeds in the formerly seedless variety. Thus, the commercial seedless variety has suddenly reverted while under cultivation to a seedy condition. The seedlessness of the pineapple cannot be considered as an indication of its age as a cultivated plant.

The early explorers of America were primarily interested in the search for gold, finding a sea route to the Indies, or carrying the gospel to the native peoples, whom they looked upon as heathens, and probably had little interest in transporting plants from one part of the new country to another. Therefore, the records they made of pineapples being present in different places may be accepted as evidence of their establishment there before the country was invaded by these semicivilized Europeans.



Earliest known pineapple illustration—produced by Oviedo about 1520. It appeared in his *History of the Indies*, published in Seville in 1535. (Reproduced from the handwritten manuscript No. HM 117 by permission of the Huntington Library, San Marino, California.)

The medicinal and other qualities attributed to the pineapple by the early travelers and colonists indicate a long-time association with the fruit to provide for the development of these beliefs—much longer indeed than the short period of sketchy European contact with the pineapple. These beliefs regarding the medicinal qualities of the pineapple must have been developed by the Indians through their long association with the fruit and passed on to the newly arrived Europeans, who then made these qualities a matter of record.

The area or place of origin of the pineapple, whence it was disseminated to other tropical American regions, is still a matter of some uncertainty, but opinion of botanists generally favors a region in South America near the middle part of the Parana River and the drainage of the Iguassu River, a region including southeastern Brazil, Paraguay, and Northern Argentina. This area also seemed to be indicated by the studies of the distribution of wild species of pineapple by Baker and Collins in 1938 and 1939.

In this region lived the Tupe-Guarani Indians in pre-Columbian times (some still do so), a hardy, intelligent, warlike people who migrated northward and westward until some branches of the tribes crossed the Amazon and reached the seacoast of northern South America. The Carib Indians who occupied northeastern Brazil continued the northward expansion and had extended into the islands of the Caribbean Sea before 1492. These tribes are believed to have carried the pineapple along with them and introduced it to other tribes in these new areas, and they in turn passed it on to their neighboring tribes. Thus, by a dual process of tribal migration and border trading between tribes, the pineapple was spread throughout tropical America.

We are somewhat puzzled as to the interpretation to be given three references indicating the possible presence of the pineapple in the Old World many centuries before the time of Columbus. These statements cannot be ignored in the present discussion of pre-Columbian pineapples and are accordingly included, together with the general opinion regarding them.

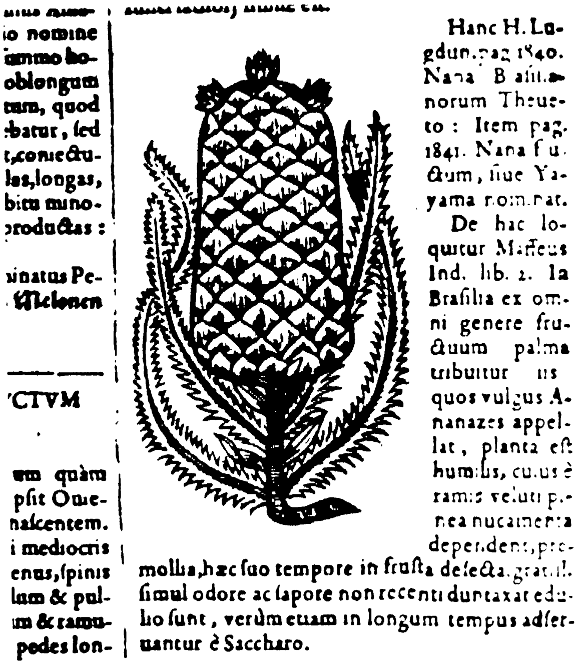
Layard and Rawlinson both describe some stone carvings on the walls of the ancient Assyrian city of Nineveh in which are shown different articles of food served at a banquet, including one that both writers listed as representing a pineapple. Rawlinson stated that "The representation is so exact that I can scarcely doubt the pineapple being intended." Layard expressed some doubt about the Assyrians being acquainted with the pineapple but argued that "the leaves sprouting from the top proved that it was not the cone of a pine tree or fir."

The third reference to the presence of the pineapple in the ancient civilizations of the Old World is that of Wilkinson in *Manners & Customs of the Ancient Egyptians*. He states that "Among the numerous productions of India met with in Egypt which tend to prove an intercourse with that country may be mentioned the pineapple, models of which are found in the tombs of glazed pottery. One is in the possession of Sir Richard Westmacott." Wilkinson seems to believe that the pineapple was a product of India, but there is no evidence that it was grown in India or Asia during that period. Marco Polo, who visited India and Asia centuries later and who described with considerable accuracy many of the products of those countries, makes no mention of the pineapple.

The pineapple is so constituted for survival by its vegetative method of reproduction and the ability of these vegetative shoots to remain alive for long periods of drought or neglect that it is very improbable it could have disappeared completely from these countries had it existed there. Other food-supplying plants known to those regions have survived the passing of ancient civilizations. The fact that the pineapple was not handed down through successive generations and civilizations, along with such plants as wheat, dates, pomegranates, melons, etc., seems to be sufficient evidence that it had never existed there.

The pineapple belongs to the family of plants known as Bromeliaceae, which includes numerous species of terrestrial and epiphytic plants, all except one being native to America. This one species is indigenous to the west Coast of Africa.

Ames, in discussing the relationship of economic annual plants and the development of human cultures, stated that the completed species indigenous to America exhibited all the characteristics indicative of great age that are shown by the economic plants forming the basis of ancient civilizations in the Old World. He concluded that "The sum of biological evidence leads to the conclusion that the cultivation of economic annuals of the New World must reach back as far into human history as the origins of agriculture in the Old World." The pineapple shares these characteristics of great antiquity along with the economic annuals of the same country.



io nomine
ummo ho-
oblongum
tum, quod
batur, ſed
t, coniectu-
las, longas,
bitu mino-
productas:

uinatus Pe-
Melonen

'CTVM

um quàm
pſit Orie-
naſcentem.
i mediocris
enus, ſpinis
lum & pul-
m & ramu-
podes lon-

Hanc H. Lugdun. pag. 1840. Nana Braſilianorum Theueto: Item pag. 1841. Nana fructum, ſiue Yayama nominat. De hac loquitur Maffeus Ind. lib. 2. In Braſilia ex omni genere fructuum palma tribuitur iis quos vulgus Ananazes appellat, planta eſt humilis, cuius è ramis veluti pinea nuca inerta dependent, præmollia, hæc ſuo tempore in fruſta deſecta, grati[illegible] ſimul odore ac ſapore non recenti duntaxat edulio ſunt, verùm etiam in longum tempus adſeruantur è Saccharo.

Drawing of the pineapple called *Yayama* by Oviedo. One of the three varieties described by him in his history of the Indies. (From Bauhin, J. *Historiae Plantarum Universalis*, 1651.)

A long period of prehistorical development for the cultivated pineapple is indicated by its wide distribution, the presence of several distinct cultivated varieties, its use as food, wine, and medicine at the time when America was discovered, and the absence of a recognizable wild progenitor.

SCIENCE ON THE MARCH

THE SCIENCE OF SANITATION

TRANSLATION of progress in science into forms that influence our personal living and economic life is an everyday occurrence. This transition, unobtrusive though it may seem, is often the result of an orderly progression through a network of technical organizations, trade associations, governmental agencies, and private industry, all working together to convert the findings of science into useful articles and methods.

The science of sanitation, which has reached a high level in the United States, has been furthered for over three decades by the National Association of Insecticide and Disinfectant Manufacturers, Incorporated.* This trade association is an organization of leading manufacturers of household and industrial insecticides, disinfectants, cleaning compounds, sanitary chemicals, and related products and equipment that are the tools of sanitation. Like other trade groups, it serves as a common meeting ground and spokesman for the industry. Binding it together are its established objectives—the promotion of technical and ethical standards for the industry, the education of the public concerning its products, and the sponsoring of scientific research.

The National Association of Insecticide and Disinfectant Manufacturers (we can call it NAIDM for short) was founded in 1914 by a small group of manufacturers who met to discuss problems of mutual interest—problems characteristic of a new and rapidly expanding industry—standardization and regulation, product development, and consumer education.

Thirty years ago, when NAIDM was entering the picture, quite a number of sanitary products were available to the public. By present-day standards many of them would be regarded as obsolete, now being considered either unduly harsh in action or else relatively ineffective. But scientific control methods were not so common, and chemistry had not yet produced the variety of organic chemicals we now employ as weapons against insects, rodents, and infectious germs. Today's modern chemical products for sanitation are on a sound standard of quality—the result of cooperative research financed by NAIDM or conducted by its committees and members.

One of the first problems of the growing industry was the need for classification of the various types of sanitary products into logical groups. This work, hampered by conflicting terms and definitions, was aided when the NAIDM sponsored a fellowship at Antioch College to define such terms as "antiseptic," "germicidal," "disinfect," etc. The results of this study appeared in the *American Journal of Public Health* in 1932.

Standardization of the types of products, with the ultimate aim of establishing a system of grades to govern buying and selling, has always been an important function of the Association. Some phases of standardization can be made on a chemical assay, whereas other phases require a biological evaluation. In the case of common fly sprays, for example, the ingredients can be purchased on chemical or physical tests, but in the finished product the ingredients may function synergistically so that performance can be rated only by trials on living insects. One of the best-known activities was the refinement and official adoption in 1932 of the Peet-Grady method for rating household insecticides, a procedure that has been widely used throughout the world for many years. To further utilize the Peet-Grady method, NAIDM made available to the entire industry the Official Test Insecticide, the "yardstick" on which is based U. S. Commercial Standard CS 72–38 for insecticides.

Similar Association activity in the field of disinfectants has led to many improvements in the widely used phenol coefficient method of rating these products. U. S. Commercial Standards for hypochlorite, pine oil, and phenolic disinfectants promoted by NAIDM have been in use for many years. Recent cooperative research undertaken by NAIDM and the U. S. Department of Agriculture on methods of testing the important group of quaternary ammonium disinfectants was published in 1947. Another group within the Association is investigating and promoting standards for special soaps, floor treatments, self-polishing waxes, and other sanitary specialties.

Fellowships at Ohio State University and at Wilmington College have been established to tackle such problems as the evaluation of roach sprays and powders, DDT residues, and aerosol bombs, under Association sponsorship. In its own members' laboratories, NAIDM committees have contributed greatly to the knowledge of mothproofing treatments and their evaluation; to the improvement of livestock sprays; and even toward the establishment of safe standards of slipperiness for floor wax. Rosters of these committees include the names of many of the country's leading scientists in chemistry and the biological sciences.

---

* Executive offices of the Association are located at 110 East 42nd St., New York 17.

Because the Association encompasses a wide variety of products and because finished products represent a blending of numerous skills, its activities are concerned with many of the sciences. As one would expect, entomology, chemistry, and bacteriology play the leading roles, although the medical and physical sciences, as well as engineering skills, are often called upon. To cite the example of the fly spray again, it is necessary to use containers of a certain design because contact with lead solder or copper can destroy the insecticidal action of pyrethrins. Sprayers and similar devices for applying insecticides and disinfectants are important aids to sanitation, and their development is followed closely.

To accomplish exchange of technical and scientific information, NAIDM maintains close liaison with the U. S. Department of Agriculture, the U. S. Public Health Service, the National Bureau of Standards, and with other technical groups such as the American Society for Testing Materials, the American Association of Economic Entomologists, and the American Chemical Society.

The sanitary products industry, like many other economic groups, has gradually become a highly regulated business. Particularly is this true in the case of insecticides, fungicides, germicides, rodenticides, and weed killers. Within the past few years, most of the states have adopted new laws governing these products, and in 1947 the long-outmoded Federal Insecticide Act of 1910 was replaced by a much stronger and broader law. The Federal Food, Drug and Cosmetic Act and the various fair-trade laws covering advertising also enter the picture.

To keep pace with this increasing governmental activity, the Legislative Committee of the NAIDM has served an important function since the depression days of the NRA. Efforts of this group have not been to oppose regulation but rather to shape the laws to give adequate protection to the public without imposing stifling and costly barriers on the industry. That these efforts have been rewarded is evidenced by the fact that all Federal and state insecticide laws are now closely patterned after a model law sponsored by NAIDM in cooperation with other trade organizations and professional groups. To reduce the burden of keeping the scientists and executives of the industry posted on the many laws and regulations governing the manufacture and sale of its products, the Association made available in November 1947 a comprehensive *Compilation of Laws* with an annual revision service. This valuable work can be obtained by both members and nonmembers from the Association office.

As a trade association, its major function of establishing ethical business relations and promoting a spirit of cooperation has indeed been accomplished. Growth of the NAIDM, which in turn is a measure of outstanding service to a growing industry, is evidenced by the increase in membership shown below:

1915	15 firms (first year of organization)
1925	25 firms
1935	108 firms
1945	207 firms
1947	269 firms

Meetings are held twice a year, usually for a three-day period in December and in June. A résumé of each meeting, as well as many of the scientific papers and addresses, is presented in the trade magazine *Soap and Sanitary Chemicals,* which is the Association's official publication. Current items of a scientific nature, as well as general information, go forward to the members promptly in frequent bulletins.

At thirty-four years of age, the National Association of Insecticide and Disinfectant Manufacturers has yet to reach its prime. Its members look forward confidently to many more years of increasing service to industry, science, and the public in general.

W. A. SIMANTON

Berkeley, California

BOOK REVIEWS

Русний язык для уценых

Chemical Russian Self-Taught. James W. Perry. vii + 221 pp. $3.00. *J. of Chemical Education.* Easton, Pa.

AT THE present time, when the study of the Russian language in colleges and universities is being placed on an equal footing with that of French and German, especially for students majoring in science, the meagerness of good textbooks and good teachers is an acute problem. Any book that can alleviate this condition is to be greeted with enthusiasm. *Chemical Russian Self-Taught,* by Perry, goes a long way toward fulfilling the need of the student interested in studying scientific Russian.

The book contains seven chapters: Aptitude for Learning Languages; Suggestions for Study Methods; The Vocabulary Problem; Inorganic Chemical Nomenclature; Organic Chemical Nomenclature; Russian Grammar; and Glossary of Russian Technical Terms.

In his foreword the author encourages the chemist who is interested in learning Russian in order to be able to read and understand Russian chemical articles, and discusses in his first two chapters aptitude for learning languages in general and method for studying Russian in particular. Without question many of his suggestions will be of aid to the student. This book, as the author points out, is designed for the chemist who has already become acquainted with Russian grammar and pronunciation, but an introduction to the alphabet and phonetic pronunciation is nonetheless given. Considerable space is devoted to the transition of nouns to adjectives and verbs and the uses of prefixes and suffixes. In his introduction to grammar the author underscores its difficulty and makes helpful suggestions for reading and understanding chemical works. The conjugation of verbs and the use of prefixes that can change the meaning and aspect of words are taken up in considerable detail. The glossary consists of Russian technical terms, together with their root words and the English equivalents.

This glossary will be of great help to the student, and, as a matter of fact, it should be helpful not only to English-speaking chemists but also to Russian chemists desiring to know the English equivalents for Russian technical terminology. At any rate, I have found it of help to me.

There are very few inaccuracies: *superoxide* is translated into Russian as being *suboxide*; and Walden's *rotation* is given in Russian as Walden's *conversion.* These cannot be avoided in a first edition, however.

In general, the author is to be commended for having undertaken such a difficult work and for having produced such a good text. It can be recommended without question to English chemists desiring to learn scientific Russian.

V. IPATIEFF

Department of Chemistry
Northwestern University

Scientific Russian Reader. Noah D. Gershevsky. xxii + 253 pp. $3.50. Pitman Publishing Co. New York and London.

THIS book is a compilation of more than one hundred brief excerpts from contemporary Russian writings in the fields of physics and chemistry. Within these fields, the excerpts are concerned with a wide range of subject matter. The book should prove helpful to moderately advanced students of Russian who wish to practice reading scientific material for the purpose of obtaining an extensive vocabulary of scientific terminology.

Each of the excerpts is provided with grammatical notes explaining the more difficult constructions and also with a list of selected words. A comprehensive vocabulary, and tables of the names of chemical elements and also of conversion factors for weights and measures, are included. The book does not contain a systematic grammar summary. The accent is not marked in the reading excerpts nor in the vocabulary.

J. W. PERRY

Massachusetts Institute of Technology

FOR THE JUNIOR BOTANIST

Beginner's Guide to Wild Flowers. Ethel Hinckley Hausman. vii + 376 pp. $3.50. Putnam. New York.

IN THIS compact, pocket-size field book for the identification of Eastern wild flowers either by the juvenile or adult beginner, there are 1,080 excellent line drawings (1½ × 2 inches) of flowers, three to a page, with common and scientific names, a brief nontechnical description, time of flowering, and family name. The scientific names are based on Gray's *New Manual of Botany,* seventh edition. The common names are said to be "those most familiar through long folk usage according to Webster's New International Dictionary."

The flower figures are arranged in five color groups, namely: white or whitish, yellow or orange, pink or red, blue or violet, and green or brown. When a species has more than one flower color, the figure is repeated under the additional color sections with the result that forty-one species are repeated twice and two species three times, reducing the actual number of species illustrated to 1,045.

Beginners east of the Mississippi River will find in the book a large proportion of the wild flowers they will encounter except rare species, species of very local distribution, and the many species of such large groups as the goldenrods, asters, and hawthorns.

It seems inevitable, however, that in such a popular book some very common plants would be overlooked, or missing because of a publication limit to the number of pages and illustrations, a larger proportion of the Southeastern species being missing than of the Northeastern. Among the more conspicuous omissions are: *Asarum virginicum, Clematis Viorna, Helleborus viridis, Hybanthus concolor, Hymenocallis occidentalis, Iris prismatica, Iris verna, Iris virginica, Lespedeza repens, Lespedeza frutescens, Lonicera Japonica, Oenothera laciniata, Papaver Rhoeas, Saxifraga micranthidifolia, Senecio Smallii, Spiraea sylvester,* and *Yucca filamentosa.* The book does illustrate, however, more species than any similar popular wild flower guide and will be found quite satisfactory for most beginners until they are sufficiently versed in the subject to need one of the more complete technical manuals.

P. L. Ricker

Chevy Chase, D. C.

THE SUBTILE FINE ART

Mathematics: Our Great Heritage. William L. Schaaf, Ed. xi + 291 pp. $3.50. Harper. New York.

THIS book contains a collection of sixteen essays by different authors, all of which have been previously published. These essays deal with the nature and cultural significance of mathematics. They have been selected and edited by Dr. William L. Schaaf, who has contributed a preface to the book and interesting introductory remarks to each of the essays.

In his preface the editor states that in selecting the essays he has had in mind the thoughtful reader who desires to understand why mathematics means so much to mankind. When the reader is a teacher or student of mathematics this selection is not a difficult task; but to provide interesting and readable essays on mathematics for the lay reader is a different matter; and in this respect Dr. Schaaf has been (in my opinion) remarkably successful.

The essays have been divided into five groups, each dealing with a different aspect of mathematics: its role as a creative art; its origin and development; its internal structure and intrinsic nature; its relation to science and technology; and its cultural and humanistic bearings. Of this collection I may mention three essays that particularly interested me: "On the Seriousness of Mathematics," by G. H. Hardy; "On the Relations of Mathematics and Physics," by Robert B. Lindsay; and "Industrial Mathematics," by Thornton C. Fry.

I noticed only three errors in the book, one of which is probably typographical; the others may mislead a nonmathematical reader. In the essay "Mathematics—the Subtile Fine Art," the statement is made that "no fractional numbers exist in Nature's building material." The reference is undoubtedly to atomic weights and to Prout's hypothesis (1816) that the atomic weights of the chemical elements would probably be found, by more precise measurement than was then possible, to be exact multiples of that of hydrogen. All through the nineteenth century this hypothesis was favorably regarded and gave rise to many redeterminations of atomic weights without finding any integral ratios; and in the twentieth century physical methods of making these measurements have been developed that are capable of much greater precision than chemical methods. It now appears that these ratios are all slightly different from whole numbers. For instance, assuming the atomic weight of oxygen to be 16, that of hydrogen is 1.00812. Although there may be no ratios expressible as common fractions, there are plenty of decimal fractions in nature's building material.

Again, in the essay "The Larger Human Worth of Mathematics," it is said that "it is an eternal truth that every integer is the sum of squares of four integers." But what about the number 3? This cannot be divided into four integral parts unless we suppose one of these parts equal to zero. It is not easy to understand what the author means by this statement.

On the whole, however, there is much of value in this collection of essays, and the book can be recommended to those who are interested in mathematics.

Paul R. Heyl

Washington, D. C.

LITTLE-KNOWN TRIBES

Economics of the Mount Hagen Tribes, New Guinea. Abraham L. Gitlow. Monographs of the American Ethnological Society, No. XII. xii + 110 pp. $2.75. J. J. Augustin. New York.

AS recently as 1933, the Mount Hagen area in the interior of New Guinea—next to Greenland the largest island in the world—was an unexplored and unknown land. With the coming of war in the Pacific, the north coast of New Guinea saw much bitter fighting. Nor did the interior remain outside the widely encompassing scope of World War II. In order to support air operations in the New Guinea theatre, a number of emergency landing strips were constructed in the central part of the island. Through the vicissitudes of war, Dr. Gitlow was dropped into the Mount Hagen region and was fortunately allowed a brief but productive period with the native peoples of the area. In this report he has assembled relevant published and unpublished material, together with his own information, and has prepared a concise and well-written account of the little-known Mount Hagen tribes.

The people of whom Gitlow writes live in a long upland valley whose western terminus is Mount Hagen. The valley is approximately 5,000 feet above sea level, and is bordered by high mountain ranges. Although New Guinea lies in the tropics, the altitude of this rolling, grass-covered valley makes possible its temperate climate. The native inhabitants are agriculturalists, depending on the cultivation of the sweet potato and a number of subsidiary plant crops for their food, as well as on the raising of pigs. The entire valley is estimated to contain 125,000 persons, of whom about 25,000 live in the Mount Hagen area proper. This populous region is one the last frontiers to be affected by contact with the white man, a principal reason why its tribes hold a special attraction for the anthropologist.

In describing the economic aspects of Mount Hagen life, Gitlow uses a procedure that has proved to be fruitful in ethnology. Through experience, anthropologists engaged in ethnological work have found that a single aspect of a culture can be adequately understood only if its relations to the other most closely allied aspects of the culture are determined. Thus Gitlow comments as follows on his approach to presenting a valid picture of Mount Hagen economics: "It was necessary to take into account the facts of the physical environment, the institutional forces of the Mount Hagen society, the force of custom, habit, and tradition, and the highly complex and variegated range of motives to which the natives are subject."

For this reason, the author has included useful material on geography, social organization, and religion, as well as on economics alone. As an example of the close relation between "economic" and "non-economic" pursuits, we may note one of Gitlow's findings. Pig-raising is a significant element of economic life, for the pig is important both as a source of food and as a medium of exchange. Pigs are raised, however, primarily for a religious reason, for the sacrifice of the animal is necessary to establish relations of amity with the spirits, on whose good will depends the successful pursuit of Mount Hagen life. Fortunately, the spirits demand only the soul of the pig, which leaves the flesh to be consumed by humans. The incentives that motivate the Mount Hagen peoples in the economic aspects of their daily life are not, therefore, simple functions of economic self-interest, but are complex and various, and serve to show once again that "economic man" is not the uncomplicated fiction of earlier writers on economics.

The present book is not to be considered, however, —nor could it have been intended—as a complete account of the economics of the Mount Hagen people. The report is not based on an extended period of field work, and is consequently limited in its scope and in its penetration. Within the modest framework of his study, Gitlow assembled such relevant material as he was able to obtain. In so doing, he has produced an account that will be of real service to future field investigators; that will remain a useful survey of the peoples of the area; and that is today the most up-to-date report on the Mount Hagen tribes.

ALEXANDER SPOEHR
Chicago Natural History Museum

◆

THE

SCIENTIFIC MONTHLY

DECEMBER 1948

METEORITES, RELATIVE ABUNDANCES, AND PLANET STRUCTURES[*]

HARRISON BROWN

Dr. Brown (Ph.D., Johns Hopkins, 1941) is associate professor in the Institute for Nuclear Studies at the University of Chicago. During 1943–46 he was assistant director of chemistry for the Clinton Laboratories, Oak Ridge, Tennessee. Dr. Brown was awarded the Annual Thousand Dollar AAAS Prize at the Chicago Meeting in 1947.

PRESENT evidence indicates that approximately three billion years ago the elements were formed, roughly in the proportions in which they exist today. Whatever the process might have been that terminated in the formation of elements, it was followed (probably rapidly) by a process of agglomeration that led to the formation of the bodies of matter which now make up our visible universe: the stars and the planets.

For many years scientists have puzzled over the nature of the processes resulting in the formation of elements, stars, and planets, but most questions concerning the origins of our universe are still either unanswered or at best incompletely answered. The scientist asks on the one hand: What were the conditions that existed in the universe when the elements were formed? What was the process of formation? What are the relationships between the relative amounts in which the elements were formed and the stability of the constituent nuclear species? When did the formation process take place? On the other hand he asks: What processes were involved in the agglomeration of matter, once formed, into stars? When and how were the planets of our solar system formed? What are the internal structures of the planets?

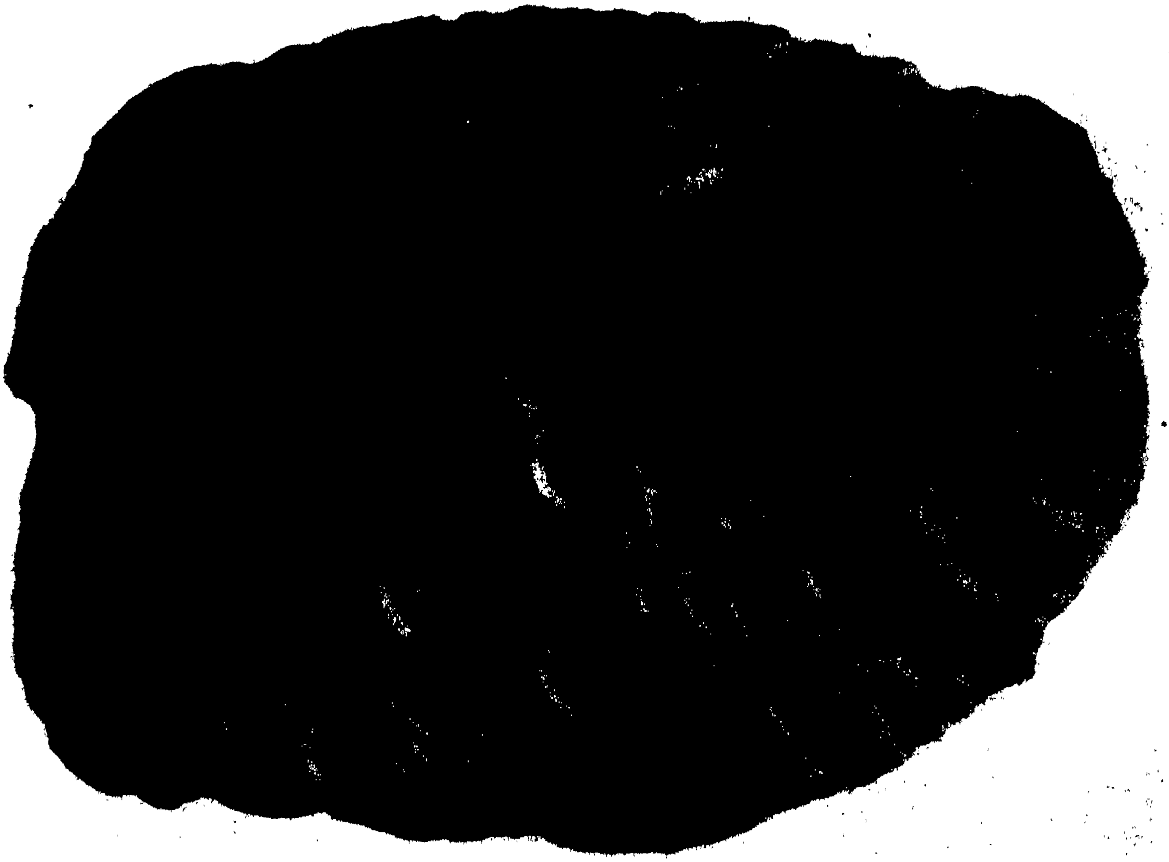
Essentially all branches of the physical sciences have been utilized in attempts to answer these profound questions. Astronomy, the various branches of physics (including nuclear physics), geology, and chemistry—each of these sciences has brought specialized knowledge to bear upon the problems. Seldom is the scientist confronted with major problems involving such large numbers of the physical sciences.

More recently, in large measure due to the classic work of such men as Farrington, Merrill, Prior, Tammann, Goldschmidt, Jeffreys, and Daly, the science of meteoritics has been used as a tool in solving many of the problems concerned with the origins of things. Work done during the past few years now strongly indicates that in meteoritics scientists possess a Rosetta stone that may well prove to be a major key in answering some of the questions propounded above.

Meteorites and their origin. Meteorites are solid bodies which arrive on the earth's surface from outer space. They range in size from a few milligrams to many tons. If one is seen to fall and is subsequently picked up, the meteorite is called a *fall*. If the meteorite is not seen to fall, but is instead found in the countryside, it is called a *find*. There are many interesting and important features of meteorites which space does not permit discussing here; the reader is referred to a recent popular review by Watson.[1] In the present discussion

---

[*] Photographs by courtesy of the Smithsonian Institution, Washington, D. C.



Tulia, Swisher County, Texas, Meteorite. About natural size, photograph shows "flight markings" on the forward face of the meteorite. Composition of this chondrite type is: olivine, 34.71%; enstatite, 25.64%; feldspar, 12.92%; iron and nickel, 4.14%; and troilite, 7.39%.

we shall be interested primarily in features of meteorites that are related to their composition.

Meteorites in general are of two types: *iron* meteorites and *stony* meteorites. Iron meteorites are fragments of pure metal, consisting primarily of an alloy of iron and nickel. Stony meteorites consist primarily of magnesium, calcium, and iron silicates through which finely divided particles of metallic iron-nickel are dispersed. The stones may thus be thought of as consisting of two phases: the *silicate phase* and the *metal phase*. In a pulverized meteorite, the two phases can be separated magnetically. A third phase, known as *troilite* and consisting in the main of ferrous sulfide, exists both in stony and in iron meteorites. A few meteorites are known, termed *pallasites*, which consist of approximately equal proportions of silicate and metal phases.

The average compositions of stony and iron meteorites[2] are given in Table 1, together with the compositions of the silicate phase (with the metal and sulfide phases removed), the metal phase, and the composition of igneous rocks.[3] The latter make up approximately 95 percent of the earth's crust. The averages for stony meteorites are based largely upon falls.

Inspection of Table 1 suffices to show that stony meteorites differ markedly in composition from igneous rocks, the latter being considerably higher in oxygen and silicon contents and lower in magnesium, chemically combined iron, and metal-phase contents. On the other hand, the compositions of iron meteorites and of the metal phase of stony meteorites bear a striking resemblance to each other. Meteorites clearly resemble no known bodies of matter found in large quantity on the surface of the earth. Yet there is a certain similarity in the compositions of igneous rocks and the silicate phase of stony meteorites. The similarity gives rise to the question as to whether there might not be a family relationship between meteoritic and terrestrial matter.

As early as 1850 A. Boissé suggested that the earth's composition is probably equivalent to the mean composition of meteoritic matter. Since that time much effort has been expended by scientists in attempts either to develop or to disprove Boissé's speculation. Investigations during the past fifty years have yielded sufficient evidence to give basis to two beliefs that have been generally held: (1) that the dense core of the earth, as defined by seismic data, is probably similar to iron-nickel meteorites in composition; (2) that the silicate phase of stony meteorites is similar to the mantle surrounding the core of the earth. The logical conclusion to be drawn from these beliefs, if the beliefs can be transformed into fact, is that meteorites are the fragments of a planet, similar to the earth in general chemical characteristics.

Recently thermodynamics has been applied to the problem of the composition of meteoritic matter[2] with considerable success, giving further weight to the hypothesis that meteorites had their origin in a planet. Perhaps the most convincing evidence in this connection is that derived when one applies thermodynamics to the distribution of elements between the meteoritic phases.

It has been recognized for some time that those major meteoritic constituents whose oxides possess low heats of formation exist primarily in the metal phase. This phenomenon has been put upon a quantitative basis,[2] and it has been demonstrated that the observed distribution of elements between the metal, silicate, and sulfide phases of meteorites can be explained satisfactorily if one assumes that the distribution was obtained at a temperature of approximately 2,000° K and a total pressure of about 1.5×10^5 atmospheres. Such temperatures and pressures are of the order of those to be expected in the interior of a planet about the size of Mars. Using thermodynamics, it can also be demonstrated that the conditions under which the elements distributed themselves between meteoritic phases varied from meteorite to meteorite in such a way that the greater the metal-phase content of the meteorite, the higher the temperature or the pressure must have been in order to establish the observed equilibrium.

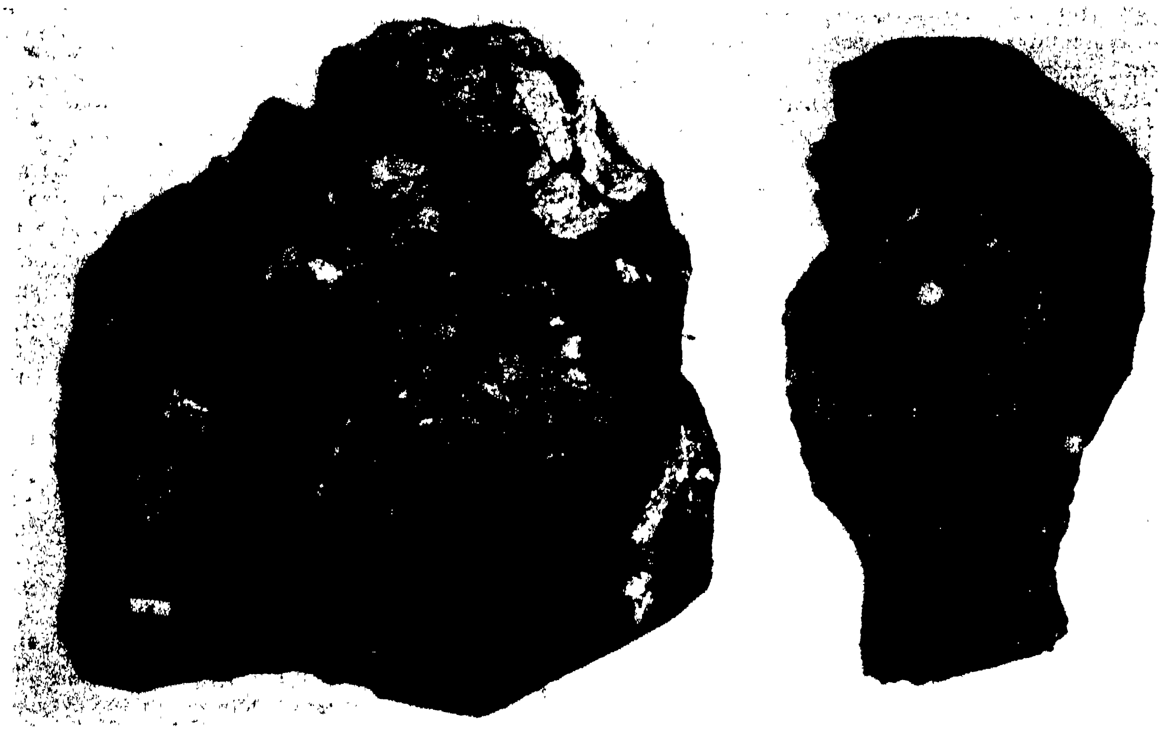
The conclusion to be drawn from thermodynamic evidence, then, is that meteorites were part of a planet, roughly the size of Mars. The planet was disrupted in some unknown manner, and since that time fragments of the planet have been striking the earth. In general, the greater the metal-phase content of a meteorite, the further from within the depths of the disrupted planet did it arise. The planet possessed a core of nickel-iron and a mantle of silicate phase containing dispersed metal.

On the basis of such findings, one can obtain some idea of the structure of the unknown planet by the simple process of averaging the compositions of meteorites over intervals of metal-phase content, keeping in mind that the greater the metal content, the closer one comes to the core. Table 2 shows the average metal-phase content and, for comparison, the compositions of igneous rocks and

TABLE 1

THE AVERAGE COMPOSITION OF STONY AND IRON METEORITES
(Percentage by Weight)

Element	Silicate Phase of Stones	Metal Phase of Stones	Iron Meteorites	Total Stony Meteorites	Igneous Rocks
Oxygen	43.12 ± 1.00	...	...	36.15 ± 0.89	46.49
Silicon	21.61 ± 0.19	...	...	18.12 ± 0.22	27.62
Combined iron	13.23 ± 0.42	...	...	14.21 ± 0.39	5.10
Combined nickel	0.39 ± 0.07	...	...	0.33 ± 0.06	0.01
Comb. cobalt	0.02 ± 0.01	...	...	0.02 ± 0.01	
Magnesium	16.62 ± 0.32	...	...	13.93 ± 0.29	2.11
Sulfur	...	...	...	1.79 ± 0.08	0.05
Calcium	2.07 ± 0.21	...	...	1.74 ± 0.18	3.63
Aluminum	1.83 ± 0.15	...	...	1.53 ± 0.13	8.12
Sodium	0.82 ± 0.06	...	...	0.69 ± 0.05	2.85
Chromium	0.36 ± 0.06	...	...	0.30 ± 0.05	0.04
Manganese	0.31 ± 0.07	...	...	0.26 ± 0.06	0.09
Potassium	0.21 ± 0.02	...	...	0.18 ± 0.02	2.60
Phosphorus	0.17 ± 0.01	...	...	0.14 ± 0.01	0.13
Titanium	0.10 ± 0.03	...	...	0.08 ± 0.03	0.63
Hydrogen	0.07 ± 0.02	...	...	0.06 ± 0.02	...
Metallic iron	...	88.58 ± 0.55	90.78 ± 0.26	9.97 ± 0.69	...
Metallic nickel	...	10.68 ± 0.51	8.59 ± 0.24	1.20 ± 0.10	...
Metallic cobalt	...	0.70 ± 0.06	0.63 ± 0.02	0.08 ± 0.01	...



A fall from Walters, Oklahoma. This stony chondrite meteorite weighs about 62 pounds. In the cross section, dark veins represent material from the outside which has penetrated the meteorite. White spots are chondrules, peculiar rounded granules.

plateau basalt (corresponding to very deep-seated rocks). It can be seen that the transition is a fairly smooth one, with silicon and oxygen diminishing steadily with increasing metal content, combined iron passing through a maximum, then decreasing steadily, magnesium increasing, then leveling off, and calcium passing through a maximum and then leveling off. These effects can be interpreted on the basis of pressure effects within the planet.

TABLE 2

Average Composition of Stony Meteorites as a Function of Metal-Phase Content (Percentage by Weight)

	Metal-Phase Content	Combined Iron	Oxygen	Silicon	Magnesium	Calcium
Igneous Rock	. . .	5.10	46.49	27.62	2.11	3.63
Plateau Basalt	. .	8.09	45.41	23.64	2.95	5.78
	0.19	14.04	41.53	21.92	10.93	6.08
	1.06	14.79	41.39	21.87	12.22	3.29
	1.87	17.63	38.70	19.10	16.04	1.26
	2.78	20.67	37.78	18.00	11.86	2.16
	6.33	16.43	38.03	18.37	14.86	1.16
	9.97	14.16	36.44	18.36	14.38	1.23
	15.49	11.77	33.47	16.85	13.74	1.39
	22.48	8.90	30.71	15.90	13.71	1.37

The conclusion that must be drawn from thermodynamic and chemical studies on meteorites is that a parent planet once existed which was quite hot, probably similar to the earth in structure and in physicochemical characteristics. With its composition fixed by the fundamental abundances of the elements involved and the chemical processes connected with the initial formation of the planet, pressure effects produced segregations of matter within the body. The ratio of combined iron to metallic iron for the planet as a whole was fixed by the reducing conditions in existence during the beginning stages of the formation of the planet. Due to differences in ionic radii and density, however, localized shifts in equilibrium took place in such a way that the greater the distance from the core, the greater the state of chemical oxidation. As iron was the only major constituent of the magma that could readily give up oxygen, it played a major part in determining the states of the minor constituents of the planet.

Meteorites and the inner planets. Chemical evidence points strongly to a planetary origin of meteorites. Astronomical evidence is less clear-cut, but recent observations by Bauer[4] add weight to the possibility from the astronomical point of view. The gap between Mars and Jupiter has been widely discussed as the possible location of the parent planet.

Clearly, the gross composition of meteoritic matter resembles what one might expect for the earth on the basis of the earth's known physical and chemical characteristics: gross density, ellipticity, surface composition, and seismic discontinuities. Could it be that the parent planet and the earth more than resembled each other, and actually possessed nearly identical gross compositions? A study of the densities of the three planets Venus, Earth, and Mars and the densities of meteoritic matter makes this possibility appear highly likely.

In Table 3 are given the relative masses and densities of the three planets in order of increasing mass (Mercury is omitted as its density is not known with sufficient accuracy). It will be noted that the density increases markedly with the mass. This phenomenon has commonly been attributed to major differences in compositions.

Let us assume, on the other hand, that all three planets, together with the meteoritic parent planet, possessed nearly identical compositions and structures; namely, a metallic iron-nickel core, of the composition of iron meteorites, and a silicate mantle containing dispersed metal phase, of the composition of stony meteorites. Under such circumstances, but for compression effects, all four planets would have identical densities. Compression effects, however, can play a major role, as evidenced by calculations by Jensen[5] on the density of iron at extremely high pressures. Utilizing Jensen's values, together with the known location of the boundary of the earth's core, and Bullen's[6] estimate of pressure versus distance within the core, one can calculate the density of the earth "uncompressed" and the ratio of metal phase to silicate phase in the earth as a whole. Using this ratio, one can then calculate the normal densities of Mars and Venus. On this basis, all three planets have an "uncompressed" density that is the same within the limits of the validity of such a calculation. In other words, *there is nothing inconsistent in the picture of all of the inner planets possessing nearly identical compositions, differing from one another in density almost entirely due to compression effects.* If this is true, then intensive study of the distribution of elements in meteorites should enable one to map out with fair confidence the internal structures of the inner planets.

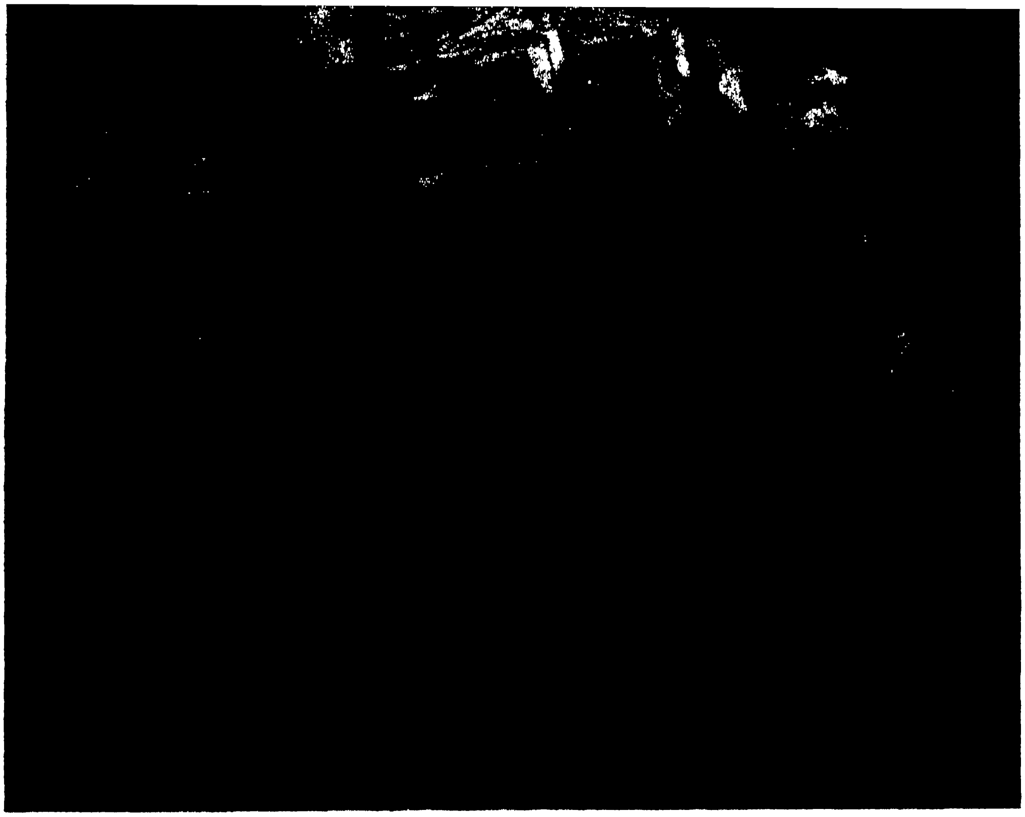
TABLE 3

PLANETARY MASSES AND DENSITIES

Planet	Mass	Density
Mars	0.11	3.96
Venus	0.81	4.86
Earth	1.00	5.52

Meteorite from Tennasilus, Esthonia. Rims around light areas (chondrules) are meteoritic iron, which rarely invades a chondrule.

Elements in meteorites and in the sun. It is possible to measure the relative abundances of certain elements in the sun by spectroscopic means. It is of interest to compare the composition of gross meteoritic matter with the composition of the sun, assuming a metal phase to silicate phase ratio for meteorites the same as that calculated for the earth. Comparison shows that, whereas the sun is primarily made up of light substances which condense only with difficulty, meteorites are composed primarily of heavier materials that are easily condensable, or that readily form easily condensable compounds (such as SiO_2). If one compares the abundances of the elements common to both meteorites and the sun, an interesting feature develops. Table 4 shows the abundance of certain elements in meteorites as measured chemically and computed in the method given above, together with the



An etched section of the Drum Mountains (Utah) meteorite, which at the date of its discovery (1944) was the eighth largest ever found in United States. This structural pattern is classified as a medium octahedrite, also known as Widmanstatten figures. The circular area in the picture is a cross section of one of the deep depressions that occurred on the surface of this meteorite.

abundances of the same elements in the sun as determined recently by Unsöld.[7] When one considers that the accuracy of the solar data is such that errors of the order of a factor of two might be involved in some cases, the agreement is quite good.

On the basis of this evidence, it is most tempting to postulate that the inner planets (including the meteorite parent planet) and the sun had a common origin from the same basic material, the inner planets retaining only the heavier, more easily condensable elements and compounds, and the sun retaining the lighter materials in addition.

TABLE 4

COMPARISON OF ELEMENTS IN METEORITES AND IN THE SUN

ELEMENT	RELATIVE NUMBERS OF ATOMS	
	Meteorites	Sun
Na	462	750
Mg	8,870	12,000
Al	882	850
Si	10,000	7,800
K	69.3	64
Ca	670*	670*
Sc	0.18	0.87
Ti	26	36
V	3	4.4
Cr	93	150
Mn	81	120
Fe	26,200	21,000

* The solar values have been adjusted so that meteoritic and solar calcium are equal. Silicon in meteorites = 10,000.

In order to explain the much smaller densities of the outer planets (Jupiter, Saturn, Uranus, and Neptune), it is necessary to assume that owing to their size and the conditions of formation they retained substantial fractions of the lighter and more volatile substances during the process of planet formation. Space, however, does not permit a detailed discussion of the composition of the major planets in this article.

The relative abundances of elements. Measurement of the abundances of elements in the sun and in the stars by spectroscopic means is quite difficult, and the number of elements that can be measured even approximately is limited. Spectroscopic evidence indicates, however, that the composition of the visible universe might well be fairly uniform throughout, with the exception of lighter elements whose abundances are changing because of thermonuclear transformations. If this is true, meteorites can be used to good advantage in measuring the relative abundances of the less volatile elements. Fortunately, the vast majority of the elements fall into this category. The abundance values as determined using meteoritic data can then be coupled with the spectroscopic solar and stellar values for the cases where meteorites cannot be used with validity. On this basis, by suitably combining meteoritic and spectroscopic data, a table of "Cosmic Relative Abundances"

TABLE 5

COSMIC RELATIVE ABUNDANCES

(Atoms per 10,000 Atoms of Si)

ELEMENT	ABUNDANCE
H	1.2×10^8
He	2.8×10^7
C	75,000
N	160,000
O	210,000
F	10
Ne	$10^3 - 10^4$
Na	462
Mg	8,870
Al	882
Si	10,000
P	170
S	3,300
Cl	250
A	190
K	69.3
Ca	670
Sc	0.18
Ti	26
V	3
Cr	93
Mn	81
Fe	26,200
Co	157
Ni	2,130
Cu	6.9
Zn	2.6
Ga	0.54
Ge	0.39
As	0.73

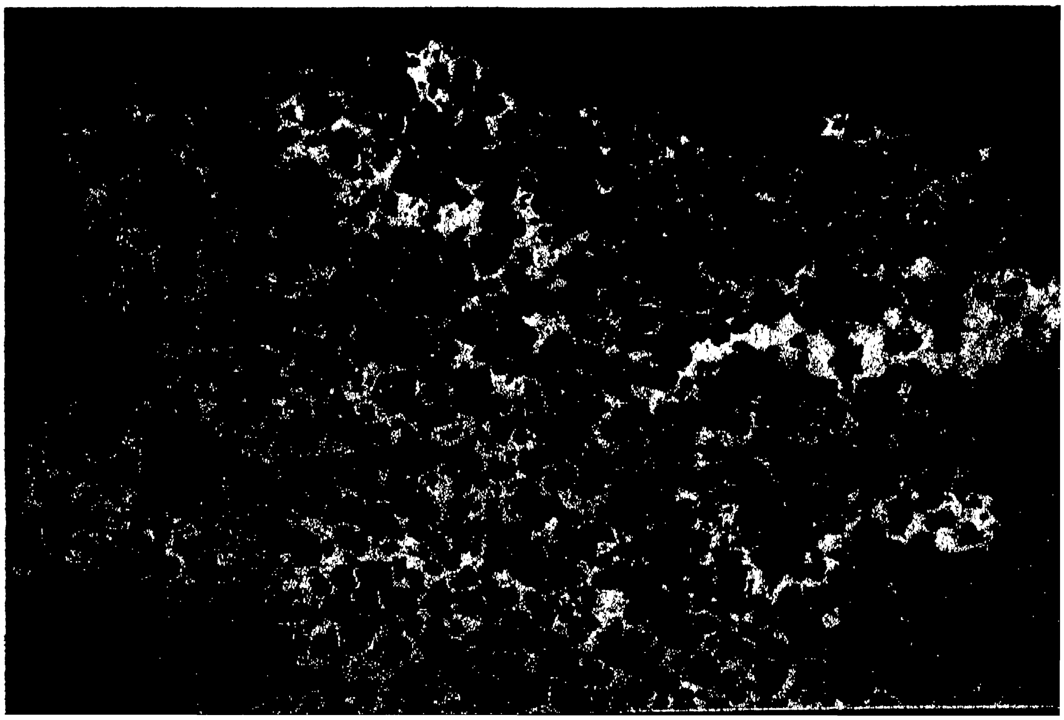
can be compiled. Table 5 shows the first section of such a compilation.

In view of the fact that the vast majority of elements are present in meteorites to an extent of but a few parts per million, the analytical problems involved are large. But fundamentally there are no limitations to such an approach, and the time should not be too far distant when a precise table of elemental abundances is available to aid in the solution of many astrophysical problems. The surface of the field of meteoritics as applied to astrophysical problems has barely been scratched. It is clearly a field that will yield rewarding information in the future.

REFERENCES

1. WATSON, F. G. *Between the Planets.* Philadelphia: Blakiston Co., 1941.
2. BROWN, H., and PATTERSON, C. *J. of Geol.*, 1948, **56**, 85.
3. CLARKE, F. W., and WASHINGTON, H. S. *Nat. Acad. Sci. Proc.*, 1922, **8**, 1.
4. BAUER, C. Paper presented at Chicago Meeting, AAAS, December 26, 1947.
5. JENSEN, H. *Zeits. f. Tech. Phys.*, 1938, **19**, 563.
6. BULLEN, K. E. Monthly Notices Royal Astronomical Society, *Geophys. Suppl. 3*, 1936, **3**, 395. *Royal Soc. New Zealand Trans.*, 1937, **67**, 122.
7. UNSÖLD, A. *Zeitsch. fur Astrophysik*, 1948, **24**, 323.

Mount Vernon meteorite. Knowledge of the existence of this olivine and iron, or "pallasite," meteorite (*below*) dates from 1868, but it was not recognized as a meteorite until 1902. Weighing originally about 351 pounds, it was found in one mass near Hopkinsville, Kentucky. Cross section shows (*white areas*) iron and nickel alloy.



PROBABILITY, RARITY, INTEREST, AND SURPRISE

WARREN WEAVER

Dr. Weaver (Ph.D., University of Wisconsin, 1921) taught at his alma mater from 1920 to 1932, when he resigned as chairman of the Department of Mathematics to become director of the Division of Natural Sciences of the Rockefeller Foundation. During World War II he served on the National Defense Research Committee, the Naval Research Advisory Committee, and as a member of the War Department Research Advisory Panel.

THE unusual is often interesting. Moreover, the frequency definition of probability, which is the one consciously or tacitly used in all applications to experience, makes it clear that an improbable event means precisely a rare event. Hence, an improbable event is often interesting. But is an improbable event always interesting? We shall see that it is not. If an event actually occurs, and if its probability, as reckoned before its occurrence, is very small, is the fact of its occurrence surprising? The answer is that it may be, or it may not be.

Suppose one shuffles a pack of cards and deals off a single bridge hand of thirteen cards. The probability, as reckoned before the event, that this hand contain any thirteen specified cards is 1 divided by 635,013,559,600. Thus the probability of any one specified set of thirteen cards is, anyone would agree, very small.

When one hand of thirteen cards is dealt in this way there are, of course, precisely 635,013,559,600 different* hands that can appear. All these billions of hands are, furthermore, equally likely to occur; and one of them is absolutely certain to occur every time a hand is so dealt. Thus, although any one particular hand is an improbable event, and so a rare event, no one particular hand has any right to be called a surprising event. Any hand that occurs is simply one out of a number of exactly equally likely events, some one of which was bound to happen. There is no basis for being surprised at the one that did happen, for it was precisely as likely (or as unlikely, if you will) to have happened as any other particular one.

So we see that an event should not necessarily be viewed as surprising, even though its a priori probability is very small indeed. If you ever happen to be present at the bridge table when a hand of thirteen spades is dealt, remember that what you ought to say is this: "My friends, this is an improbable and a rare event: but it is *not* a surprising event. It is, however, an interesting event."

For it remains stubbornly true that, under the circumstances described, we are sometimes interested in the outcome, and sometimes uninterested. It seems clear that this occurs because the hands, although equally likely, are just not equally interesting. Thirteen spades *is* an interesting hand, and indeed a Yarborough with no cards above the ten, is interestingly poor; whereas millions of the hands are dull and wholly uninteresting messes.

On the other hand, think of a round disk of metal, like a coin except with perfectly plane faces and no milling on the edge. If this coin-disk were tossed, and landed in an almost perfectly vertical position, with practically no spin, it would remain standing on its edge. One could easily adjust the thickness and the mode of spinning so that the probability of its coming down and standing on its edge would be, say, one in a billion.

When this metal disk is tossed, there are, then, three possible outcomes. It may come up heads (for which the probability differs from one half by one half of a billionth), or it may come down tails (which is exactly as likely as heads), or it may stand on its edge (for which the probability is one one-billionth).

Either heads or tails would be probable, not rare, uninteresting, and not surprising. Standing on edge would be improbable, rare, interesting, and surprising. But this surely surprising event is more than 635 times as probable as would be an uninteresting hand in our previous illustration. So standing on edge is not surprising just because it has a low probability. Why is it surprising?

It is surprising, of course, not because its probability is small in an absolute sense, but rather because its probability is so small as compared with the probabilities of any of the other possible alternatives. Standing on edge is half a billion times as unlikely as the only other two things that can happen, namely, heads or tails: whereas the dull hand is precisely as likely (or unlikely) as any of the possible alternative outcomes of the dealing process.

* Order of appearance of cards in the dealing process is not taken into account: only the final constitution of the hand. Two hands are, of course, "different" if they differ in one or more cards.

Thus one concludes that *probability* and *degree of rarity* are essentially identical concepts: that a *rare* event is *interesting* or not depending on whether you consider it interesting or not[†] and that an event is *surprising* only providing its probability is very small as compared with the probabilities of the other accessible alternatives. This requires that a *surprising* event be a *rare* event, but it does not at all require that a *rare* event be a *surprising* event.

It is easy to make this idea more precise, and to define a Surprise Index (S. I.) for an event. To do this, it is necessary to say a preliminary word about what is called, in probability theory, mathematical expectation, or simply expectation.

If a probability experiment can result in any one of n ways, W_n, and if the a priori probabilities of these various ways are $p_1, p_2, \ldots p_n$, then any quantity Q which takes on the values $Q_1, Q_2, \ldots Q_n$ as the experiment eventuates in its possible ways W_n is said, relative to this experiment, to have the expected value

$$E(Q) = p_1Q_1 + p_2Q_2 + \ldots + p_nQ_n.$$

This definition sounds a little formidable, but the essence of the matter is really very simple. For, since the probabilities can be thought of as the relative frequencies of occurrence of the various outcomes, the formula gives a weighted average of Q, weighting each possible value of Q proportionately to the frequency with which it may be expected to occur. That is to say, the mathematical expectation of any quantity Q is simply the average value that Q may be expected to tend toward as the number of experiments becomes large.

Now, when a probability experiment is carried out, one of the ways W necessarily occurs—that is, in fact, what we mean by "doing the experiment." This way W (the one that actually happens) had a certain a priori probability p that it would occur; and, since it has occurred, we can say that we have realized, in this experiment, a probability p. How much probability can one expect on the average to realize in a given experimental setup? We have just seen that mathematical expectation answers precisely this kind of question. We must calculate the expected value of the probability. Thus

$$E(p) = p_1^2 + p_2^2 + \ldots + p_n^2$$

is the average amount of probability we can expect to realize per trial of the experiment in question. And the way, say, W_i that the experiment actually comes out, is to be considered surprising, according to whether the Surprise Index

$$(\text{S.I.})_i = \frac{E(p)}{p_i} = \frac{p_1^2 + p_2^2 + \ldots + p_n^2}{p_i}$$

is large compared with unity or is not. The S. I., as so defined, obviously measures whether the probability realized, namely, p_i, is small as compared with the probability that one can expect on the average to realize, namely, $E(p)$. If this ratio is small and S. I. correspondingly large, then one has a right to be surprised.

For the card-hand illustration, all the p_i are equal, and at once it turns out that the Surprise Index for any such hand is equal to unity. Thus in this experiment no outcome, however rare it may be, however interesting or uninteresting, is in the least surprising.

For the coin illustration, however, if we say that W_1 denotes a head, W_2 a tail, and W_3 standing on edge, then it is at once calculated that $(\text{S.I.})_1$ and $(\text{S.I.})_2$, which are equal, differ from unity only in the eleventh decimal place; whereas $(\text{S.I.})_3$ is within one of being a half million. Standing on edge is, therefore, decidedly surprising, as well as being improbable and interesting; whereas neither a head nor a tail is at all surprising.

It was remarked, earlier, that a hand of thirteen spades is rare and interesting, but should not be viewed as surprising. How about two hands of solid trumps in one evening or, what is entirely equivalent, any two identical hands in one evening? That is a very different matter! If an experiment consists of dealing just two hands, then this experiment can come out in either one of two ways. It may, way W_1, result in two unlike hands, or it may, way W_2, result in two identical hands. Since W_2 has one chance in many billions of occurring, it is easily calculated that the Surprise Index for W_1 is essentially unity, whereas the S. I. for W_2 is an extremely large number. Thus, two unlike hands in succession are not in the least surprising, whereas two alike in succession are very surprising indeed.

This calculation also indicates that there is in fact a viewpoint which justifies surprise at a single hand of thirteen spades. Suppose, for example, that one is so much interested in a perfect hand that he lumps together all imperfect ones. That is, for this person there are only two sorts of hands, perfect and imperfect. For him the deal of a hand comes out in only two ways. The calculation of the Surprise Index corresponding to these two ways is, of course, exactly that given in the preceding paragraph. The S. I. for an imperfect hand is essentially unity, whereas the S. I. for a perfect hand is more than 635,000,000,000. It is therefore necessary to modify the general statements made previously and say that when an improbable event is so interesting that all its alternatives are lumped together as events so dull as to be indistinguishable, then the interesting event may thereby become a surprising event.

[†] It may perfectly well (alas for after-dinner conversation) be interesting to you, say, because it happened to you before, and not at all interesting to anyone else.

For those who play with statistics it may be worth while to point out that it seems to be difficult indeed to calculate, in general terms, the expectation $E(p)$ of the probability for either the binomial or the Poisson case. The resulting series involves squares of factorials, which are most unpleasant.[‡]

For the normal case, it is trivially easy to show that the expectation in the probability density, as expressed in terms of the standard deviation σ, is given by $1/2\sigma\sqrt{\pi}$. The Surprise Index for a deviation x from the mean is found at once to be equal to $\sqrt{2}e^{x^2/2\sigma^2}$. Thus the Surprise Index is large compared with unity provided x is large compared with σ; which is of course just what one would expect.

This last calculation, because of long experience with, and feel for, standard deviation, gives one a little notion as to what would be a sensible agreement, throughout this discussion, as to what "large" ought to mean. A Surprise Index of 3 or 5 is surely not large: one of 10 begins to be surprising: one of 1,000 is definitely surprising: one of 1,000,000 or larger is very surprising indeed: one of 10^{12} would presumably qualify as a miracle.

Also, for those who have a more professional interest in statistics, it may be interesting to mention that the ideas here presented have some rather remote connection with the concept of "likelihood" as introduced more than twenty years ago by R. A. Fisher. The simplest case to which Fisher's notions apply is similar to the cases here considered in that a probability event has occurred and one wishes to draw certain inferences from this fact of occurrence. But, in the Fisher case, the probability of occurrence depends upon some unknown parameter, and the main point is that one is attempting to infer some information concerning this unknown parameter. In the cases to which the ideas of the present article apply, there is no such unknown parameter, it being assumed that all the necessary facts are at hand to permit a precise calculation of the probabilities in question. In actual experiments, it is seldom if ever true that all the facts for an absolutely precise calculation of the probabilities are indeed available. Thus, in a card experiment, one could not be entirely certain that the shuffling process is perfect. Indeed, one could have quite an extensive argument as to what "perfect" means. Thus, in actual experiments, it is quite likely to be true, at least in a precise sense, that certain unknown parameters are present: and therefore the Fisher theory is, if one wishes to be quite precise, necessary. I do not think that this destroys the interest in the above remarks, which do apply to those many instances in which one is quite willing to simplify the situation by agreeing that the facts concerning the probabilities are known.

The excuse for making these rather obvious, but still not trite, remarks to a general audience is that all scientists have to be concerned with probability; and it is not altogether rare, although surely surprising, to find scientists who are surprised to find improbable things occurring. They always have a right to be interested, but only seldom do they have a right to be surprised.

It is hoped, then, that the considerations here presented will give some pause to those who say too glibly and too simply: "But the theory of this experiment *must* be accepted, for the probability of the results occurring by pure chance is only 10^{-20}. Surely nothing as improbable as that could ever occur." The mere fact that a probability is low should not of itself lead to incredulity. In fact, life consists of a sequence of extremely improbable events. One must always, before concluding that a certain event is surprising, examine the probability of the alternatives. For there are circumstances, and an evening of hands of bridge is a very good example, under which the occurrence of an over-all event of fantastically small probability is, in fact, inevitable.

[‡] I have spent a few hours trying to discover that someone else had summed these series and I spent substantially more trying to do it myself; I can only report failure, and a conviction that it is a dreadfully sticky mess.

ON LIVING IN THE BIOSPHERE

G. E. HUTCHINSON

In the article below, which is from a paper presented in the symposium on "The World's Natural Resources" (AAAS Centennial, September 13-17), Professor Hutchinson claims originality only for some of the computations and part of the interpretation—most of the fundamental data are easily accessible in the scientific literature. Professor Hutchinson is on the staff of the Department of Zoology at Yale and consultant in biogeochemistry, American Museum of Natural History.

IN discussing the subject of "The World's Natural Resources," I want first to make a number of general observations that will provide an intellectual framework into which our developing knowledge, both academic and practical, may be fitted. We live in a rather restricted zone of our planet, at the base of its gaseous envelope and on the surface of its solid phase, with temporary excursions upwards, downwards, or sideways onto or into the oceans. These regions in which we can live and which we can explore are characterized by their temperature, which does not depart far from that at which water is a liquid, and by their closeness to regions on which solar radiation is being delivered. This zone of life is spoken of as the biosphere. Within it, certain natural products can be utilized in both biological and cultural life. It is customary to consider these resources as either material or energetic, but the two categories are not easily separable; contemporary solar radiation is an energetic resource, coal and oil are to be regarded as material resources valuable for their high energy content, which we may call, epigrammatically, fossil solar radiation. There is a third very important though inseparable aspect, namely, the pattern of distribution. Most fossil sunlight, or chemical energy of carbonaceous matter, is diffused through sedimentary rocks in such a way as to be useless to us. Schrödinger says that we feed on negative entropy, and I am almost tempted to regard pattern as being as fundamental a gift of nature as sunlight or the chemical elements.

The first major function of the sunlight falling on the earth's surface is as the energy of circulation of the oceans and atmosphere. The second is to increase the mobility of water molecules, to become latent heat of evaporation, and so to keep the water cycle operating. The third major function is photosynthesis. Apart from atomic energy, and a little volcanic heat which presumably is actually of radioactive origin, all industrial energy is solar and due to one or the other of these three processes.

The material requirements of life are extremely varied. Between thirty and forty chemical elements appear to be normally involved. Industrially, some use appears to be found for nearly all the natural elements, and some of the new synthetic ones also. Looking at man from a strictly geochemical standpoint, his most striking character is that he demands so much—not merely thirty or forty elements for physiological activity, but nearly all the others for cultural activity. What we may call the anthropogeochemistry of cultural life is worth examining. We find man scurrying about the planet looking for places where certain substances are abundant; then removing them elsewhere, often producing local artificial concentrations far greater than are known in nature. Such concentrations, whether a cube of sodium in a bottle in the laboratory, or the George Washington Bridge, have usually been brought into being by chemical changes, most frequently reductions, of such a kind that the product is unstable under the conditions in which accumulation takes place. Most artifacts are made to be used, and during use the strains to which they are submitted distort them, and they become worn-out or broken. This results in a very great quantity of the materials that are laboriously collected being lost again in city dumps and automobile cemeteries. The final fate of an object may depend on many factors, but it is probable that in most cases a very large quantity of any noncombustible, useful material is fated to be carried, either in solution or as sediment, into the sea. Modern man, then, is a very effective agent of zoogenous erosion, but the erosion is highly specific, affecting most powerfully arable soils, forests, accessible mineral deposits, and other parts of the biosphere which provide the things that *Homo sapiens* as a mammal and as an educatable social organism needs or thinks he needs. The process is continuously increasing in intensity, as populations expand and as the most easily eroded loci have added their quotas to the air, the garbage can, the city dump, and the sea.

The most important general consideration to bear in mind in discussing the dynamics of the biosphere and its inhabitants is that some of the processes of significance are acyclical, and others, to a greater or less degree, cyclical. By an acyclical process will be meant one in which a permanent change in geochemical distribution is introduced into the system; usually a concentrated element tends to become dispersed. By a cyclical process will be meant one in which the changes involved introduce no permanent alteration in the large-scale geochemical pattern, concentration alternating with dispersion. The cyclical processes are not necessarily reversible in a thermodynamic sense; in fact, they are in general no more and no less reversible than the acyclical. Most of the cyclical processes operate because a continuous supply of solar energy is led into them, and sunlight will provide no problems for the conservationist for a very long time. It is important to realize that most of the acyclical processes are so slow that man appears as an active intruder into a passive pattern of distribution. They are safer to disturb because we know what the result of the disturbance will be. If we mine the copper in a given region sufficiently assiduously, we know that ultimately there will not be any more copper available there. Cyclical processes involve complex circular paths, regenerative circuits, feedback mechanisms, and the like. Small disturbances of such processes may merely result in small temporary changes, with a rapid return to the previous steady state. This has been beautifully demonstrated in the experiments of Einsele, who added single massive doses of phosphate to a lake, changing for a time, but only for a time, its entire chemistry and biology. This stability does not imply that if large disturbances strain the mechanism beyond certain critical limits very profound disruption will not follow; in fact, the very self-regulatory mechanisms that give the system stability against small disturbances are likely to accentuate the disruption when the critical limits are transcended. In disturbing cyclical processes we usually do not know what we are doing.

The most nearly perfect cyclical processes are those involving water and nitrogen. Some losses to the sediments of the deep oceanic basins must occur, but they are very small and are doubtless fully balanced or more than balanced by juvenile water and perhaps by molecular nitrogen and ammonia of volcanic origin.

The least cyclical processes are those in which material is removed from the continents and deposited in the permanent basins of the ocean. With one or two exceptions, the delivery to the deep-water sediments of the ocean is of little significance. Most of the mechanical and chemical sedimentation in the oceans takes place in relatively shallow water. The uplifting of shallow water sediments constitutes an important method of completing cycles. One, and perhaps two, exceedingly important elements are, however, sedimented less economically. Calcium, during the Paleozoic, was mainly precipitated in shallow water, but since the rise of the pelagic foraminifera in the Mesozoic, a great deal of calcium, along with an equivalent amount of carbon and oxygen, has been continually diverted to regions from which it is unlikely ever to be removed. At present the sedimentary rocks of the world are an adequate biological and commercial source of calcium, but, with progressive orogenic cycles, less and less of the element will be uplifted (Kuenen), and whatever organisms inherit the earth in that remote future will have to face the problem of the biosphere "going sour on them." For phosphorus, the case is less well established, but there is probably a slow loss in the form of sharks' teeth and the ear bones of whales (Conway), which are very resistant and which are known to be littered about on the floor of the abysses of the ocean.

It is desirable to consider two of the main geochemical cycles in order to gain an idea of the effect of man upon them. It must be admitted that we are ignorant of many matters of importance here. In the cycle of carbon we have a remarkable, possibly a unique, case in which man, the miner, increases the cyclicity of the geochemical process. It is generally admitted that our available store of carbon is ultimately of volcanic origin. A steady stream of carbon dioxide and lesser amounts of methane and carbon monoxide are entering the atmosphere from volcanic vents. Part, probably a major part, of this carbon dioxide is ultimately lost to the marine sediments as limestones; a very small part of it is then returned to the air, wherever lime kilns are in operation. Another part of the CO_2 entering the atmosphere is reduced in photosynthesis. A part of the organic matter so formed is fossilized, and a small part of this fossilized organic carbon is available as fuel, in the form of coal and oil. At the present time it appears that the combustion of coal and oil actually returns carbon to the atmosphere as CO_2 at a rate at least a hundred times greater than the rate of loss of all forms of carbon, oxidized and reduced, to the sediments (Goldschmidt). This particular process obviously cannot go on indefinitely. It concerns only the reduced carbon; to complete the cycle in the case of oxidized carbon, a great deal of energy would have to be supplied. It concerns only the reduced carbon which is aggregated. The poorest sources would be the poorest exploitable oil shales. Most of the reduced carbon is much more dispersed than this; in making an estimate of the total reduced carbon of the sediments, the commercially usable fuels constitute a negligible fraction that need not be considered.

Although the rate at which carbon dioxide is returned to the air by the human utilization of fossil fuels is so very much greater than is the primary production of carbon dioxide from volcanic sources, the rate is evidently a very small fraction—of the order of 1 percent—of the rate of photosynthetic fixation and subsequent respiratory liberation of CO_2 by the organisms of the earth. Since about 1890 a slight increase in CO_2 content in the air, at least at low altitudes over the land surfaces of the Northern Hemisphere, has been noted. This has been attributed to the accumulation of industrially produced CO_2, as the quantity of CO_2 that has appeared in the atmosphere is of the same order of magnitude as the total combustion of fuel (Callendar). In view of the small fraction of the total CO_2 production that industrial output represents, it seems very unlikely that merely adding an extra percent to the natural biological production should overload the cyclic process, so that it rejects quantitatively the additional load. It is known that the air at high altitudes still shows nineteenth-century values. The most reasonable explanation of the observed increase is that the photosynthetic machinery of the biosphere has been slightly impaired, probably by deforestation. It is clear that, in any intelligent long-term planning of the utilization of the biosphere, an extended study of atmospheric gases is desirable, even though for the moment it seems unlikely that the observed change is a particularly serious symptom.

The only other cycle that can be considered in any great detail is that of phosphorus. The chief event in the geochemical cycle of phosphorus is the leaching of the element from the rocks of the continents, and its transport by rivers to the sea. At the present time the rate of this transportaion is of the order of 20,000,000 tons of phosphorus per year for the entire earth. Part of this phosphorus, when it enters the sea, will ultimately be deposited in the sediments of the depths of the ocean. Such phosphorus will probably be largely lost to the geochemical cycle, as has just been indicated. The sedimentary rocks of the continents, therefore, will gradually lose phosphorus; there is some evidence that this has actually occurred (Conway). The main return path is by the uplifting of sediments formed in continental seas, which then undergo renewed chemical erosion. Of particular interest are methods by which concentrated phosphorus can be returned to the land surfaces. As far as is known, there are two such methods: The first is the formation of phosphatic nodules and other forms of phosphate rock in regions of upwelling in which water at a low *p*H, rich in phosphate, is brought up to the surface of the sea. The *p*H falls and an apatite-like phosphate is deposited. When the sea floor is later elevated, a commercial deposit may result. The second method is by the activity of sea birds, such as the guano birds of the Peruvian coast. There is little or no unequivocal evidence that guano deposits of great extent were formed prior to the late Pliocene or Pleistocene. Some of the well-known occurrences of rock phosphate, such as that of Quercy, have been explained in this way, but they are certainly not typical guano deposits. During the late Tertiary and Pleistocene, an extraordinary amount of phosphate was deposited on raised coral islands throughout the world, and bird colonies seem to provide the only reasonable agencies of deposition. The great deposits of Nauru, Ocean Island, Makatea, Angaur, the Daito Islands, Christmas Island south of Java, Curaçao, and some of the other West Indies all seem to have been formed in this way. This process is as characteristic of the time as is glaciation, though less grandiose. Its meaning is not clear, but it is probably connected with changes in vertical circulation of the ocean as glaciopluvial periods gave place to interpluvials. Today in certain regions massive amounts of guano are deposited, and it is probable that the oceanic birds of the world as a whole bring out from several tens to several hundreds of thousands of tons of phosphorus and deposit it on land. Only about 10,000 tons of the element are delivered in places where it is not washed away and where it can be carried by man to fertilize his fields.

The main processes that tend to reverse the phosphorus depletion of the continents are, therefore, the deposition of marine phosphorites on the continental shelves and subsequent elevation, and the formation of guano deposits. Both processes are evidently intermittent, and are quantitatively inadequate to arrest deflection of the element into the permanent ocean basins. Man contributes both to the loss and to the gain of phosphorus by land surfaces. He quarries phosphorite, makes superphosphate of it, and spreads it on his fields. Most of the phosphorus so laboriously acquired ultimately reaches the sea. At present the world's production of phosphate rock is about 10,000,000 tons per annum. This contains from 1,000,000 to 2,000,000 tons of elementary phosphorus. Human activity probably, therefore, accounts for from 5 to 10 percent of the loss of phosphorus from the land to the sea. Man also contributes to the processes bringing phosphorus from the sea to the land. This is done by fisheries. The total catch for the marine fisheries of the earth is of the order of $25–30.10^6$ tons of fish, which corresponds to about 60,000 tons of elementary phosphorus. The human, no less than the nonhuman, processes tending to complete the cycle seem miserably inadequate. It is quite certain that ultimately man, if he is to avoid famine, will have to go about completing the phosphorus cycle on a large scale. It will be a harder task than that of solving the nitrogen problem, which would have loomed large in any symposium on "The World's Natural Resources" fifty years ago, but possibly an easier problem than some of the others that must be solved if we are to survive and really become the glory of the earth.

The population of the world is increasing, its available resources are dwindling. Apart from the ordinary biological processes involved in producing population saturation already known to Malthus, the current disharmony is accentuated by the effect of medical science, which has decreased death rates without altering birth rates, and by modern wars, which one may suspect put greater drains on resources than on populations. Terrible as these conclusions may appear, they have to be faced. The results of the interaction between population pressure and decline of resource potential are further partly expressed in such wars, which are pathological expressions of attempts to cope with these and other problems and which now invariably aggravate the situation. It is evident that the fundamental causes of war lie in those psychological properties of populations which make them attempt solutions in a warlike manner, and *not* in the existence of the problems themselves. The two problems of war and of resources are, however, at present very closely interrelated; it is probably impossible to find a solution for one without progress in the solution of the other. Any kind of reasonable use of the world's resources involves better international relations than now exist. It is otherwise impossible to operate on a planetary basis, or to avoid the fearful material and spiritual expense of living in a world divided into two armed camps. The lack of international trust is the first difficulty in achieving rational utilization of the resources of the world; the second difficulty for the United States is what may be called the problem of the transition from the pioneering to the old, settled community. For a pioneer, life may be hard, but in good country there is "plenty more where these came from," whether lumber or buffalo tongues or copper. This attitude is incorporated into popular thought very deeply; in a crude form, it is now completely destructive, but it seems possible that attitudes might be developed that could utilize such a point of view. There is at least one thing of which we have plenty more than we use, whether we call it Yankee ingenuity, American know-how, or the human intellect. In some industrial fields there has been a notable series of triumphs of the kind that really will give plenty more of a number of things, and give them in a cyclic manner. The rise of the magnesium industry, and the utilization of the magnesium of sea water as a source of the metal is an interesting example. The production of plastics, though it is probably at the moment not as geochemically economical as it should be, is another. These, along with the development of silicate building materials not involving any particularly uncommon elements, all point the way to the kind of material culture that permits a reasonably high living standard, at least in certain directions, without devastation of the earth.

The future outlook for the world, particularly in food resources, has been put before the public in several recent books, notably those of Fairfield Osborn and William Vogt. Anyone with any technical knowledge understands that the dangers described in these books are real enough, more real and more dangerous perhaps than the threat of an atomic world war. The problem of getting action to forestall such dangers in a culture that has developed under conditions of potentially unlimited abundance just around the corner, is obviously extremely difficult. I do not think it is impossible. The first requirement is a faith that the job can be done. The very difficulty of the task, its apparent impossibility, may here and now prove a challenge that brings the desired response. The difficult we do at once, the impossible takes a little longer. I doubt that a direct appeal to fear will produce any results except a disbelief in the prophets of doom. Cassandra seems even more unpopular in modern America than in ancient Ilium. There would seem to be forces operating in society which tend to reverse the destructive processes, or which could be made to do so. One of the most immediate needs is to find out what they are and do everything possible to strengthen them. I will give one example. A number of industrial concerns—particularly in the chemical and pharmaceutical field, but also some engineering and publishing firms—have used in their advertising a legitimate pride in the learning, skillful research, and development that have gone into the manufacture of their product. It is quite certain that there are many cases in which one particular product, the result of considerable research, and of taking risks in development, actually reduces the drain on the natural resources of the country and the world. I should like to see a small systematic experiment, on the part of some such concern, in advertising in which it is pointed out that by buying this product one is letting the industrial skill behind the product operate for the benefit of one's children. I am fully aware that if this point of view were worked up skillfully enough by a few responsible, public-spirited corporations and put into a form that would pay the corporation, as well as the country and the world, there would be other less responsible concerns who would use the method when their product is actually not one sparing natural resources. This, however, seems to be a lesser evil than the total neglect of the commercial advertising field, which is one of the most potent in determining the values of the public and which at present is largely disruptive. I do not doubt that a professional cultural anthropologist could pick out a great many other fields that could be used to promote the idea of an expanding economy based on an abundance of human ingenuity rather than on an excess of raw materials.

There is one further point that I should like to develop. Though the pursuit of happiness is embedded deeply in the constitutional foundations of the United States, we do not know much about it. It is fairly certain that no metric exists that can be applied directly to happiness, but intuitively we may proceed a little way by arguing as if such a metric could exist. It is obvious that only a very few people, with a genius for sanctity, can be happy if half-frozen and starving. If the temperature be raised, and the food supply and other amenities increased, the possibility of happiness is obviously at first also increased, but beyond a certain increase in the environmental resources available no further increase in happiness would be expected and we might begin to look for a decline. The image of an overheated kitchen, filled with too much electrical equipment and catering to overfed people, will, if adequately evoked, have a nightmarish quality. In more formal language, if we could find a function of the environmental resources that expressed the relationship of happiness to those resources, it is reasonably certain that the function would not be monotonic. For every resource there seems likely to be an optimum level of consumption, but we do not know if this optimum level is, in any particular case, widely exceeded, so that gross overutilization is actually producing avoidable distress.

The problem is not by any means as simple as that of determining discrete optima. In any given society all the cultural values are probably interrelated to form a coherent system, so that the existence of one set of values may greatly modify the others. If, as seems possible, our attitude toward food leads a considerable section of our population to be definitely overweight, it is legitimate to inquire to what extent, by changes in the upbringing of our children, the psychological needs filled by food can be satisfied in other ways so that the psychologically optimum intake falls to a level nearer the psychological optimum for the individual and the moral optimum for the world. We might ask, for instance, how we can substitute the delights of ballet and Mozartian opera, which are geochemically very cheap, for part of those provided by hot dogs or apple pie and ice cream, which may in the long run prove too expensive to use except as a source of energy and essential nutrients. This example is chosen with a view to indicating that the kind of substitutions that might be considered need not be in the least puritanical. It may appear overintellectualized, but that at least is a guarantee that it is not inhuman. Indeed it raises the very interesting problem that those of us in the educational world have to face, namely, why we are raising a generation in the belief that the majority of constructive, complicated, difficult activities are boring duties, when the same generation shows us that in certain specific fields, mainly concerned with electronic amplifiers and with the explosive combustion of hydrocarbons, complicated activity can be very entertaining. What we have to do is to show by example that a very large number of diversified, complicated, and often extremely difficult constructive activities are capable of giving enormous pleasure. This is, in fact, the reason why it is essential that the teachers in our colleges and universities should be enthusiastic investigators in the fields of scholarship or practitioners and critics in their arts. It ought to be possible to show that it is as much fun to repair the biosphere and the human societies within it as it is to mend the radio or the family car.

WOOD IN AN INDUSTRIAL WORLD

J. A. HALL

Dr. Hall (Ph.D., Wisconsin, 1934) is director of the Pacific Northwest Forest and Range Experiment Station, USDA, at Portland, Oregon. Speaking as a biochemist, he delivered the address from which this article is taken during the Centennial Celebration of the AAAS in Washington, September 13–17.

WE LIVE in the industrial revolution that began early in the nineteenth century and was cradled in Western Europe. Within the short period of a century and a half we have witnessed a violent transformation in the economic and social aspects of human society influenced by a completely revolutionized technology. We have also seen an explosive upsurge in world population that has influenced the course of the industrial revolution itself and made necessary the extension of the industrial system to the outermost regions of human habitation. Since what we know as the industrial system seems to have become a permanent part of organized human life, and since its existence and perpetuation depend so much upon the abundant availablity of raw materials, it is peculiarly appropriate that we should examine the place in this system of one of our most abundant raw materials—wood.

WOOD AND THE INDUSTRIAL REVOLUTION

Historically, wood was important in the early beginnings of the industrial revolution. It had been a vital fuel stuff, an important part of the crude machinery that antedated the advent of modern, more complex machines, the material of which ocean-going transport was built, and the principal component of all land-borne, wheeled transport. The basic raw materials for the development of the modern factory system were present in Europe and North America, but they awaited the catalytic effect of the application of steam to power machinery before they could be put together into the industrial system that arose rapidly on both continents. The requirements for industrialization are and have always been (1) a base of scientific knowledge, especially in the fields of application of energy in the transformation of materials; (2) large sources of available energy; and (3) raw materials of varied nature and large supply, both inorganic and organic in their nature.

In Europe the source of energy in the early days of industrial development was present in a fairly well-developed coal-mining industry in England and Belgium. Iron ore was available in the mountains of Europe in quantity and quality for the fabrication of pig iron and, later, steel. Higher grades of iron and steel have long been manufactured in the Scandinavian peninsula, where wood was an abundant resource and made ample supplies of charcoal available. These fundamental ingredients together made the rise of organized and integrated industry in Europe a possibility.

In North America matters took only a slightly different course. Abundant wood in the East made it a satisfactory power fuel for a long period after coal had become the standard power fuel in Europe. As a matter of fact, wood powered the first steam-operated factories and the first steam locomotives in America. In 1814 our total coal production was reported as 20 long tons. It is a matter of historical record that the cutting of the forests for fuel created the serious local shortages in New England and Middle Atlantic states that were reported about 1820. Need of charcoal for iron caused the harvesting and depletion of vast areas of oak in the central hardwood region until the advent of the Bessemer furnace. From 1830 to 1860, power requirements far outran wood as a supply of fuel, and coal entered upon its continuing role as an industrial fuel.

Thus coal and iron ore became the principal factors in that rapid transformation from handicraft industry to the factory system that characterized most of the nineteenth century. It is of importance to note that neither is obtained in great quantity without the service of wood. Except for the limited amount mined from near the surface in open pits, all coal comes from mine tunnels supported by wooden props: "pit props" in Europe, "mine timbers" in America—a lowly and humble service, indeed, performed by wood to help produce the material that long ago displaced it as an industrial fuel. The volume of wood required for this purpose is considerable. For example, England, in 1947, required nearly 91 million board feet of pit props, of which she imported well over one third. Finland, Sweden, and Norway, rich in wood but poor in coal, export pit props and import coal.

New basic raw materials and new systems of manufacture brought about new and greatly increased needs in transportation. Highly developed transportation systems for the functioning of coordinated industrial setups all over the world arose in the form of railroads that run on wooden crossties, or "sleepers," as they are called in Europe. In the United States today more than a billion wooden crossties are in use, enabling the rapid transportation of the millions of tons of raw and finished goods that constitute the supply and product of our complex factory system. It takes about 100 crossties per mile per year to service the 420,000 miles of railroad trackage in the United States. Within the past fifty years, the length of service of railroad ties has been trebled by impregnation with coal-tar creosote, a by-product of the industrial system itself, based on coal.

Even the tracks upon which the coal is hauled from the mine, or the iron ore from the pit or shaft, the slides within which the hoists operate, the handles of the tools of the laborers, all are made of wood.

Wood is a necessity in the production of the raw materials of industry and in the transportation systems weaving industry together. The web of communication systems that bind industrial centers throughout the world is mostly carried on wood. In the U.S.A., 90 million miles of telegraph and telephone wire, half of it carried above the ground on wooden poles, are an indispensable part of the industrial setup.

The transformations that came upon the Western world with the advent of organized, integrated industry had nowhere more drastic effect than on the relations existing between urban and agricultural populations. At the time of the beginning of the industrial revolution, the world's population may be said to have been mostly occupied with the business of feeding itself by the application of comparatively primitive tools and methods to agriculture. The advent of industrial production based on steel not only made necessary and possible the development of large centers of industrial population drawn from hitherto rural populations; it made possible, also, great increases in the ability of the agricultural worker to produce food on the farm by supplying him more efficient implements and machinery.

Briefly, the concentrations of population in industrial centers that were required as the system of industrial production developed could not have occurred unless members of rural communities could have been spared from the production of food. Nor could they have been fed had not the productivity per man-unit on the farm been greatly increased. The new agricultural lands of North America literally fed the developing industrial centers of western Europe during their formative years. Industrial Europe still depends to a very large degree upon imported food for its existence. Industrial centers in America depend upon the rapid transportation of food supplies from our agricultural regions for their support.

The volume of food production necessary for the maintenance of the rapidly increasing industrial population was largely obtained as a result of the expansion of agricultural lands in America. This process took place at a tremendous rate and consumed huge amounts of wood for housing, fencing, and farm structures. The two processes of expansion of needs for urban housing and rural housing went on together, and their requirements were met partly by the clearing of the virgin forests from the needed land itself. This process in the United States reached its peak about 1910.

The ratio of this shift of population proportions is well expressed in the statement that, in an industrial country, about one seventh the man power is now required to produce a bushel of wheat that was required a hundred years ago. The tools with which agriculture has become more efficient in the production of food, thus making heavy industrial concentrations possible, have been in turn the product of organized and integrated industry. Wood catalyzed the beginnings of industrial manufacture, serving as a crude beginning material. It continued to serve during the development of the industrial system, and its availability and usefulness were greatly enhanced by the application of the steel tools of production and manufacture that were in turn the product of industry itself.

The pattern of development of an industrial center was almost uniform. First, there came temporary industrial structures and residences, mostly of wood. As technology and markets developed, and permanence for an industrial organization became assured, building took on more lasting character. Industrial structures of steel and concrete and brick became standard, but by far the major part of the dwelling house was still of wood, and wood was even used in the erection of steel and concrete structures.

The industrial system produces the tools of production in increasing abundance, and its products exert great influence upon the continually changing characteristics of the system itself. Steam and steel catalyzed by wood made possible the industrial production of petroleum. There could have been no development of any consequence of the internal-combustion engine until petroleum had become an available industrial raw material in some quantity. Nor could the internal-combustion engine have been of any importance without the availability of huge quantities of steel. Thus the secondary revolution created by the advent and widespread adoption of the internal-combustion engine may be considered to have been a product of the fundamentals of the industrial revolution itself. Similar reasoning can be applied to the remarkable increase in the use of electrical energy. Its large-scale production would not have been possible without the large-scale production of steel and copper, which in themselves are major products of the industrial system.

APPLICATION OF TOOLS AND TECHNIQUES

Briefly sketched, this is the history of the rise of the industrial world. Since a principal characteristic of the system has been and continues to be the application of new tools and techniques to the transformation of materials, it is well to examine the ways in which industry has learned to modify the form and character of wood, the most abundant renewable resource available to it. The industrial system started with wood and, as it developed, increased the requirements for wood to an enormous extent. These requirements have been met by using the new tools of industry in the harvesting and processing of wood.

There are three general ways in which wood enters into use:

1. Through its use as wood itself, its physical form alone being modified to meet diverse requirements.
2. Through improvement of the physical properties of wood by various treatments while preserving its fundamental structure.
3. Through the use of wood as a raw material in the chemical engineering industry.

In the first category one need only examine the woodworking tools of a century ago to understand both their usefulness and limitations. Wood generally is porous, light, strong for its weight, easily shaped with simple tools, and possessed of oriented strength properties that are its exclusive possession among materials. Even in the early stages of industrial development, its workability and adaptability made it a preferred material. Harvested from the forest by the ax and saw, then transported as logs by sled, wagon, and oxen, and, finally, laboriously sawed into boards by hand or slow, water-powered, vertical saws, or even riven into boards by the maul and froe, the shaped pieces of raw wood filled a myriad of uses in a changing world.

But the products of industry in the form of new, tough alloys and machine tools worked a subrevolution in the methods of harvesting and processing raw wood. The rapidly expanding need for lumber to house industrial populations could not have been satisfied except by the application of the tools produced by industry itself. The major equipment of modern logging is steel, from the blade of the ax of the faller to the tractor and arch and the huge trucks that transport the logs to market. Chain saws of new alloys, powered by light gasoline motors, have removed much of the drudgery of logging and greatly increased the production of logs per man-day of labor. In the mills high-speed band saws of tough steel cut boards at an incredible rate. The products of the steel and petroleum industries have revolutionized the lumber-producing industry, and made possible the production of the astronomical quantities of wood required to maintain the industrial system.

In the further shaping of raw wood to its multitude of uses, the employment of high-speed machine tools and assembly-line production makes possible a large and ceaseless flow of houses, furniture, and instruments of modern life that could only take place in a thoroughly mechanized industrial world.

Naturally, an important result of man's ability to harvest and process trees so rapidly and, withal, so wastefully has been the rapid disappearance of the virgin forests that characterized the preindustrial world. The prodigal use of our own forests that has characterized our national life during the past century is without parallel in the world's history. Not only have we become terribly efficient in harvesting and manufacturing wood without regard for the forest, we have also become terribly efficient in clearing land for agricultural use at the expense of the forest, and too often at the expense of the land resource itself. A balance in this matter appears to be in process of achievement; we shall probably keep most of the land now in forests growing trees simply because we shall need the wood to maintain our standard of living. But we will exhaust our virgin forests and be dependent upon smaller logs that we grow for our raw material. We must satisfy the apparently endless demands for wood from forests managed for permanent wood production on land best suited for forest growth.

This does not present insuperable difficulties because, again, the products of industry make possible the efficient use of smaller pieces of wood to do the jobs formerly done by the huge pieces of structural timber cut from the virgin forests. We have learned, for example, to fabricate ingenious fasteners of steel in order to make strong joints between relatively small pieces of wood, and to construct intricate engineering structures of small wooden members joined by steel.

The advent of synthetic resins, products of an organic chemical industry founded upon the distillation of coal, makes possible the construction of glued forms of wood that were not possible before the industrial revolution. Thus, very large beams, pillars, and arches are built up by laminating and gluing thin boards and shaping them to fit their final use. The glued joints are weatherproof. Because of the vast storehouse of knowledge of the mechanical properties of various commercial species of wood accumulated in recent years, wood takes its place as a precise engineering material and lends itself well to mass production of accurately designed structures.

It is a far cry from the laboriously applied artistic veneers of the eighteenth century to the mass-produced, resin-bonded plywood of today. Plywood is a thoroughly modern product of an industrial world. The long, continuous sheets of thin veneer whirling from a huge log being rotated against a steel knife offer as concrete an example as possible of the application of modern tools to a primitive material. The continuous veneer driers, the automatic glue spreaders, the huge presses, all attest the revolution in the usefulness of wood that has come from the application of the tools of industry.

To make the transformation more dramatic, in recent years we are witnessing the application of high-frequency electric fields to the setting of resin glues on wooden joints and surfaces, thus making possible the production of thicker and more intricate forms of shaped and molded laminated wood products, and continuous line production of boards glued up from small pieces.

In the second category of the uses of wood, the fundamental fibrous and oriented structure of the material is preserved while improving its properties and usefulness.

Wood has limitations in use. When moist, it is subject to attack by fungi and insects. In some uses, therefore, it becomes necessary to impregnate it with materials that are toxic to these attacking organisms. Again, the products of the chemical industry of the industrial world have gone far toward removing this limitation and enhancing the usefulness of wood. Because of their porous structure most woods can be easily impregnated with preservative materials, and, for poles, piling, posts, crossties, crossarms, permanent mine timbers, and exposed structural members, this method is in wide use and is the basis of a large, world-wide industry.

Wood is also combustible and, when carelessly used in construction, can constitute a serious fire hazard. It has been amply demonstrated, however, that by impregnating wood with fireproofing chemicals and using such fireproof wood in critical locations the threat of fire, especially in dwellings, can be effectively minimized. It is of importance to note also that modern laminated beams and arches can withstand very high charring temperatures without serious loss of load-bearing strength, whereas metallic structures subjected to the same treatment often soften and crumple.

We often hear that we live in an age of plastics, but we hardly realize that we have always lived in the age of wood and that wood itself is a plastic. Essentially, wood consists of hollow cellulose tubes, impregnated and bound together by lignin. Under proper conditions of moisture and temperature, these bonds soften and flow so that wood can be molded and shaped. Various forms of molded plywood exemplify the principle.

But, in addition, wood is quite compressible. Its volume is roughly two thirds air and one third solid wood substance. When heated and compressed, many relatively soft and light woods can be compressed to one third their original volume, in which state their specific gravity is trebled, and their strength properties correspondingly increased. Such compressed woods absorb moisture extremely slowly and offer considerable promise in specialized fields of use.

If the wood is impregnated with the components of various of the synthetic resins and then subjected to heat and pressure, there are obtained very hard, dense products, practically completely resistant to the absorption of water and of uniform composition throughout. Such materials are new to the industrial world, so new that their range of usefulness and economics has not yet been well explored.

Thus the products of the chemical industry combine with this age-old material, wood, to give new and more useful forms. But what of the use of wood as a raw material of the chemical industry itself?

Wood may well be considered as the most primitive of the materials of crude chemical engineering, for the beginnings of the manufacture of charcoal for fuel are lost in the mists of antiquity. This was an archaic process, of course, but it produced the fuel for the primitive extraction and working of metals and the handicraft industries. With the advent of the industrial age, and the possibility of fabricating large shapes of metal, the dry distillation of wood assumed industrial importance and the volatile products, tars, acids, and wood alcohol, were recovered as chemical raw materials. Although recent advances in synthetic organic and fermentation chemistry have greatly lessened the relative importance of wood distillation, it still operates as an important industry, especially on hardwood species. We are still dependent upon charcoal for the manufacture of carbon disulfide, for example, an indispensable substance in the preparation of viscose from wood pulp.

It is considered possible by some that industrialization of certain tropical regions, poor in coal and oil, can be based on dry distillation of the abundant hardwoods of the tropical forest. Naturally, there are difficult unsolved technical problems, but it is not likely that these tremendous renewable sources of' carbon and its derivatives will long remain unexploited.

It is an interesting historical fact that a principal source of alkali in preindustrial times was wood; the potash of earlier days was leached from wood ashes, and the slow accumulation of many years of tree growth became the base for the earliest soaps and of wide application in handicraft. Naturally, the industrial age itself early began to produce the so-called heavy chemicals in mass quantities, and it was not long until these chemicals were turned to reaction upon wood itself for the production of wood pulp and paper.

The paper-making industry is very old, indeed, and its ancient materials were the agricultural fibers and certain grasses. With the advent of the combination of industrial chemicals and wood, however, a deep transformation in character and magnitude took place, so that paper from wood in huge volume became a leading characteristic of modern civilization. The influence of cheap and abundant paper upon the course of modern society can hardly be estimated and is worthy of volumes of discussion in itself.

Nor is the end of development of the usefulness of defiberized wood in sight. Whether we speak of chemically or mechanically prepared wood fibers, the array of new products and new processes that appear annually assumes staggering proportions. At the one extreme are the simpler wood fibers prepared by the disintegration of wood by mechanical or mild chemical means and their reassembly in new physical form. Some of the most interesting of these take the form of sheets of lightly compressed fibers sandwiched between thin, hard sheets of other materials such as metals, fiber glass, or fabrics sometimes impregnated with synthetic resins to give a bewildering variety of new materials. Or the low-density core may be glued between thin sheets of wood veneers to produce light, strong materials of new beauty and usefulness.

A striking, recently developed material consists of paper sheets, impregnated with synthetic resins, or perhaps with the lignin residue from the pulping of the wood itself. The impregnated sheets, piled to the desired thickness, are subjected to heat and pressure and emerge as a tough, strong material, approaching mild steels in properties. They have a singular advantage, however. Because of their fundamentally fibrous structure, and because the strength properties of the final product can be varied by the orientation of the fibers, it is possible within limits to construct the material to meet specialized requirements of use.

Similarly, as an infinity of specialized pulping processes and pulp treatments has developed, a marvelous variation in the properties of paper for a wide field of uses has been attained. Tough, strong paper for packaging has a far different origin and process than a delicate facial tissue. Book papers and newsprint arise from different woods and different manufacturing processes. And, again, the combination of paper with the new resinous products of the chemical industry is yielding papers with high strength properties, even when wet, for a vast new field of usefulness. It is interesting to speculate upon what the course of World War II might have been but for the packaging of goods in paper and paperboard.

Much of this progress has grown out of our expanding knowledge of the basic nature of the wood fiber itself and the fundamental chemistry of the cellulose molecule. It must be considered that this field of scientific endeavor, a part of the large field of the chemistry of high polymers, has only within very recent years reached a stage in which one may speak of cellulose molecules and chains with some degree of accurate meaning. And it is only still more recently that meaning has been given to the term "reactivity" as applied to cellulose. Even during the developmental period, we have witnessed the advent of new fibers and new resin and lacquer components based on cellulose that required the production of alpha-cellulose of high purity in industrial quantities. Soon, with developing knowledge of the fundamental reasons for the variable properties of industrial cellulose, it will be possible to specify not only the content of alpha-cellulose, but the average chain length and limits between which chain lengths may vary and the specific reactivity of the cellulose chains under definite conditions. These requirements for highly specialized properties in wood cellulose are now known to be characteristics of not only its method of preparation, but of the manner of its formation in the living wood. In the future, we will select species for special uses, and give them even more highly specialized methods of cellulose extraction.

Since industrialization has taken form largely in the north temperate countries, in which the major forests were coniferous, it is not surprising that most development of the paper and cellulose industries has been based upon coniferous woods. Increasingly, the needs for paper and cellulose that accompany the growth of integrated industry have gone beyond local supplies of coniferous wood and into zones of essentially broad-leaved or hardwood forests. We shall, therefore, witness increasing use of these species for cellulose and paper and wood fiber products.

In chemical processes for the production of cellulose, roughly one third the weight of the wood goes into solution as that substance or mixture of substances known as lignin. There are few materials that have been subjected to more scientific investigation than lignin, and yet, at a recent meeting of lignin chemists in this country, the first and last questions posed to the assembly were concerned with the nature of lignin.

The difficulties attendant upon the characterization of lignin arise from the fact that, in the wood itself, it is probably combined with cellulose and the hemicellulose, and in the processes of isolation it undergoes deep-seated changes that vary quite broadly according to the methods used. Thus the term "lignin" actually has no specific reference apart from complete description of the methods used in its preparation. To make matters still more difficult, hardwood and softwood lignins differ, and it is quite probable that the ultimate composition of lignin may vary from species to species.

In spite of the problems posed by this infinity of variables, considerable progress is being made in the industrial utilization of those lignins that are the by-products of the acid and alkaline processes for the preparation of wood pulp. The alkaline industries generally burn the evaporated lignin containing liquors for power in the plant, but, increasingly, the lignin is being precipitated and recovered by various means for a wide variety of uses as crude lignin.

The sulfite pulping industry is faced with another by-product of the industrialization of the world, namely, stream pollution. Difficulties in evaporating and consuming its waste liquors have caused it to be attacked as a major contributor to this grave problem. The pollution with industrial wastes of the water supplies of industrial communities cannot much longer be tolerated in a civilized world. Large quantities of used industrial water must not be discharged in such a manner as to endanger the lives and health of people. Because of the growing importance of limited water supplies, rapid progress is being made in processes for the checking of stream pollution from sulfite pulp mills. It is clear that profitable utilization of the lignin component of wood is the key to preventing stream pollution from this source.

In acid processes for the pulping of wood, notably the sulfite process, considerable amounts of sugars are liberated and are present in the waste liquors discharged. In those countries not blessed with the abundance of foodstuffs available in this country, these sugars are thoroughly utilized, usually by fermentation. In Sweden, for example, the fermentation of these sugars to alcohol or other products is required. Large quantities of yeast for human or animal food are prepared from the same raw material.

In North America, two sulfite pulp mills now convert the hexose content of their waste liquors to ethyl alcohol, and a third plant is under construction. Pilot-plant operations on the production of feeding yeast from the complete sugar content of the liquors have been successfully completed, and it is expected that commercial production of sulfite liquor yeast will soon be established.

Important as are these developments in the pulp industry, their ultimate magnitude must be strictly limited by the size of the sulfite pulp industry. This in turn is distinctly limited in the species of wood that it can use, and it can reach only the limit set for it by the capacity of forest lands to produce those species.

FUTURE DEVELOPMENT OF SUPPLIES

From the considerable number of industries based upon wood that have already been described as part of the industrial world, it would appear possible to utilize rather completely the products of the forest. In some parts of the world such utilization is possible and practiced, but for the most part the forest industries have been and remain prodigal in their consumption of the forest crop by one-product industries, highly selective of the species and qualities of wood manufactured. Yet in recent years it has become increasingly apparent that only the highest possible degree of integration of wood-using industries can bring about the necessary balanced utilization of the forest crop required for the perpetuation of the crop itself. The forest is unique among all other crops of the soil in this respect: The method of the harvest determines to a large degree the nature and value of the succeeding tree crop. It is for this reason that wood industries, widely differentiated so as to use the forest crop completely, and integrated with the capacity of tributary lands to produce wood, have become considered necessities for the future development of supplies of wood adequate to our need. The principal lack in the ability of the industrial world to establish such completely integrated industrial organizations has been the absence of a type of wood-using industry capable of taking a wide range of species and qualities of wood and economically manufacturing from this raw material goods required by the modern world in mass quantities.

When it is considered that the sawing of wood into lumber brings only roughly one third of the total yield of the forest to market in the form of usable material and that a similar proportion holds true in the field of pulp and paper products, the significance of such an industry capable of profitably utilizing the other two thirds of the wood crop becomes apparent. As indicated before, there may be situations, especially in tropical countries, where the carbonization of wood and the consumption of the carbon products as such or for conversion to liquid fuels may offer a solution to this problem. The simple consideration of the fact that a large part of the fuel value of wood resides in the lignin, and that all carbohydrate content of the wood is destroyed in the process of carbonization, leads to the idea that the recovery of the carbohydrates in the form of simple sugars prior to carbonization of the lignin content might be a better solution to the problem posed than total carbonization.

Considerable progress has been made in this field along the line of acid hydrolysis of the carbohydrate content of wood. Although it has been more than 125 years since it was first ascertained that cellulose could be broken down by acids to glucose molecules, it has only been in the twentieth century that serious attempts to base industries upon this simple fact have begun to show some promise of success. A wide variety of hydrolytic agents has been employed, ranging from quite dilute mineral acids to extremely concentrated mineral acids; a considerable variation of temperature and pressure has also been employed. Suffice it to say that the technological means for the conversion of the carbohydrate content of wood to simple sugars are available and that by known processes approximately half the weight of the raw wood can be obtained in the form of simple sugars. In the case of coniferous woods, approximately 80 percent of these sugars is glucose and the remainder a mixture of pentoses, mostly xylose. In the case of the hardwoods, the proportion of pentoses is increased to around 35 percent. No great difficulties are encountered in the use of the crude sugar solution obtained by these processes of hydrolysis as the base for various fermentation industries with the production of industrial chemicals as the aim. Under current conditions no difficulty should be experienced in maintaining profitable production of such materials in competition with other sources of carbohydrates or even with petroleum for industrial chemicals. There are many serious considerations, however, that should affect our thought on the use of wood as a source of carbohydrates, other than the mere production of industrial chemicals.

It has been remarked earlier that one of the accompanying features of the rise of industrialization has been a tremendous increase in the population of the world. In a little over a century the world's population has increased by approximately two and one-half times. Fairfield Osborn in his striking book, *Our Plundered Planet,* estimates that there are not more than 4 billion acres of arable land left to fill the needs of 2 billion people, or less than 2 acres per capita. A generally accepted computation has it that the products from 2.5 acres of land of average quality are required to provide even a minimum, adequate diet for each person. These figures indicate the heavy impact of population on the agricultural resources of the earth when those resources are measured in terms of traditional methods for the production of food. Further industrialization of agriculture and the possible exploitation of new lands can only delay a little the application of the ironclad rule of Dr. Malthus. Tropical regions cannot be depended upon for agricultural productivity, and even if they could would only provide a temporary stopgap. Furthermore, there can be no political stability in the world if the basic needs of the people for subsistence are not satisfied. The most important things in the modern world, whether they be weapons of war or peace, are food and machinery. Both are today products of an industrial world, but it would be well to examine especially the means by which the industrial facilities of which we are possessed can be better turned to the satisfaction of the world's needs for food.

If we consider the unused wood crop in the United States alone, it amounts to something in the neighborhood of 150 million tons a year. This is a very conservative figure and has little relationship to the amount of wood that could be grown on the 630 million acres of forest land available in the United States if this land were properly managed. There are no really reliable figures on the amount of the wood crop that could be produced in the world under even simple forms of forest management. Some measure of the potentialities may be obtained from the estimate recently made to the effect that the energy stored by photosynthesis in a year is approximately 100 times all the coal mined per year, or 10,000 times the energy at present being obtained from water power. What proportion of this total is stored in the form of wood cannot be estimated, but it certainly is large.

The question that needs to be solved is the turning of this vast quantity of renewable carbohydrate material, at least in part, to the task of feeding the world's population. It has already been stated that methods for the conversion of the carbohydrate content of wood to simple sugars are available. By the addition of nitrogen, extracted from the air by known industrial methods, and small amounts of other nutrient materials amply available from industrial processes, the conversion of the crude sugars, produced by the hydrolysis of wood, to protein is easily effected. Roughly, a ton of wood can be converted into a quarter ton of yeast with a protein content of approximately 50 percent. When it is considered that only a few of the many species of microorganisms have ever been grown for such purposes, the opportunities for further profitable investigation in this field become striking. We have here the possibility not only of the production of vast quantities of edible carbohydrate material from wood, but also their transformation into nutritive protein, which is perhaps the greatest single lack in the diet of most of the world's teeming population. It has been estimated that if it became necessary the total sugar requirements for the population of the United States could be currently obtained from the sawmill waste now being produced in the states of Oregon and Washington.

When one considers the availability of carbohydrate materials from wood in the tropics, the potentialities of this raw resource in the production of foodstuffs are indeed staggering. The forests of the tropics do not apparently offer unlimited wood to replace or supplement the traditional uses to which wood in the Northern Hemisphere has been put. But there is a vast and unknown storehouse of carbohydrate material awaiting development.

The successful, complete wood chemical industry will not be a unit in itself. It will be geared to the forest and to such other wood-utilizing industries as may be found suitable to the kinds and qualities of wood that the forest grows. There are three characteristics that distinguish wood as a raw material for chemical industries:

1. It is a perpetually renewable resource of the same kind and in the same place.
2. Wood for chemical industries can be a by-product of forestry of which the principal product may be lumber or similar material.
3. The methods used in harvesting the wood crop determine the nature of future crops.

The industrial plants that utilize the product of the forest determine the harvesting methods.

Diversified utilization of the forest crop can be expected to encourage intensive management of the forest. As long as the sun shines and the rains fall there need be no shortage of wood in the world if we manage the forest well and learn to use the tools of industry, products of our industrial civilization, for the more complete employment of wood in meeting human needs. And, above all, the land that grows the forest is land that usually cannot be put to other profitable use. The forest resource is unique in that its proper harvesting is the best means possible of ensuring a permanent forest resource.

◆

DEFINITIONS AND PRINCIPLES OF DYNAMICS*

JOSEPH H. KEENAN

Professor Keenan has been professor of mechanical engineering at M I T since 1934. He is chairman of the Subcommittee on Propulsion Systems, NACA, and a consultant in heat engineering and fluid mechanics, including jet propulsion for aircraft. He has written, either alone or in collaboration with others, a number of books on thermodynamics.

IN A recent paper[1] the development of the exposition of thermodynamics was traced from its beginnings to the present. It was pointed out there that more than half a century elapsed between the time the first law of thermodynamics was understood sufficiently well for effective application by savants like Kelvin and Clausius and the time when an adequate exposition was first published. It was found that the causes of this long delay included the following: first, a vestige of the caloric theory to the effect that heat resides in a body and is a state property: second, the persistence of the idea that nothing is understood unless it can be explained in terms of Newtonian mechanics.

This inquiry into the foundations of thermodynamics brought into focus two points—first, that the exposition hinged on definitions of a few quantities, the definitions consisting of rules for getting numbers to represent the magnitudes of quantities designated by certain names; second, that these quantities antedated in the minds of men the new science of thermodynamics.

Let us review briefly the exposition of the first law of thermodynamics, following the method of Henri Poincaré, the first adequate method. The first law is the statement that work and heat are proportional to each other in a cyclic process. It remains to define the terms work, heat, and cyclic process. Beginning with the last we say that a cyclic process is any process in a prescribed system (or collection of matter) for which all observable characteristics of the system are the same initially and finally. Work and heat are both quantitative effects of one system on another. We shall discuss each separately.

Work flows from system A and to its environment during a given process in A if A could pass through the same process while the sole effect external to A was the rise of a weight. The number which represents the magnitude of the work is found by counting the number of standard weights which can be raised by A from one selected level to another at a given location on the surface of the earth. The amount of work flowing from system A is identical with the amount flowing to its environment. The implication here that work is always measured in terms of flow *from* a system is a necessary one.

The definition of heat involves a preliminary definition of temperature which in turn requires a previous definition of equality of temperature. Any pair of systems, in the absence of any extraordinary barrier between them, will in general affect each other even in the absence of work. The corresponding changes in two systems that are exposed only to each other will gradually decrease in rate. The condition which the pair approaches as a limit is called equality of temperature.

Associated with this definition is an axiom, sometimes called the zeroth law of thermodynamics, to the effect that two systems that are each equal in temperature to a third system are equal in temperature to each other.

A temperature scale may be defined in terms of certain reproducible levels of temperature and the magnitude of a selected property of a prescribed system under prescribed restraints as to pressure, volume, etc.

The definition of equality of temperature implies the existence of inequality of temperature, for which the two systems in communication have an effect on each other. This effect is called heat. The number which represents the magnitude of the heat for a process of system A is found by counting the number of standard systems in communication with A which will go from one prescribed level of temperature to another as a result of the heat effect in question. For the purpose of this test system, A must exchange heat during the process only with the standard systems which are subject to prescribed restraints as to pressure, volume, etc.

These rules illustrate the two points mentioned above. First, they are rather elaborate rules for counting to get numbers to represent the magnitudes of work and heat. Second, they are new rules for old quantities. Work is a quantity appearing in the early discussions of mechanical equilibrium or statics. In a displacement of a balanced lever the work quantities at the two ends were known to be equal in magnitude and opposite in sign. Work had previously been associated with a force. Here it is an interaction between systems. Heat was variously considered as an interaction between bodies and as something stored in bodies. Calorimetry, or the measurement of heat, was not new but had not previously been strictly defined.

---

* From a lecture presented by Professor Keenan at Purdue University, April 27, 1948.

The new principle—namely, the first law—is stated in terms of these old concepts redefined.

Upon closer examination of these definitions three additional points become clear: first, the new principle results in more precise definition of the old quantities; second, these precise definitions, or rules of counting, are strangely arbitrary in nature; and, third, each involves "extrapolated concepts" or "extrapolated devices."

The first of these points has already been commented upon. The second, relating to arbitrariness, becomes evident upon considering the rule about measuring work flowing from a system in order to measure work flowing to another, or the rule about heat effects in an interaction being equal and opposite. These definitions are *ad hoc* devices tailored to the purpose of stating the principle at hand.

As regards extrapolated concepts, we find them implied throughout these definitions. When a gas expands behind a piston the effects outside the gas will include, besides the rise of a weight, the warming of bearing surfaces between piston and cylinder and between links connecting piston and weight. By repeating the experiment with greater and greater refinement of bearings so that less and less material is warmed, a condition is approached, though never attained, for which the sole effect is the rise of a weight. The work for this limiting condition can be obtained only by extrapolation.

As another example, consider the condition in equality of temperature requiring that the two bodies should be isolated from everything but each other. This condition can be attained only by means of an indefinitely thick wall of inert material as the immediate environment of the two bodies. Equality of temperature can thus be found only by an extrapolation of observations to an unattainable condition.

This concept of extrapolated values implies two things: first, that the only limit to the nearness with which we may approach the limiting condition is one imposed by our own limitations as to time, effort, or diligence; second, that results obtained with different materials or devices, within the prescriptions of the definitions, will converge upon each other as the limiting condition is approached.

This approach to the subject of thermodynamics has justified itself in that it has made the subject intelligible to men of ordinary intellect. Whereas before only the savant could safely find his way through the obscurities, confusions, and apparent contradictions of the subject, now many ordinary minds can have the satisfaction of solving problems not solved in the textbooks. Indeed, this success has been so notable that one is impelled to inquire whether similar methods have been applied to the exposition of the sciences of statics, dynamics, electromagnetism, etc.

On this occasion it is proposed to make this inquiry as regards dynamics. It is doubtless a foolhardy thing to do. To venture with critical eye and pen out of one's own specialty into someone else's is a tried and true method of losing friends and alienating people. The present justification for proceeding along this perilous path is the hope that someone better qualified will be inspired, if only through resentment, to clarify this subject.

The subject of dynamics has been critically examined on several previous occasions by scientists of uncommon skill. A reasonably extensive sampling of the literature seems to indicate that a statement of the principles of dynamics of a clarity comparable to Poincaré's statement of the first law of thermodynamics has not yet appeared. Two faults seem common to all published statements: first, that the necessary definitions are not refinements of previously recognized quantities; second, that the definitions do not include practical counting procedures, which through extrapolation will yield the desired numbers.

Just as the exposition of thermodynamics was long hampered by following too slavishly the words of the great pioneers, so too the exposition of dynamics seems to have suffered. Whereas in thermodynamics we had to overcome the handicap of Helmholtz' mechanical theory of heat, in dynamics we seem to suffer from Newton's preoccupation with astronomy. A reading of several eminent authors in this field is likely to leave one with the impression that these men had lived their lives floating about in space—*extra*stellar space—in which they could perform at will experiments quite beyond the capacity of earthly mortals.

Let us first note that the extrapolated concept must enter into each definition in dynamics. As an example, consider the quantity length, which must certainly be defined. We shall not complicate the matter by considering relative velocities great enough to raise questions of the meaning of simultaneity, but shall limit ourselves to a prerelativity concept of length. We may prescribe the distance between two marks on a particular bar of metal held at a particular temperature as the unit of length. When the bar is moved in the direction of its length to a position where the first mark is where the second was previously, then we have added a second unit of length to the first to obtain two units of length. The question arises whether the orientation of the bar is of any consequence. The answer seems to be in the affirmative; that is, two bars which give the same number for a horizontal distance may give slightly different numbers for a vertical distance.

Upon investigation it is found that bars which for a given weight have great resistance to deformation will show better agreement in measurement of vertical distances than bars having small resistance to deformation. Here we must extrapolate the result to that for a measuring bar of infinite rigidity per unit weight.

To avoid this difficulty, as well as some others, it is now the practice to use the wave length of a certain part of the electromagnetic spectrum as the basic distance in terms of which the unit of length is defined. Nevertheless, extrapolation of the result of counting to idealized conditions is still necessary.

Newton, like Kelvin and Clausius in thermodynamics, had an intuitive grasp of the significance of his discoveries. He did not present them in terms adequate to the understanding of the common mind, nor even with any logical completeness. He defines several different kinds of force, one of which, "an impressed force, is any action upon a body which changes or tends to change, its state of rest, or of uniform motion in a straight line." When this is compared with his first law, namely, "Every body perseveres in its state of rest or of uniform motion in a straight line, except in so far as it is compelled to change that state by impressed forces," definition and law become indistinguishable.

Ernst Mach in 1883[2] reviewed Newton's exposition critically and came to the conclusion that "In reality only *one* great fact was established [by Newton and his scientific forbears Galileo and Huygens]. Different pairs of bodies determine independently of each other, and mutually, in themselves, pairs of accelerations, whose terms exhibit a constant ratio, the criterion and characteristic *of the pair.*" Although a good argument can be made that Newton's principles embrace more than this, it is significant that a careful student of the subject could not convince himself from Newton's exposition that Newton referred to anything more than a ratio of a pair of accelerations for a pair of bodies.

Let us see what Mach proposed in place of the exposition of Newton.

> It is possible to replace Newton's enunciation by much more simple, methodically better arranged, and more satisfactory propositions. Such, in our estimation, would be the following:
>
> a. *Experimental Proposition.* Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary *accelerations* in the direction of their line of junction.
>
> b. *Definition.* The mass-ratio of any two bodies is the negative inverse ratio of the mutually induced accelerations of those bodies.
>
> c. *Experimental Proposition.* The mass ratios of bodies are independent of the character of the physical states (of the bodies) that condition the mutual accelerations produced, be those states electrical, magnetic, or what not; and they remain, moreover, the same whether they are mediately or immediately arrived at.
>
> d. *Experimental Proposition.* The accelerations which any number of bodies A, B, C . . . induce in a body K, are independent of each other. (The principle of the parallelogram of forces follows immediately from this.)
>
> e. *Definition.* Moving force is the product of the mass value of the body into the acceleration induced in that body.

Of these, the Experimental Propositions *c* and *d* cannot well be objected to. Proposition *c* states that the mass ratio of two bodies is independent of the physical states of the bodies. Proposition *d* states the independence of force effects from each other and indicates vector summation of forces and their effects.

Experimental Proposition *a,* on the other hand, has a disturbing vagueness. "Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary accelerations. . . ." Quite obviously the definition *b,* which is to the effect that the mass ratio of two bodies is given by the ratio of accelerations, is only valid for bodies which are acted on only by each other or for bodies constrained to move in a horizontal plane but otherwise acted upon only by each other. This latter is so special an interpretation as to seem not to be Mach's intention.

It is the misfortune, perhaps, of earthbound creatures that they can never experiment at will upon two bodies acted on only by each other. The dynamicist's lament might well be "The earth is too much with us; late and soon," which is only a slight variation on Wordsworth's. As a terrestrial proposition *a* is not experimental.

In thermodynamics we have learned to be terrestrial in our outlook, to the great advantage of the student. Gibbs opened his great paper "On the Equilibrium of Heterogeneous Substances" by quoting Clausius: "The energy of the Universe remains constant, the entropy increases toward a maximum." Some of his followers have stated that these were the principles from which Gibbs' argument proceeded. On the contrary, they played no part in his argument but served only to embellish his first page. Such cosmic principles are of no utility in laboratory or workshop. The universe cannot be the subject of controlled experiment.

A further objection to these statements of Mach is that neither mass nor force, as here defined, is a concept known aside from these definitions. This despite the fact that both mass and force were well-recognized concepts before the advent of dynamics. Mass was a concept of the market place; force was a concept of the workshop and of the science of statics.

Max Planck, writing in 1916,[3] expounded the fundamentals of dynamics by steps which may be roughly outlined as follows:

> "A material point which is deprived of all causes of motion moves uniformly and rectilinearly." This may be illustrated, but not proved, by the motion of a body on a smooth horizontal surface.
>
> Muscular effort is required to accelerate a body. For a given body the effort increases with the acceleration. Therefore, we partially define a force by saying that it is something acting on a body which is proportional to the acceleration of that body.
>
> But some bodies require more muscular effort than others for the same acceleration. Therefore, the proportionality constant between force and acceleration must depend upon some characteristic of the body, which we shall call mass.
>
> Up to this point it should be noted that neither force nor mass is completely defined.
>
> It is known from experiment that all bodies have the same acceleration when falling freely. Therefore the force causing the fall, or gravity, is proportional to the mass.
>
> Now, if equal masses are attached to the two ends of a string which hangs over a pulley, the forces on the two ends will be equal and the string will not move from rest. Thus we have a means of measuring masses.
>
> Force now becomes a derived quantity, namely, the product of mass and acceleration.

Although this presentation seems more realistic than that of Mach, it leaves one unsatisfied in several respects.

First, the reasoning is involuted. An inadequately defined force is proportional to an inadequately defined mass for fixed acceleration.

Second, the concept of force is not identified with the concept of force in statics. How can we be sure that equal forces of the kind in question will not rotate the pulley? Is not the pulley device an arbitrarily selected means of measuring mass, rather than a means derived from the previous considerations? By inference we may find an association of the two concepts, static force and dynamic force, through reference to muscular sensation—a reference not made by Mach.

Both Mach and Planck define force as if it were a concept which derives from the dynamic principle, although Planck does attach to it a qualitative "muscular" significance as well. In view of these definitions, it is fair to conclude that force is another name for the product of mass and acceleration. All forces in statics are omitted from this definition. It becomes logical to assume that to support a body at rest involves no force because it involves no acceleration. Perhaps the forces on the string over the pulley in Planck's measurement of mass vanish when the bodies are brought to rest. Here is no recognition of the back-breaking toil of generations of laborers who had to "lift dat load and tote dat bale." Lifting and toting was the serious work of man—and the worker soon learned to leave acceleration to those foolish enough to waste their substance on athletics.

It might be argued that the definition in terms of mass and acceleration will do if forces can be applied one at a time, so that counteracting forces may be recognized. But this argument seems meaningless in the absence of further means of recognizing forces. Force is evidenced by acceleration and alters along with acceleration. When acceleration vanishes, force vanishes; whether from cancellation of one force by another or by the vanishing of all forces, we cannot determine.

A. G. Webster, in his *Dynamics of Particles* (1912),[4] also defines force as the product of mass and acceleration. He seems to try to break through the difficulty just discussed.

> The introduction of the term force has given us no explanation of the cause of motion. . . . It is undoubtedly true that our fundamental notions of dynamics are derived through what may be called the muscular sense. . . . Suppose that we find that under given conditions a certain agency will produce a certain force, as shown by the motion of some body, and suppose that as the circumstances are changed we can always measure the force. If then it is possible to submit a second body to the action of the same agent under similarly varying circumstances, we shall be able to find the motion of the second body. . . . Under certain circumstances, an agent which would under other conditions cause motion, may cause no motion. We then say that its effect is counteracted by that of some other agent, or otherwise, that the two forces are in equilibrium.

Here the implication is that the existence of a force may be recognized in terms of the agent that provides it, although the implication is vague.

The phrase "to submit a second body to the action of the same agent under similarly varying circumstances" suggests that identical forces may be recognized aside from their accelerating effects, although the means of recognition are not clear.

Planck and Webster both imply that force is a concept in its own right, that it derives from muscular effort, that it may be recognized in terms of the agent that provides it. Webster says it is an interaction between bodies, and Mach states that such interaction exists but only indirectly associates it with the term force. Nevertheless, no one of these authorities suggests that a measure of force can be found other than the product of the acceleration it produces and the mass it acts upon. Force, in the last analysis, is nothing but the product of mass and acceleration, and the principle of dynamics becomes, in part at least, a definition of force.

It seems evident that a principle stated solely in terms of previously defined quantities is simpler than one combined with a definition of a new concept or a redefinition of an old one. In accordance with this view, we state the first law of thermodynamics as *the proportionality of the quantities, work and heat,* as previously defined, *in a cyclic process.* Subsequently, we may derive a unit of work or of heat by making the constant of proportionality unity. But to state this law in the form *heat and work are equal in a cyclic process* without first having defined heat, for example, would be to obscure the principle quite hopelessly. If we were to combine such a statement with some vague remarks concerning the physical sensations of warmth associated with heat we should leave our principle about where the principle of dynamics seems now to be left.

Since Poincaré resolved many of our difficulties in the exposition of thermodynamics, it may prove profitable to read what he has to say about the exposition of dynamics. The following is from his *Science and Hypothesis* of 1903, as translated by Halsted.[5]

> The Principle of Inertia—A body acted on by no force can only move uniformly in a straight line. . . .
>
> Is the principle of inertia, which is not an *a priori* truth, therefore an experimental fact? But has anyone ever experimented on bodies withdrawn from the action of every force? And, if so, how was it known that these bodies were subjected to no force?
>
> The Law of Acceleration—The acceleration of a body is equal to the force acting on it divided by its mass. Can this law be verified by experiment? For that it would be necessary to measure the three magnitudes which figure in the nunciation [sic]: acceleration, force, and mass.
>
> I assume that acceleration can be measured, for I pass over the difficulty arising from the measurement of time. But how measure force, or mass? We do not know even what they are.
>
> What is *mass?* According to Newton it is the product of the volume by the density . . . it would be better to say that density is the quotient of the mass by the volume. What is *force?* It is, replies LaGrange, that which moves or tends to move a body. It is, Kirchhoff will say, the product of the mass by the *acceleration.* But then, why not say that the mass is the quotient of the force by the acceleration?
>
> These difficulties are inextricable.
>
> When we say force is the cause of motion, we talk metaphysics, and this definition, if one were content with it, would be absolutely sterile. For a definition to be of any use, it must teach us to *measure* force; moreover that suffices; it is not at all necessary that it teach us what force is *in itself,* nor whether it is the cause or the effect of motion.
>
> We must therefore define the equality of two forces. When shall we say two forces are equal? It is, we are told, when applied to the same mass, they impress upon it the same acceleration, or when, opposed directly one to the other, they produce equilibrium. This definition is only a sham. A force applied to a body cannot be uncoupled to hook it up to another body, as one uncouples a locomotive to attach it to another train. It is therefore impossible to know what acceleration such a force, applied to such a body, would impress upon such another body if it were applied to it. It is impossible to know how two forces which are not directly opposed would act, if they were directly opposed.

In the interest of brevity, the remainder of Poincaré's discussion will be summarized. He attempts to use the downward pull of an arbitrarily selected heavy body for identifying vertical forces, but concludes that this pull is variable with position and that therefore mathematical rigor is lacking. He rejects the resiliency of a dynamometer as a measure of force because "temperature and a multitude of circumstances" may cause it to vary. He points out that "We are . . . obliged in the definition of equality of forces to bring in the principle of equality of action and reaction; on this account, this principle must no longer be regarded as an experimental law, but as a definition."

He discusses the consequences of accepting what he calls Kirchhoff's definition: "force is equal to the mass multiplied by the acceleration." He points out that this, with the principle of action and reaction, results in determining the ratio of a pair of masses by the ratio of their accelerations *when they act upon each other only,* an impossible condition.

He speculates as to using the law of universal gravitation, but seems to reject it, because he questions the right to admit the hypothesis of central forces. He says finally: "Therefore nothing remains and our efforts have been fruitless; we are driven to the following definition, which is an avowal of powerlessness: *masses are coefficients it is convenient to introduce into calculations.*"

He raises the question whether the principles of dynamics are no more than definition and, in trying to show that they are more than this, says:

> There is not in nature any system *perfectly* isolated, perfectly removed from all external action; but there are systems almost isolated. . . . We ascertain then that the motion of this center of gravity is *almost* rectilinear and uniform, in conformity with Newton's third law.
>
> That is an experimental truth, but it cannot be invalidated by experience . . . a more precise experiment . . . would teach us that the law was only almost true.
>
> *We can now understand how experience has been able to serve as basis for the principles of mechanics and yet will never be able to contradict them.*

It seems that Poincaré leaves us on the horns of the dilemma. Nevertheless, he has stated with great clarity the problems we must face. These are, in brief, as follows:

> No one has ever been able to experiment on a body withdrawn from the action of any force. His later contention that there are systems *almost* isolated must refer to an astronomical mechanics rather than a terrestrial one.
>
> "A force applied to a body cannot be uncoupled to hook it up to another body, as one uncouples a locomotive to attach it to another train." Identical or equal and opposite forces can therefore not be determined.
>
> "For a definition to be of any use it must teach us to measure force; moreover that suffices. . . ."
>
> "Masses are coefficients it is convenient to introduce into calculations."

From the standpoint of terrestrial mechanics, however, one need not despair of resolving these difficulties. One need not despair of devising a definition for mass that will teach us to measure mass, and a definition for force that will teach us to measure force and to uncouple a force and to hook it up to another body. Despite the melancholy implications of the limerick

An intrepid young student named Raleigh
Deserted a fast-moving traleigh
To discover a flaw
In Newton's first law,
But his efforts were faleigh, by galeigh,

it is proposed here to embark on an attempt to state a set of definitions and principles fundamental to the science of mechanics in general and of dynamics in particular. The result can hardly be definitive, though it may prove provocative.

The definitions will be primarily rules for counting to obtain numbers which will represent the magnitudes of what we call length, time, mass, and force. These might be called the primary physical quantities. Regarding these A. H. Shapiro has made the following useful comments, which are quoted with his permission:

> Primary physical qualities have no "inherent," "intuitive," or "absolute" significance. They are defined arbitrarily, and their definitions consist of sets of specifications for assigning numbers in experiments where the sole ingredients are certain arbitrarily-selected bodies and our own physical senses.
>
> The definitions of primary physical quantities are, for practical reasons, heuristic. The aim is to discover relations or laws between various primary and secondary quantities so that natural events may be described and, ultimately, predicted. The usefulness of the primary quantities, as defined, is judged by (*i*) whether such laws may be found at all, (*ii*) whether the laws as so found are simple to apply, (*iii*) whether the method of assigning numbers to the primary quantities is reasonably simple and essentially free of ambiguity.
>
> To make the definitions of primary quantities unambiguous, the specifications for assigning numbers become complex. In all practical cases, this requires extrapolations (with the implied notion of convergence) for getting numbers accurately.

The quantity length was discussed above in sufficient detail to show that it is manageable, though troublesome. Equally troublesome is the quantity time.

Newton implied that we had to make do with an astronomical time, although he looked wistfully toward an absolute time, otherwise called duration, the flow of which cannot be changed.

Various authorities state that the basic unit of time in mechanics is the "mean solar day." The adjective "mean," however, suggests that solar days are of varying duration, which in turn suggests some measure of time independent of the solar day. This seems to be the sidereal day. Smaller units of time, like the hour, minute, and second, are subdivisions of the mean solar day, which are arrived at by means of clocks. A clock consists of some mechanism which regularly repeats some operation such as the oscillation of a pendulum. Presumably, the acceptability of clocks depends largely on agreement between them as to the subdivision of the mean solar day into fractional units such as the second and the hour. The assumption is implicit in this definition of time that the time elapsed between two events is independent of the position or motion of the observer. A science based on such an assumption can be no more valid, of course, than the assumption itself.

The rule for measuring mass is that the body supported in one pan of an equal-arm balance, which is balanced and free, is equal in mass to that supported in the other. The equal-arm balance must, of course, balance again when the bodies at either end are interchanged. An arbitrary collection of matter may be considered the unit of mass. That and the material which balances it constitute together two units of mass, and so on. Two equal masses which together balance the unit of mass are each one-half unit of mass, and so on.

This is, in fact, Planck's method of measuring mass written into a definition. The involution of argument employed by Planck is avoided by this arbitrary step.

Force is an interaction between two bodies. (Heat is also an interaction between two bodies, the rules of measurement for which are different from those for force.) When the interaction occurs at the surfaces of the bodies, as when it results from contact, then distortion of the bodies occurs. The amount of the distortion may be different for two different bodies in contact. The body which is distorted less is said to be the more *rigid*. The concept of perfect rigidity, or no distortion under force, is an extrapolated concept.

Some bodies have the characteristic of complete recovery from distortion when the interaction ceases. These are called *elastic* bodies. The distortion of elastic bodies may be used to measure force.

A long elastic body on a horizontal surface has the same length regardless of its orientation, provided that it is not in contact with, or too near, any body other than the surface. As its position is altered from the horizontal some change in length generally occurs, although the amount of this change differs from one body to another. The greater the mass of the body for a given rigidity, the greater will be this effect of departure from the horizontal. For the purpose of measuring forces it would be desirable, although not necessary, to have an elastic body of negligible mass for a given finite rigidity.

For measuring forces in a horizontal plane, one may select a single long, slender elastic body arranged with "grips" at the ends which permit the attachment of strings. A unit of force may be said to act on each end of this body directed away from the body and along the line defined by the stretched string when the body is simply extended by a prescribed amount which is within the range of elastic behavior of the body. The temperature of the body must be prescribed; for example, it may be required that the body be surrounded by an ice bath. The extrapolated nature of the measurement of force becomes evident when one considers that absolute smoothness at the points of contact between the body and the supporting horizontal surface is necessary.

If this standard body is duplicated as nearly as possible, and the two standard bodies are caused to interact by tieing them end to end and pulling them apart, then the extensions of the two bodies will be equal. This leads to the principle, which is equivalent to Newton's third law, that *when one body exerts a force on a second body, the second body exerts an equal and opposite force on the first.* This may be taken to be a generalization of a simple experience to include all cases of force interaction in all directions and for all conditions of motion. If we were to accept this principle beforehand it would be unnecessary to duplicate the original standard body in all respects, because the distortion of any other body under unit force could be found by interaction of the other body with the single standard body.

It seems necessary at this point to introduce another principle which is equivalent to Newton's first law, to the effect that *the summation of the forces on a body in every direction is zero if the body has no acceleration.* This might be called the principle of equality of forces. It permits us to use intermediate bodies for transmitting known forces. Two standard bodies may be yoked together in parallel and the yoke tied to a body to which two units of force are to be applied. If the yoke is not accelerating, then two units of force must be applied at each end of the yoke in opposite directions, and by Newton's third law the two units of force from the standard bodies are transmitted to the body which is to be acted upon. Also, by means of intermediate yokes, the pull of standard bodies can be translated into a push on other bodies.

In order to measure vertical forces, in the absence of standard bodies of finite rigidity and zero mass, it is necessary to be quite arbitrary in definition. We shall say that *the tension in a massless string extending over a perfect pulley is the same at both ends.* To use this axiom requires extrapolation to zero mass of the string (a simpler problem than extrapolation to zero mass of our standard body) and to the perfect pulley. The perfect pulley has zero diameter at its bearing journals and it must be balanced. That is, it must rotate slowly upon infinitesimal disturbance, but it must never rotate spontaneously whatever the position of the wheel.

The justification of this axiom for forces in the horizontal plane only can be readily made by measuring the string tension at both ends. A partial justification for forces in vertical and horizontal planes can be made by using vertical measuring bodies of various mass-rigidity ratios. As the mass approaches zero for a given rigidity, the deflections of duplicate bodies at the two ends of the string will approach identity. Nevertheless, the axiom concerning equality of tension must remain fundamentally a rule for measuring vertical forces and, as such, a part of the definition of force.[†]

From our definition of force and the principle of equality of forces, it follows that all bodies are acted upon by a vertically downward force called gravity. This is evident from the fact that in order to prevent acceleration of any body it is necessary to provide a vertically upward force on it. This force—which may be measured—is, by the principle of equality of forces, equal in magnitude and opposite in direction to the force of gravity. The gravity force is obviously not the result of interaction by contact between the body in question and some other body, as the measured supporting force is. Gravity is thus an action at a distance. Such forces are usually measured indirectly by providing a counteracting measurable contact force which results in zero acceleration.

Thus far we have defined stationary force; that is, force which is applied to a body which is not accelerating. This definition appears to be necessary to an exposition of the science of statics; but in this field, as in dynamics, the early investigators did not adequately define their terms. Some of the concepts expressly stated in the definition given above were implied in early discussions of statics. For example, the principle of equality of tension in a string stretched over a perfect pulley was implied in the work of Stevinus approximately a century before the enunciation of Newton's laws. Force was apparently measured by weights—that is, by the number of units of mass that would be supported against gravity. Newton was aware that two forces that appeared to be equal by this method might in fact have very different effects if one were measured on a high mountain and the other at sea level. The deformable body, as a force standard, is free from this defect.

Poincaré objected to the dynamometer, or the elastically distorted body, as a means of measuring force because it would be affected by "temperature and a multitude of circumstances." The effect of temperature can be readily controlled. The effect of orientation can be avoided, as indicated above. The effects of the other multitude of circumstances seem to be of less consequence. For example, the pressure of the surrounding atmosphere may be controlled, although its effect within terrestrial variations is quite negligible. It seems probable that the remainder of the multitude of circumstances referred to by Poincaré would equally affect the prototype of the unit of mass which seems to be entirely acceptable.

In dynamics forces must be applied to bodies that are accelerating as well as to those that are stationary. Since a force measured by distortion of a standard body which is accelerating may not (and indeed will not) have the same effects as one measured by an equal distortion of the standard body when stationary, it is necessary to devise a means of measuring moving forces in terms of stationary standard bodies.

Suppose that a string which applies a force to a moving body extends over a perfect pulley so that the two ends of the string are at right angles to each other. If the pulley bearings are held fixed by means of two stationary forces in the directions of the two ends of the string, then each stationary restraining force is equal to its opposite moving force. This procedure seems to be justifiable by the principle of equality of forces and Newton's third law in combination. If it were not so, then this procedure would become part of the definition of a moving force.

We are now prepared for the statement of Newton's second law in terms of the measurable quantities length, time, mass, and force. It may be stated as follows: *An elementary body is accelerated in the direction of the resultant force on the body, and the product of the mass of the body and its acceleration is proportional to the resultant force.* The resultant force is the vector sum of all forces. An immediate corollary is that the effect of each force acting on a body is independent of the other forces acting on the same body—that is, that a force in the X direction results in the same acceleration in the X direction whether or not a force acts in the Y direction.

It appears that the definition of force is not simple and not easily devised. Perhaps the one who rashly treads this ground exhibits something less than the wisdom of angels. On the other hand, the definition of heat—which proceeds through equality of temperature and temperature change in arbitrarily restrained standard bodies—is neither simple nor easy. Nevertheless, this definition has illuminated the science of thermodynamics.

It seems probable that the science of dynamics will be illuminated by a similar treatment, a similar insistence on the rules for counting to get the numbers which go into the equations. These procedures for counting are what distinguish the science of dynamics from the science of mathematics. It may safely be said that this distinction has not been evident to many students of dynamics.

The proposed procedure also emphasizes the arbitrary nature of the quantities in terms of which the science is expressed. It disperses much of the hoary atmosphere of the metaphysical that still clings to Newtonian mechanics, despite the limitations emphasized by the theory of relativity. It makes it seem as ridiculous, for instance, to try to explain thermodynamics in terms of dynamics as it would be to explain dynamics in terms of thermodynamics.

[†] The equal-arm balance for measuring mass is, in a sense, a special case of the perfect pulley.

Ernest L. Robinson once said that in his youth he felt that Newtonian mechanics was superfluous. He did not need to be told by Newton why an apple fell to the ground—it fell because the stem rotted. Newton's point of view was no more logical than Robinson's. It was justified only because it clarified relationships between phenomena in a way that Robinson's apparently did not.

And when the student has come to understand this heuristic quality of science, he can enter a new science without violent readjustment. Undergraduates who have studied the methods of counting peculiar to dynamics will doubtless find thermodynamics less strange when they come to it a year or so later. It is entirely possible that the theory of relativity will be to them just another quantitative science of no unusual difficulty.

Mechanics which is simply more mathematics—more equations, more calculus—is not true mechanics. Mechanics which requires the student to know the meaning of symbols, the significance of the relationships between them, the distinction between definition and principle, is an exercise in logic and inquiry. It is an invitation to the student to examine critically and freely, to probe deeply and skeptically, the subject he studies. Thus even mechanics can be made to contribute to the development of what may be the last great hope of mankind, the free spirit of inquiry.

REFERENCES

1. Keenan, J. H., and Shapiro, A. H. History and Exposition of the Laws of Thermodynamics. *Mech. Eng.*, 1947, **69**, 915–21.
2. Mach, E. (tr. by McCormack, T. J.). *The Science of Mechanics.* Chicago Open Court Publ. Co., 1893.
3. Planck, M. (tr. by Brose, H. L.). *Introduction to Theoretical Physics.* New York: Macmillan, 1933, Vol. 1.
4. Webster, A. G. *Dynamics of Particles.* Leipzig: B. G. Teubner, 1912.
5. Poincaré, H. (tr. by Halsted, G. B.). *The Foundations of Science.* Lancaster, Pa.: Science Press, 1946.

◆

THE GREAT EVENT

(ACCORDING TO WAVE MECHANICS)

Arrayed in dark and bounded space, the diamond-studded horseshoe tensely beams.
Drums roll. The baton of the Great Conductor, plummeting from high,
Sounds the mightiest of chords in an endless symphony—
A clash of symbols and out of cold and fury flashes a spinning fiery jewel,
A vibrato in the harmony of singing strings.

Now a hush—the woodwinds chant its lustrous beauty.
Then a rumbling bass—a string is plucked, another, then another
As her radiant arms drop dancing pearls in Kepler's orbs,
Happy silent images sparkling in the brilliant light,
A ballet of ruby, emerald, and gold.

G. W. Grimm

THE OUTLET OF LAKE BONNEVILLE*

RONALD L. IVES

Mr. Ives (M.S., University of Colorado) has long been interested in the geography of the West, about which he has published a number of articles. Now in the Department of Geography, Indiana University, he found time, after his war service, and before going to this post, to do extensive work in the Lake Bonneville region.

THE Pleistocene lakes of North America constitute one of the most fascinating and most challenging fields for research in modern geology, despite the extensive and competent pioneer work by such men as G. K. Gilbert and I. C. Russell. Perhaps the most familiar of these ancient bodies of water is Lake Bonneville, which once covered parts of Utah, Nevada, and Idaho to a maximum depth of more than 1,000 feet.

So extensive and obvious was the evidence for this lake that the first literate white man to enter the area, Fray Silvestre Velez de Escalante, wrote in his journal, on October 2, 1776, ". . . This place, which we named Llano Salado, because we found some thin white shells there, seems to have once had a much larger lake than the present one."[1,2] These "thin white shells," which may have been *Lymnaea bonnevillensis*, later helped Gilbert and others to reconstruct climatic conditions at the time of high lake levels.

About three quarters of a century later, Captain Howard Stansbury, in the course of his official duties, made a survey of the valley of Great Salt Lake, noting, during the progress of his work, the presence of a number of "benches" on the mountains. From these, which he correctly identified as ancient shore lines (Fig. 1), Stansbury arrived at a reasonable estimate of the dimensions of the former lake, description of which is given in his official report.[3]

Shortly thereafter, there came to the Great Basin region, as a member of an exploring party, a young geologist named Grove Karl Gilbert, a keen observer who devoted an appreciable part of his highly productive working life to the study of this ancestor of Great Salt Lake. His report, "Lake Bonneville,"[4] is not only a classic of geological exposition, but still constitutes the "bible" of all geologists working in the Great Basin region.

From the publication of Gilbert's report in 1890 until about 20 years ago, further progress in the study of Lake Bonneville awaited significant "advances in the art" of geology. Then studies of Pleistocene climates, by Blackwelder,[5] Antevs,[6] and others, showed that climatic changes like those which produced Lake Bonneville also took place in other parts of North America, and that perhaps a stage-for-stage correlation of Pleistocene lake levels with Pleistocene glacial advances could be made. Finding of evidence of ancient man in some sea caves along the abandoned shores of Lake Bonneville, by M. R. Harrington, of the Southwest Museum, and others, added impetus to this study (Fig. 2). Although much progress has been made in these correlations, their present status calls for continued application of the concept of multiple working hypotheses.

History and extent of Lake Bonneville, as reconstructed by Gilbert, are competently outlined in his monograph, with its accompanying maps (Map References), and are shown, in summary form on a modern base, in Figure 4. Modern work confirms all Gilbert's factual data, and most of his conclusions, but has so augmented the evidence that the following history now seems to merit consideration:

1. During middle or early Pleistocene the Great Basin was occupied by a series of playa lakes, their number, their extent, and their interrelations, if any, being still undetermined.

---

* During the course of this investigation, valuable assistance, ranging from trail information to help in "unsticking" jeeps, was received from many residents of isolated mining camps and ranches in the Bonneville area. This is here gratefully acknowledged, as is the hospitality so generously offered, during wartime food shortages, at many Utah, Idaho, and Nevada ranches.

Profiles of many parts of the area were furnished at various times by the U. S. Geological Survey; the U. S. Bureau of Public Roads; the highway departments of the states of Nevada, Utah, and Idaho; and the Union Pacific Railroad. Access to files of aerial photographs, made possible by the U. S. Forest Service, the U. S. Department of Agriculture, and the U. S. Army, saved many miles of travel and permitted double checking of most aerial observations.

The writer is indebted to Col. John R. Burns, CWS, for assistance during parts of this study; to the late George Langham, Capt. A.C., for skilled pilotage during aerial reconnaissances; to Maj. R. A. Davis, CWS, for check observations and for the photographs comprising Figures 5 and 6; and to Dr. Wallace T. Buckley, of Indiana University, for helpful suggestions during the preparation of this paper.

Fig. 1. Aerial view of Reservoir Butte, a former island in Lake Bonneville, shows Bonneville (*upper*) and Provo (*lower*) shore lines. Between Reservoir Butte (*center*) and the Simpson Mountains (*on horizon*) is the ancient river bed joining the Sevier Desert and the Salt Lake Desert. Distance between the two shore lines is about 350 feet.

2. Immediately[1] thereafter the region became at least as dry as at present, and so remained for a period measurable in tens of thousands of years. During this time massive fans were constructed at the bases of the mountains.
3. Early in the latter half of the Pleistocene, water rose in the Bonneville Basin to within about 90 feet of its maximum level—the Bonneville shore line. This level was retained continuously for a period long enough to permit the deposition of up to 200 feet of laminated yellow marl.
4. Water level fell to an undetermined low level and remained there for an undetermined period, probably much shorter than (2).
5. Water again accumulated in the basin, rising to the Bonneville shore line, and remaining there long enough to permit the deposition of up to 50 feet of thin-bedded white marl. This marl, and the yellow marl previously deposited, effectively seal the earlier fan deposits at their lower ends, creating an extensive buried aquifer, from which irrigation water, of some present and considerable future economic importance, is pumped.
6. The lake level declined from the Bonneville shore line to an undetermined low level, and for an undetermined time, probably short.
7. The lake again rose, attaining a height slightly above the Bonneville shore line, then declined fairly rapidly, with a series of minor stadia, to the Provo shore line, leaving the area between the two shore lines terraced with abandoned beaches (Fig. 3). This decline in level was, correctly, attributed by Gilbert to the lowering of the outlet from the basin at Red Rock Pass (Fig. 4, pass index, 1).
8. After a long pause at the Provo shore line, lake levels again declined, in a series of short-lived stadia (Fig. 3), to the Stansbury shore line, which may, in part, have been "left over" from a previous low-water regime; and then declined to or below the present level of Great Salt Lake.

Although this history is at present somewhat complex, a not-inconsiderable volume of evidence suggests that there may have been additional stages, Antevs, for example, postulating a dual occupation of both the Provo and Stansbury levels.

THE PROBLEM OF OUTLETS

Early in the course of his study of Lake Bonneville, Gilbert concluded, with excellent logic, that long-time stability of lake levels was only possible if the lake had a definite outlet and, hence, that Lake Bonneville must have had an outlet, for deeply incised shore lines, such as the Bonneville and Provo, could only be produced by long-continued wave action at a constant level. From the field evidence then available, Gilbert determined that this outlet had an elevation approximating that of the Bonneville shore line during earlier stages in lake history, and that it then was lowered to the Provo level, probably by erosion, during a later period.

Fig. 2. Sea cave on abandoned shore of Lake Bonneville, near Magna, Utah. In some of these caves, evidence of ancient man has been found.

Systematic investigation of the periphery of the Bonneville Basin, by Gilbert and his aides, disclosed a number of low passes in the basin rim. Further exploration showed that Red Rock Pass (Fig. 4, pass index, 1) not only was the lowest pass, but showed incontrovertible evidence of former great outflow from the Bonneville Basin to the Snake River. This discovery, reported in 1878,[7] solved the outlet problem as it was then understood, and today explains in satisfactory detail the decline in lake level from the Bonneville shore line to the Provo shore line, and the relatively long-term stadium at the Provo level.

Not explained by Gilbert's findings are the stabilization of lake levels at and near the Bonneville shore line during the deposition of the white marl and of the older yellow marl; and the factors permitting low-level stabilization (perhaps several times) at the Stansbury shore line. Although the possibility of other and older outlets was considered by Gilbert and others,[8] and very extensive surveys have been made in Utah since the termination of Gilbert's field work (1885), the problem remains unsolved to the present time, and at least one more outlet, to account for the early stabilization of lake levels, is called for by most current theories.

During the period 1928–1946, the entire problem of the outlet of Lake Bonneville was reviewed in considerable detail, in conjunction with attempts at correlation of glacial and pluvial stages from the California Sierras to the Colorado Rockies. This investigation, after preliminary cartographic study (Fig. 4; Maps), included an aerial reconnaissance of the entire periphery of the Bonneville Basin, and ground studies in all passes (Fig. 4, pass index) leading from the basin to exterior drainages.

Lower limit of the area in which outlets of Lake Bonneville might be found is quite obviously the Bonneville shore line, there being neither evidence nor suspicion of underground outlets at present or in the past. Consideration of post-Bonneville deformation of the region[9] indicated that, in some areas at least, such as the Escalante Desert in southwestern Utah, a pass now as much as 5,450 feet above mean sea level might have served as an outlet in Bonneville time; and backward extrapolation of this deformation suggested that all passes below the present 6,000-foot contour should be considered. In consequence, the search area (Fig. 4) was defined as all parts of the Great Basin below the 6,000-foot contour, from the Bonneville shore line to the 5,000-foot contour of an exterior drainage. During the course of field checking of these areas, a large part of Gilbert's field evidence was inspected and verified.

RED ROCK PASS

Occupying a structural valley between the Bannock and Portneuf Ranges, about 90 miles north of Odgen, Utah, is Red Rock Pass (Fig. 4, pass index, 1), the lowest gap in the periphery of the Bonneville Basin. This pass is today traversed by the main railroad from Ogden, Utah, to Pocatello, Idaho, and by a paved highway connecting the same points. Highest point in the pass is about 4,380 feet above mean sea level, an elevation far below the Bonneville shore line, and only a few feet above the Provo.

During his investigation of Lake Bonneville, Gilbert visited Red Rock Pass, studied it in considerable detail, and published a clear, concise description (1878), which was later expanded and augmented by maps and sketches.[10] Gilbert found that the Bonneville shore line entered the southern part of the pass, but terminated abruptly near its center, at the gap between a pair of red limestone buttes on opposite sides of the valley (Figs. 5, 6). Floor of the pass was level, and very slightly above the Provo shore line, but contained evidence of recent (post-Provo) filling. North of the Red Rock buttes (Fig. 6), the valley floor slopes gently northward, draining into the Snake River via Marsh Creek and the Portneuf River.

Near the present town of Arimo, which is about 18 miles north of Red Rock Pass, the valley of Marsh Creek contains a lava tongue, of pre-Bonneville (probably later Tertiary) age, on each side

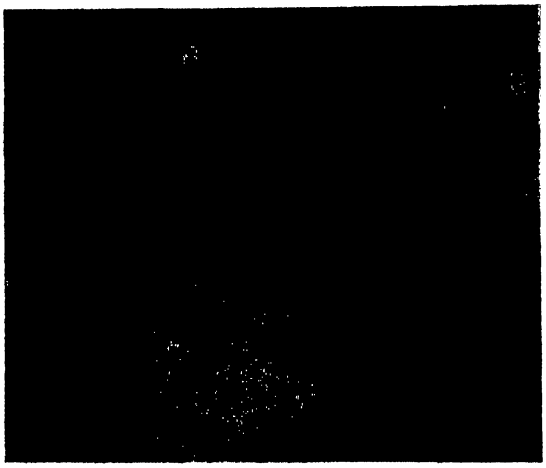


Fig. 3. Major and minor shore lines of Lake Bonneville, as seen from a terrain clearance of 20,000 feet. *B*, Bonneville shore line; *P*, Provo. Other terraces, parallelling these, were cut and constructed during minor lake stadia.

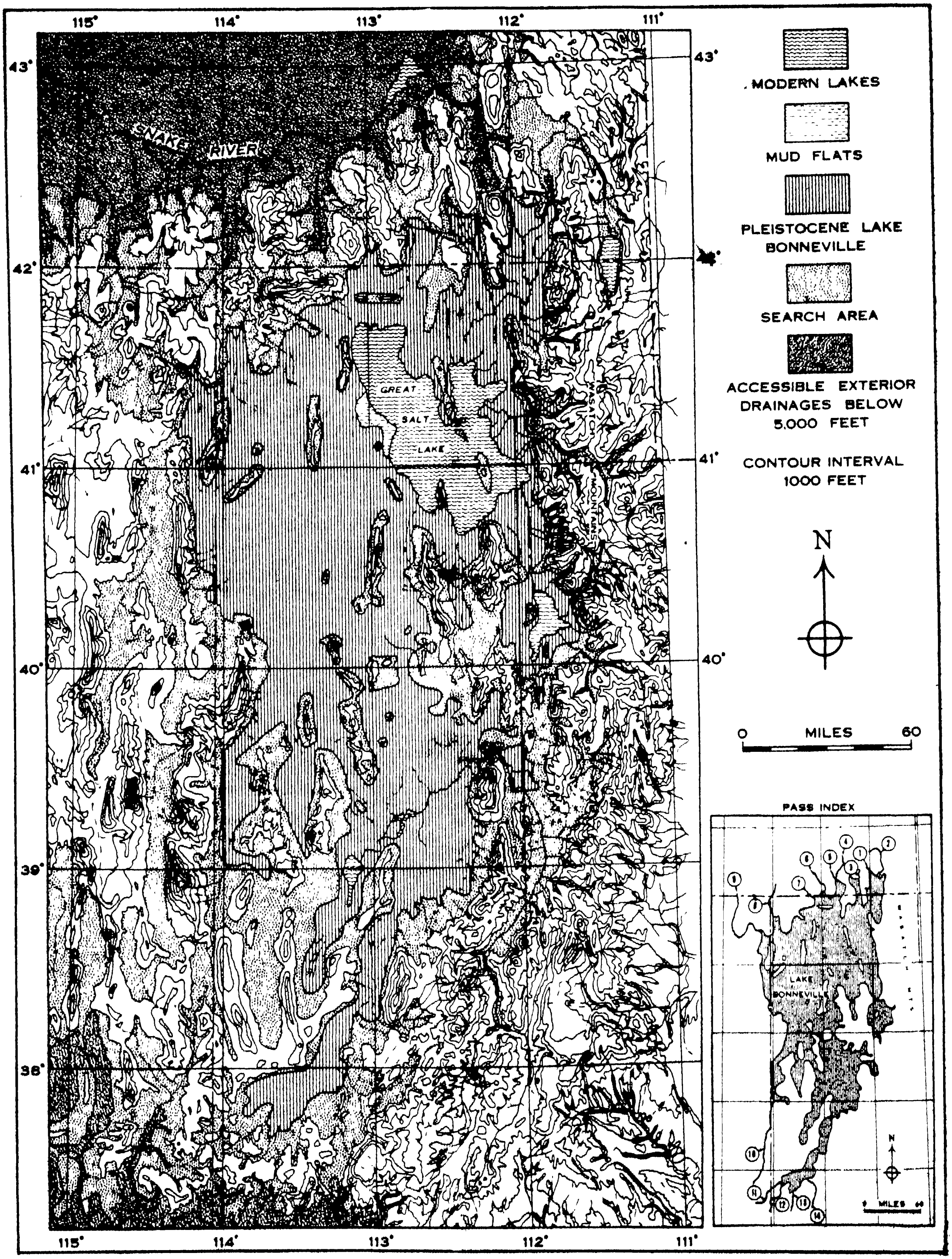


Fig. 4. Summary map of the Bonneville Basin, showing modern lakes and mud flats, extent of Pleistocene Lake Bonneville, "search area" in which outlets should be, and accessible exterior drainages. "Pass index" in lower right is for identification purposes.

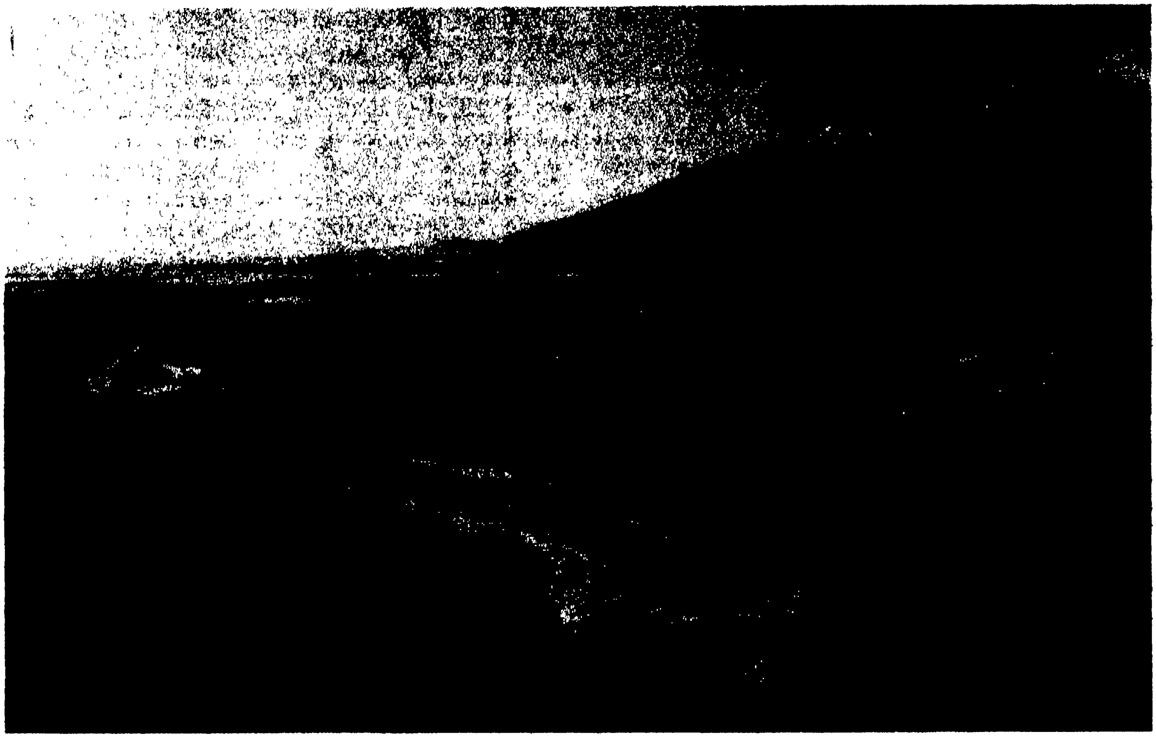


Fig. 5. View south in Red Rock Pass, from the east wall. Note steep inner bank of channel eroded in alluvium on west wall below Oxford Mountain (*upper right*). Swamp in middle distance is the result of post-Provo interruption of drainage, due in part to local deposition.

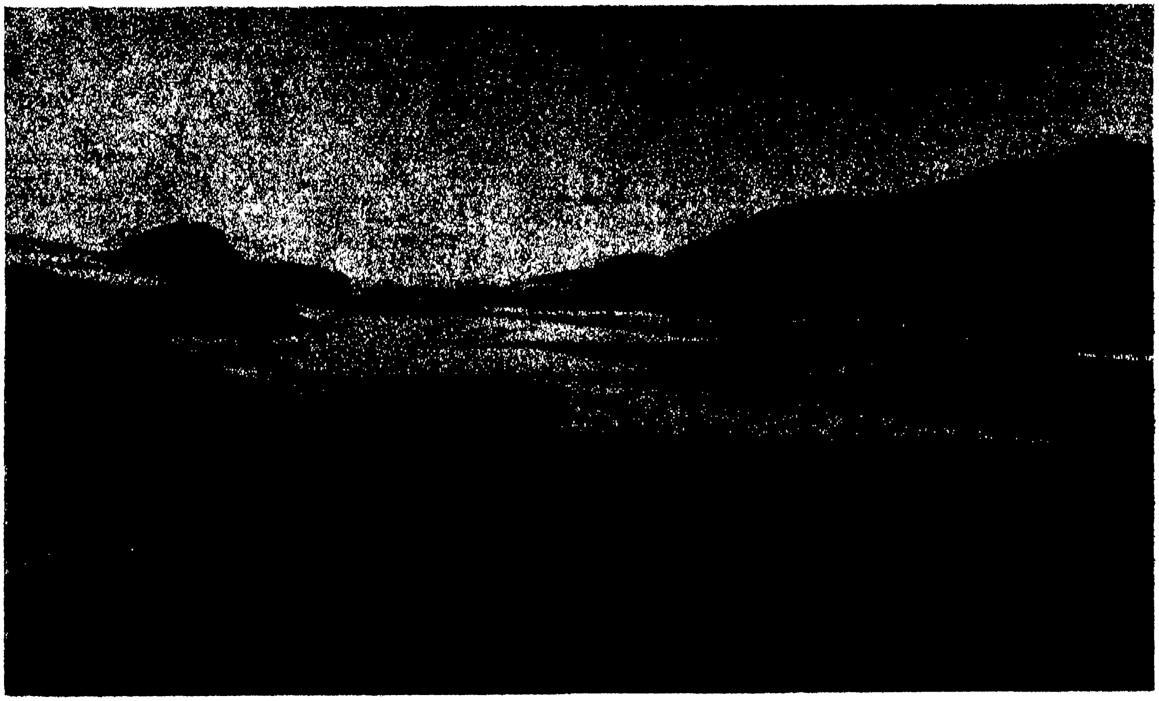


Fig. 6. Red Rock Pass from the north, showing the red limestone buttes (*middle distance, right and left*), the flat-floored channel (*center*), and the alluvial fans (*right and left*) which formerly coalesced, damming the channel to a level above the present 5,200-foot contour.

of which a deep channel has been eroded. Today, the eastern channel is occupied by the Portneuf River and some of its tributaries, and the western channel, parallel and only a few hundred feet distant, carries the waters of Marsh Creek. Several to many yards above the present stream levels, the upper surface of the lava is carved, fretted, and potholed by the now-vanished torrent that once flowed through Red Rock Pass from the Bonneville Basin.

Gilbert concluded from this evidence that Red Rock Pass was the overflow point in the rim of the Bonneville Basin during the last rise of the water to the Bonneville shore line, that the fall to the Provo level was caused by the washing out of the barrier of alluvial material formerly blocking the pass, and that the stabilization at the Provo level occurred when erosion reached bedrock in the bottom of the pass.

Thorough field checking of this area indicates that Gilbert's findings are entirely correct, a conclusion also reached independently by Major R. A. Davis, CWS. Valley wall deposits north of Red Rock Pass were scrutinized with considerable care because of Peale's contention (1878) that the outlet of Lake Bonneville was near the Marsh Creek–Portneuf junction (Map S), rather than at Red Rock Pass. A few vague indications of shore lines were noted from the air between Red Rock Pass and Downey, and a number of small isolated masses of alluvium, similar to terrace material, were found at various elevations above the valley floor in different parts of this region, but no evidence was found to relate them either to each other or to any phase in the history of the Bonneville River. Some of this material appears older than Pleistocene.

The possibility that the dam of alluvial material in Red Rock Pass was washed out during an early lake stage, after stabilizing the level for a considerable time, and then was rebuilt by further deposition, to repeat the process at a later stage, producing the evidence now present and obliterating the evidence of earlier outflow, was suggested by Gilbert.[11]

Because this suggestion is not only in rational accord with standard geological mechanics, but would also account for some or all of the "troublesome" early stabilizations of lake level, a rather careful investigation of deposition rates in this area was made. Although present datings of Pleistocene occurrences still leave much to be desired, we can, for the purposes of this study, assume (following Antevs) that the last outflow through Red Rock Pass took place not less than 20,000 years ago. Depth to bedrock, in the approximate center of Red Rock Pass, as shown by excavations for the highway overpass (Fig. 5, center), is about 15 feet, half the fill being bog deposit mixed with alluvium, the lower half being hard (stream gravel). If it is assumed that all this material is post-outflow deposition, an average rate of deposition of about 0.00075 foot, or about 0.01 inch, per year results. Because the rate of radial growth of an alluvial cone declines as the radius and height increase, a linear extrapolation, giving a time of growth of 350,000 years for the barrier, is incorrect and excessive. When due corrections are made, it appears that the time necessary for the filling of Red Rock Pass from the Provo to the Bonneville level (about 350 feet) is not less than 250,000 years.[†] As this time interval is almost twice the probable total time since the earliest high level in the Bonneville Basin (the yellow marl, or "Escalante," stage), as is indicated by Flint's latest age estimate,[12] it appears that Gilbert's plausible suggestion cannot be applied in this specific case because of inadequate time.

In consequence, at least one outflow from the Bonneville Basin, prior to the final break-through at Red Rock Pass, is called for by present theories and must have occurred at some location other than Red Rock Pass.

THE GENTILE VALLEY–BEAR RIVER–PORTNEUF RIVER AREA

Easternmost of the topographic "lows" connecting the Bonneville Basin with the Snake River drainage consists of a tortuous series of valleys and lava flows between the Bear River (Map A) and the Portneuf River. In this area, a long arm of Lake Bonneville once extended up Gentile Valley toward Grace, Idaho, where the delta of the Bear River was located during high-water stages.

From the northernmost point on the 5,100-foot contour in Gentile Valley (a level approximately that of the Bonneville shore line) to an equal elevation in the valley of the Portneuf is less than 15 miles, with no intervening elevation exceeding 5,800 feet. Land surface in many parts of this area, locally known as Basalt Valley, is composed of basalt flows, some definitely of Tertiary age, others

[†] Simplified formula for cone growth is:

$$\frac{dr}{dt} = C\left(\frac{Z - r\tan\theta}{r^2\tan\theta}\right),$$

in which:

$\frac{dr}{dt}$ = rate of increase of radius of cone
C = constant of proportionality
Z = height of source cliff
r = radius of cone
θ = angle of rest of component materials

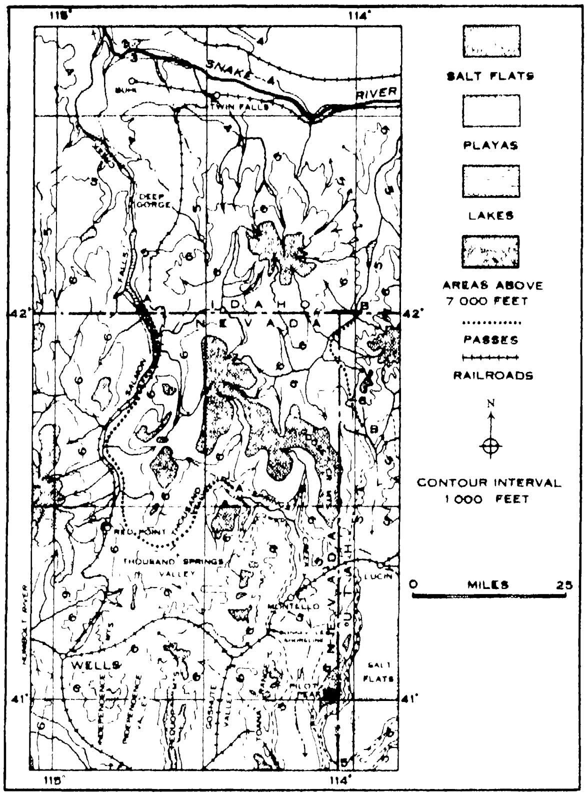


Fig. 7. Summary map of the Thousand Springs area, showing axis of pass (*A—A'*), major stream channels, and elevations. Contour designations are in thousands of feet. Secondary pass (*B—B'*) is Goose Creek Pass (No. 8 in Fig. 4).

younger, possibly Pleistocene. Beneath this basalt, which has a thickness exceeding 250 feet in many places, is alluvium, of probable Tertiary age, locally interstratified with lavas, and carrying large quantities of water, probably from the Bear to the Portneuf River. This suggests that an ancient channel of the Bear River, which once connected with the Portneuf, has been obliterated by lava flows, which may explain the "looped-back" course of the Bear River. (Such looped courses are quite common throughout the Basin-and-Range Physiographic Province, other prominent examples being the Sevier River in Utah and the Rio de Bavispe; in Sonora.)

Aerial reconnaissance of the lava region disclosed no clear evidence that a channel through the older lavas (Tertiary) had been filled by later flows (Pleistocene, with much doubt). Ground investigation confirmed this finding, and showed that there was no close agreement between the positions of present topographic "lows" and the positions of later lavas. Backward extrapolation of regional distortions gave no indication that these lavas were depressed far enough, at any time in the Pleistocene, to permit outflow across them, either from Bear River or from a lake in the Bonneville Basin.

Investigation of the valley of the Portneuf, from Soda Springs (Map A) to McCammon, showed plainly that it has not carried, since the present channel was eroded (Tertiary), any flow greatly in excess of that at present. Shifting of the channel of the Portneuf, from McCammon northward, from the west side of the Marsh Creek lava tongue (eroded by outflow from Red Rock Pass) to the east side, after the lowering of Lake Bonneville, is indicated by the presence of a partly obliterated channel across the lava approximately 11 miles south (upstream) of the present Portneuf–Marsh Creek junction. Detailed descriptions of this area are contained in the journals of Captain Bonneville, who wintered at least once a few miles east of this junction, near "Beer Springs," on the Portneuf.

Assuming the substantial correctness of Mansfield's datings[13] of the lavas in Basalt Valley, and the absence of evidence of any marked change in relative levels, it appears that the Gentile Valley–Bear River–Portneuf River area was not an outlet from the Bonneville Basin during the later part of the Pleistocene.

THE NORTH RIM PASSES

West of Red Rock Pass, on the north rim of the Bonneville Basin, there are six passes (some having multiple adjacent channels) below 6,000 feet from the basin to the Snake River drainage (Fig. 4, pass index, 3, 4, 5, 6, 7, 8). This drainage divide is locally known as the Raft River Mountains. This entire area was studied in some detail by Gilbert, who found that the Bonneville shore line was below the summit of each pass. Restudy of this area, using modern maps and allowing for regional distortion at a uniform rate, indicates that none of these passes was in position to serve as an outlet during the last half million years.

All these passes are easily accessible by road, and all have been surveyed in some detail. Ground study indicates that not only is there no direct evidence of flow over the pass summits, but that the channels extending from the passes toward the Snake River show no evidence of formerly augmented through flow. Thus, although the Raft River area is of considerable interest to the modern physiographer and geomorphologist, it apparently does not offer a solution to the problem of outlets during the Pleistocene.

THOUSAND SPRINGS PASS

At the northwest corner of the area under consideration is Thousand Springs Pass, an important topographic low supplying a workable railroad route from northeastern Nevada to the Snake River Valley, as well as a low saddle from the Bonneville Basin to the headwaters of the Humboldt River, an important transcontinental thoroughfare since 1849. Despite its importance, parts of the area have never been mapped in detail, and most modern maps are based on Beckwith's reconnaissances, made in 1861 (Map T). A map of this area, based on extant maps, aerial photographs, and modern ground studies, comprises Figure 7.

The Thousand Springs area has had a most complex geological history, only a small part of which pertains to the problem at hand. The lower part of the pass, in the vicinity of Montello, is now a small playa, although, during high stages in the Bonneville Basin, it was a part of the main lake. A small bar separates the western part of the basin from the main Salt Lake Desert, so that there is now no "through" drainage. This may be in part the result of relatively recent uplift of a buried northward extension of the Pilot Range. Because the Bonneville shore line is locally obscured in many places by slope wash, local evidence of distortion is somewhat inconclusive.

Northwest of Montello, draining Thousand Springs Valley, is the canyon of Thousand Springs Creek, a rugged canyon incised at the junction of the Toana Range, composed largely of Paleozoic sediments and metasediments, and the Goose Creek Mountains, a mass of Tertiary lavas, with occasional inclusions of granite and black limestone.‡ Several faults roughly parallel the canyon, and parts of its course, seem to be eroded through zones of brecciation.

The Bonneville shore line can be traced into the canyon, but not through it. Toward the western end the shore features merge with, and are covered by, stream terraces related to high stages of Thousand Springs Creek, an ephemeral torrent. In many places both stream and lake deposits are concealed (or perhaps removed) by local "cloudburst" deposits. Some maps show an "improved" highway through this canyon. The present road (1947) can be traversed without too much difficulty by a jeep or half-track with the "creeper" mechanism in good condition.

Thousand Springs Valley (Fig. 7) consists of a series of basins, which are interconnected during stages of high water but are independent "blind" playas during ordinary seasons. During high-water stages at present (and during Pleistocene pluvial stages), these playas overflow through Thousand Springs Canyon into the playa at Montello. The oldest sequence of playa deposits, now somewhat tilted, is quite ancient, and may be a part of the Humboldt Formation, of Pliocene (?) age. A second sequence, only slightly tilted, is placed in the later Pleistocene and probably represents the same pluvial climate that produced the high stages in the Bonneville Basin. The most recent deposits, still forming, are quite thin compared to the others, and are currently undergoing cut-and-fill erosion and redeposition, suggesting a recent change in local erosional equilibrium.

North of Thousand Springs Valley, and separated from it by a pass approximately 5,850 feet above m.s.l., is the canyon of Salmon Falls Creek, a tributary of the Snake River. This canyon, very deeply intrenched in some places (Fig. 7), is fairly

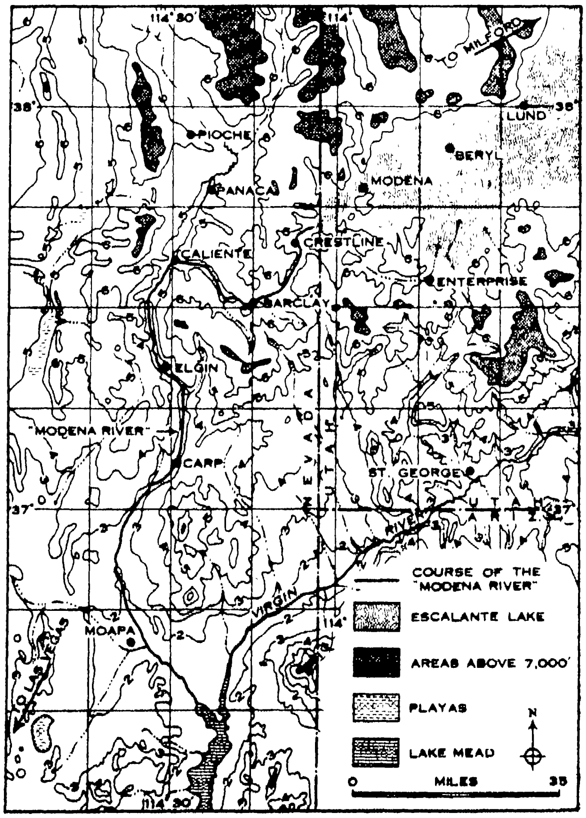


Fig. 8. Sketch map showing course of hypothetical Modena River from the Bonneville Basin to the present course of the Colorado River. Contour lines, designated in thousands of feet, are modern.

---

‡ This material closely resembles the granite and black limestone at Granite Peak (Lat. 40° 07′ N.; Long. 113° 15′ W.: Map B: Ives, R. L. The Granite Peak Area. *Utah, Rocks and Minerals*, 1946, **21**, 339–46.

typical of the Snake River lava area and of its rainfall regime, and bears no convincing evidence of having carried, in the Pleistocene, any increase in flow not compatible with local rainfall increases at that time.

For Thousand Springs Pass to have served as an outlet for a lake in the Bonneville Basin at the Bonneville level (*ca.* 5,100 feet above m.s.l.), the summit of the pass, at that time, must have been depressed to that level, or about 750 feet relative to its present position. Measurements of the deformation of the Bonneville shore line indicate a maximum elevation of only about 15 feet at the west end of Thousand Springs Canyon since the Bonneville high-water stage. The most generous backward extrapolation of this warpage rate suggests that the summit of Thousand Springs Pass was about 700 feet too high, during the time under consideration, to serve as an outlet from the Bonneville Basin. Similar considerations suggest that there was no connection between the Bonneville Basin and the Lahontan Basin, to the west, during the later Pleistocene; but field study suggests a considerable "swapping of tributaries" by stream piracy during this time. Unless new evidence is found, it appears that Thousand Springs Pass is another of the places where Lake Bonneville and its predecessors did not overflow.

THE WESTERN PASSES

Along the western margin of the Bonneville Basin, above the Bonneville shore line and below the 6,000-foot contour, is a maze of interconnected and poorly drained desert valleys having a total length of perhaps 600 miles (Fig. 4). These, typical bolsons, contain a large number of ephemeral playa lakes and resemble, in their major features, the Jornada del Muerto area of New Mexico.

Shortest channel from Lake Bonneville to the Colorado River (Fig. 4, pass index, 10) is through Snake Valley, in which there are two high points, both more than 5,900 feet above mean sea level. Present (and ancient) drainage from the area between these barriers is outward, one flow going into the Bonneville Basin and the other, via Duck Valley, going to the Colorado.

Thorough aerial and ground study in this area shows that all flow of which there is any remaining evidence was out of the maze of valleys, not through it; and that, when regional tilting is considered, even most generously, the area could not have served as an outlet during the time under consideration.

CRESTLINE PASS

Joining the southwestern extension of the Escalante Desert with the valley of Meadow Creek is Crestline Pass (Fig. 4, pass index, 11), a depression between the Mormon Mountains (southeast) and the Clover Valley Mountains (northwest). The western end of this pass drains into Meadow Creek via Clover Valley, and eventually to the Colorado River. The old California and Salt Lake Stage Road traversed this gap, which today is used by the Los Angeles and Salt Lake Railroad.

Aerial and ground reconnaissances of the Crestline Pass area disclosed that the eastern side of the pass, extending from Crestline (Nevada) to the Escalante Desert (Utah), was a typical low-gradient valley, like several dozen others on the north,

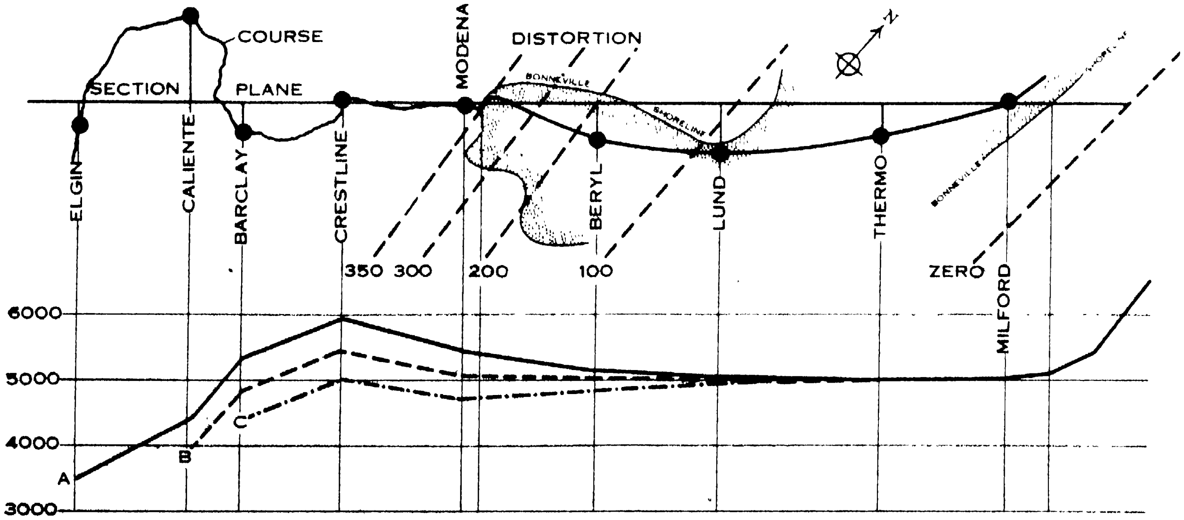


Fig. 9. Present and reconstructed profiles across a section from Elgin, Nevada, to Milford, Utah.

west, and south sides of the Bonneville Basin, with some erosion at its head and some fan deposition where the pass joined the main Escalante Desert.

The western side of the pass, however, from Crestline to Caliente, is disproportionately deep for its drainage area and has the general appearance, from any aspect, of the abandoned channel of a fairly large desert river. This valley is clearly visible on the north side of the main airway from Salt Lake City to Las Vegas, Nevada.

Ground investigations disclosed a considerable depth of fan materials in the valley and indicated that climatic and depositional conditions have been substantially like those of today for a period sufficient to permit accumulations exceeding 1,000 feet thick in some places. Whether this entire sequence is all Pleistocene, or whether it includes both Pleistocene and later Tertiary deposits, including perhaps the Panaca formation,[14] was not satisfactorily determined from either field evidence or stratigraphic descriptions.

Plentiful evidence of cut-and-fill action, normal to valleys having violently fluctuating flow rates, was found in many parts of the valley, at various levels. No convincing evidence of long-continued recent steady flow conditions was found anywhere in the area.

Because the terrain west of Crestline strongly suggests deep and geologically recent trenching of an area having little rainfall, and probable recent connection of formerly discrete playa drainages into the system now flowing into the Colorado River through Meadow Valley and Muddy River, the former existence of a fairly large river, here called the Modena River, can be postulated. Topography of the course of the hypothetical Modena River is shown in Figure 8. It will be noted that the channel here is more deeply incised than that of the Virgin River in some parts of the course.

Regional warping since the cutting of the Bonneville shore line was measured by Gilbert. These data have been transferred to Figure 9, and from them a series of profiles has been constructed. The first of these (*A*, Fig. 9) is the modern profile from Elgin, Nevada, to Milford, Utah. The second (*B*, Fig. 9) is a profile of the same area during the Bonneville Lake stage, it being assumed that Gilbert's measurements of distortion (Fig. 9, "distortion") are at least substantially correct. From Gilbert's researches, the time interval between the deposition of the yellow marl ("Escalante Lake Stage") and the cutting of the Bonneville shore line is shown to be somewhat greater than the total time lapse since the cutting of the shore line. If these times are assumed equal, and distortion for the whole period proceeded at a substantially uniform rate, the profile during the Escalante Stage approximates that shown in *C* (Fig. 9). Note here that the low point of the basin has shifted from near Modena in Escalante time to near Thermo at present. This shift may account for the apparent "pinch-off" of the lake near Thermo,[§] the apparent duality of the Bonneville shore line in the southern part of the basin, and the lack of register of successive deposits in this area.

Consideration of profile *C* shows that the waters of the Escalante Stage, which rose to within 90 feet of the Bonneville shore line at Leamington, on the axis of zero distortion, could have overflowed through Crestline Pass; and hence that such an outlet, through the Modena River, is a possibility!

Using the same basic data and various other assumed positions of the "hinge point" (chosen just west of Milford for Fig. 9), and also various distortions other than a simple tilting of the block extending from Elgin, Nevada, to Milford, Utah, additional profiles (not shown), of various curvatures and magnitudes compatible with Gilbert's distortion figures, were constructed. All these produce a topographic "low" at or near Crestline, during the Escalante Lake Stage, at a level sufficiently low to permit outflow through it. In fact, if some of the Pleistocene chronologies are used, the pass is embarrassingly low!

The hypothetical Modena River provides the water-level stabilization necessary to account for the substantially uniform conditions during deposition of the yellow marl, explains the deep incision of Clover Valley and Meadow Canyon,[15] and may have a definite bearing on the still-unsolved problem of the age and genesis of the present course of the Colorado River.[16] Additional extrapolation, toward mid-Pleistocene, indicates that the Bonneville Basin may not have been closed until about 500,000 years ago, so that the area could not have contained a great lake during the early Pleistocene glaciations, and the Sevier River, which now evaporates in Sevier Lake, could have flowed outward to the sea through such a gap. Theory and observations are in apparent concordance here, for there exists no evidence of any very high lake stage in the Bonneville Basin prior to about Illinoian time.

THE SOUTH RIM PASSES

On the south rim of the Bonneville Basin, between Crestline Pass and the west flank of the

---

[§] A number of interrelated studies in the vicinity of Thermo show one or more transverse bars on the valley floor, indicating that the Escalante Desert was isolated from the main lake at times.

Markagunt Plateau, in which the multiple canyons of Zion Park are eroded, are three groups of passes (Fig. 4, pass index, 12, 13, 14), all eroded into a complex basement of mixed volcanics and sediments, which was formerly capped in its entirety by lavas of early and middle Tertiary age. These passes are quite complex, the westernmost, to which the field name "Hebron Labyrinth" was applied, having outlets to both the Muddy and the Virgin drainages. The central pass, through Mountain Meadows, site of a famous massacre, is apparently a residual portion of an ancient river, much like Dutton's Kaibab River. The easternmost pass, now traversed by the main road from Cedar City to Zion National Park, has all the earmarks of an ancient outlet from the lake basin except that it is now, and was in the past, by any rational application of modern geological principles, more than 700 feet too high.

THE STANSBURY PROBLEM

The foregoing investigation of outlets shows that about one half the high-water stages can be related to the history of Red Rock Pass, as outlined by Gilbert. Some hope of accounting for the remaining high-water stages, prior to those controlled by the Red Rock Pass outlet, is given by the apparent warping of the basin floor in the southwestern part, leading to a possible outflow in the vicinity of Crestline.

This same evidence, however, shows plainly and convincingly that there is no possibility of an outlet at the Stansbury Level, about halfway from the Provo Level to the present level of Great Salt Lake (*ca.* 4,200 feet above m.s.l.), unless recent and radical changes took place in the Bonneville Basin without leaving any trace. Such changes being improbable, the best present explanation of the Stansbury water-level stability seems to be an evaporational hypothesis. At and near the Stansbury shore line, which is present only on high vertical cliffs, the average gradient of the lake shore is slight. Computations show that in parts of this area a raising of the lake level by only one foot would triple the volume of water contained and would quadruple the area. In consequence, because of this "bathtub effect," a shore line would be incised on vertical cliffs, but not upon gentle slopes; and minor changes in inflow, by a factor such as two, would have a negligible effect on the water level.

Restudy of the Bonneville Basin suggests that Gilbert "builded better than he knew," for not only is the outlet described by him shown to be the one

MAP INDEX

Map	Area	1	2	3	4	5	6	7	8	9	10	11	12	13	14
A	×	×	×	×	×	×	×	×	×						
B	×				×	×	×	×	×	×					
C	×										×				
D	×											×	×		
E	×										×	×	×		
F	×										×				
G	×								×	×		×	×		
H	×								×						
I		×		×		×									
J										×		×	×	×	×
K	×									×	×	×	×	×	×
L														×	×
M				×		×	×					×	×	×	×
N											×	×	×		
O											×	×	×		
P										×	×	×			
Q										×					
R		×		×	×										
S		×													
T										×					
U									×	×					
V												×			
W		×	×	×	×	×	×	×	×	×					
X											×	×	×	×	×

of greatest importance, but other evidence in his report offers a possible solution to a problem unknown to him: that of earlier outlets.

At the present time, with evidence now available, the Modena River and the "bathtub effect" are working hypotheses which may hold the answer to the problem of early outlets and the equally complex puzzle of stabilization of water levels at the Stansbury shore line. Present evidence suggests that the described effects could happen: only extensive additional field work, in many areas, will show whether they did, or did not, happen.

MAP REFERENCES

A. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Pocatello.*

B. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Salt Lake City.*

C. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Grand Junction.*

D. U. S Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Grand Canyon.*

E. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Mt. Whitney.*

F. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Reno.*

G. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Elko.*

H. U. S. Coast and Geodetic Survey Sectional Aeronautical Chart (USAAF APC 6) *Boise.*

I. Gilbert, G. K. (1890). Lake Bonneville, *U. S. Geol. Surv. Monogr. 1,* Map in pocket.

J. Dutton, C. E. (1882) Tertiary History of the Grand Cañon District, *U. S. Geol. Surv. Monogr. II,* Atlas sheet XX

K. U. S. Department of the Interior, General Land Office, *State of Utah,* 1 inch = 12 miles, 1937.

L. U. S. Geological Survey, *Iron Springs Special Quadrangle,* advance sheet

M. Rand-McNally Road Map, *Utah--Nevada,* 1946.

N. U. S. Coast and Geodetic Survey Radio Direction Finding Chart (USAAF DF 1) # *24 DF.*

O. Spurr, J. E. (1903) Descriptive Geology of Nevada South of the Fortieth Parallel and Adjacent Portions of California. *U. S. Geol. Surv. Bull. 208,* Map in pocket.

P. U. S. Department of the Interior, General Land Office, *State of Nevada,* 1 inch = 12 miles, 1930

Q. Lee, W. T., Stone, R. W., Gayle, H. S., and others. Guidebook of the Western United States, Part B, The Overland Route, *U. S. Geol. Surv. Bull. 612,* 1915, Sheet 17.

R. Rand-McNally Road Map, *Idaho,* 1946 or 1948.

S Reference Q, sheet 15B.

T. Beckwith, E. G. Explorations and Surveys for a Railroad Route from the Mississippi River to the Pacific Ocean, *Senate Exec. Doc. 78,* 1861, **2**; *Report of Lt. Beckwith,* **XI**, Map 2.

U. Fig. 7, this paper.

V. Fig. 8, this paper.

W. World Aeronautical Chart, 1/1.000,000, sheet 305.

X. World Aeronautical Chart, 1/1,000,000, sheet 362.

REFERENCES

1. AUERBACH, H. S. (Trans.) Father Escalante's Journal, with Related Documents and Maps. *Utah State Hist. Quart.,* 1943, **11**, 75.
2. IVES, R. L. An Early Report of Ancient Lakes in the Bonneville Basin. *J. Geol.,* 1948, **56**, 79–80.
3. STANSBURY, H. *Exploration and Survey of the Valley of the Great Salt Lake of Utah.* Philadelphia: 1852.
4. GILBERT, G. K. Lake Bonneville. *U. S. Geol. Surv. Monogr. 1,* 1890.
5. BLACKWELDER, E. Pleistocene Glaciation in the Sierra Nevada and Basin Ranges. *Bull. Geol. Soc. Amer.,* 1931, **42**, 865–922.
6. ANTEVS, E. On the Pleistocene History of the Great Basin in Quarternary Climates. *Carnegie Instn. Publ. 352,* 1925, 53–116; Correlation of Wisconsin Glacial Maxima. *Amer. J. Sci.,* 1945, **243-A**, 1–39.
7. GILBERT, G. K. Probable Ancient Outlet of Great Salt Lake. *Amer. J. Sci.,* 1878, **15**, Ser. 3, 65; The Ancient Outlet of Great Salt Lake. *Amer. J. Sci.,* **15**, Ser. 3, 257.
8. PEALE, A. C. The Ancient Outlet of Great Salt Lake. *Amer. J Sci.,* **15**, Ser. 3, 439.
9. GILBERT, G. K Lake Bonneville, *U. S. Geol. Surv. Monogr. 1,* 1890, 365–76.
10. GILBERT, G. K. Lake Bonneville. *U. S. Geol. Surv. Monogr. 1,* 1890, Pl. XXVII, XXIX, 171–78.
11. GILBERT, G. K. Lake Bonneville. *U. S. Geol. Surv. Monogr. 1,* 1890, 181.
12. FLINT, R. F *Glacial Geology and the Pleistocene Epoch.* New York: 1947, 400, 473
13. MANSFIELD, G. R. Geography, Geology, and Mineral Resources of Part of Southeastern Idaho. *U. S. Geol. Surv. Prof. Paper 152,* 1927.
14. SPURR, J. E. Descriptive Geology of Nevada South of the Fortieth Parallel and Adjacent Portions of California. *U. S. Geol. Surv. Bull. 208,* 1903, 143–45.
15. SPURR, J. E. Descriptive Geology of Nevada South of the Fortieth Parallel and Adjacent Portions of California. *U. S. Geol. Surv. Bull. 208,* 1903, 139–48.
16. LONGWELL, C. R. How Old is the Colorado River? *Amer. J. Sci.,* 1946, **244**, 817–35.

THE INEVITABILITY OF SCIENTIFIC DISCOVERY

AARON J. IHDE

Dr. Ihde (Ph.D., Wisconsin, 1941) is associate professor of chemistry and integrated liberal studies at his alma mater. His research has been in food chemistry, but he has long been interested in the relationships between scientific and cultural developments. His article is from an address presented at the Annual Meeting of the Wisconsin Academy of Sciences, Arts and Letters, April 24, 1948.

THE history of science reveals numerous instances of discoveries made simultaneously by two or more independent workers. At times this circumstance has resulted in violent battles over claims of priority on the part of the supporters of the opposing parties, as witness the discovery of the calculus by Newton and by Leibnitz, and the extensive litigation resulting from the simultaneous discovery of the telephone by Bell and by Gray. At other times this circumstance has resulted in the manifestation of some of the finer instincts of mankind, as in Alfred Wallace's gracious bows in the direction of Charles Darwin after both had independently arrived at an evolutionary theory based upon natural selection.

Ogburn and Thomas (1922) tabulated a list of one hundred forty-eight instances of such simultaneous independent discoveries taken from the fields of mathematics, science, and technology. In many of their examples more than three independent workers attained the same goal within months of one another. In some cases their examples are open to question, but this in no way decreases the import of their compilation. They list nine men as possible inventors of the telescope, though there is now general agreement that some of these workers did not arrive at the idea independently but obtained it by diffusion of gossip from lens shop to lens shop. Galileo, at least, is recorded as having heard of such an instrument before he constructed his own.

Such instances as these suggest the hypothesis that when the time is ripe a certain discovery becomes inevitable. This thought leads at once to the question, "What constitutes ripeness?" A number of answers suggest themselves. The theologian is apt to suggest that a benevolent deity has decided to pass this blessing on to mankind. Such an answer is unsatisfactory to the scientist, who insists on finding causal relationships more closely associated with the problems immediately at hand.

The answers that suggest themselves to the scientist are:

1. A need has arisen, so a search is undertaken to fill that need.
2. The genius with the proper insight into the problem has reached his maturity.
3. The fundamental background of knowledge necessary for the discovery has been accumulated.

The first answer has as its basis the oft-heard remark that "Necessity is the mother of invention." There is basis for the belief that an expressed need will result in an effort to satisfy that need, but this is only part of the picture. As Ogburn (1922) said in this connection, "In earlier times the necessity for quicker transportation, or a more stable food supply, or methods of preventing the death of babies was perhaps more urgent than now, but such wants did not produce inventions." The needs of the Alexandrian astronomers for a more refined tool for studying the movements of the planets did not produce the telescope. The need for painless surgery did not bring about the early use of anesthesia. The need for an instrument to study the chemical composition of the stars did not bring about the development of the spectroscope.

Obviously, necessity is no more than a minor factor in bringing about discoveries and inventions. Necessity can only direct attention toward problems that require solution but cannot bring about their solution if certain other things are lacking. Let us pass on to the second proposal.

Much popular writing about scientific discovery has pointed to the superior intellect and ability of those making great discoveries. According to this school, mankind would still be struggling along without benefit of the Mendelian laws of heredity had Mendel never lived, or without the germ theory of disease had there been no Pasteur, or without X-ray had Röntgen died in infancy. In spite of the enthusiasm of the popularizers, there seems to be little question but that all such knowledge would now be available to us. The large number of times that important discoveries were made independently by another scientist rules out the thesis that a certain person was essential to the discovery. True, the discovery might not have been recognized quite as soon, but recognition was certain to come.

Mendel's discovery, in fact, failed to receive recognition for thirty-four years following publication, partly because the journal in which he published was not widely distributed, partly because biological thought was in the throes of controversy over Darwinism. Three botanists, De Vries of Holland, Collens of Germany, and Tschermak of Austria, actually rediscovered the Mendelian principles and published within several months of one another in 1900. The fact that all three had become aware of the existence of Mendel's paper before they published does not nullify the importance or originality of their own work. All of them should have arrived at the fundamental laws had Mendel's paper been nonexistent.

Similarly, the calculus did not require an Isaac Newton. Leibnitz made the same discovery. Had there been no Newton or Leibnitz, it is conceivable that another mathematical thinker could have succeeded. The argument can be advanced that only a mind such as that of a Newton or a Leibnitz was capable of comprehending such a concept. One may ask whether the mind of Euler, Lagrange, Laplace, Gauss, or a Bernoulli might not have succeeded with the fundamental background then available.

Kroeber (1917) has stated,

> . . . no one can sanely believe that the distinction of Darwin's greatest accomplishment, the formulation of the doctrine of evolution by natural selection, would now stand to his credit had he been born fifty years sooner or later. If later, he would have been infallibly anticipated by Wallace, for one thing; by others, if an early death had cut off Wallace. That his giant restless mind would have evolved something noteworthy is as likely as it is away from the point: the distinction of the particular discovery which he did make, would not have been his. Put on earth, by contrary supposition, a half century earlier, his central idea would not have come to him; as it failed to come to his brilliant predecessor, the evolutionist Lamarck.

One is led to question, further, the alleged need of a certain mind for a certain discovery. Although there is little question that discoveries are seldom made by those of inferior intelligence, there have been numerous instances of developments made more as a result of persistence than of mental brilliance. The record is filled with the names of those who made only a single important discovery. If their intellects were so extraordinary should they not have continued to make great contributions? Whenever, on the other hand, their work has been extensive they have generally contributed their full share of erroneous ideas.

Added to this is the fact that many important discoveries were completed while the worker was still a comparative youth. The U. S. Patent Office once published a "Table of Important Inventions." Of a total of eighty-five inventions, only two were made by men who had passed their sixtieth birthday, only eight by men in their fifties. Forty-three of these inventions were credited to men who had not yet reached thirty-five. Genius or learned background can hardly be the answer.

This brings up the third proposal, that discovery becomes inevitable when the fundamental background for the discovery has been laid. Here solid ground appears. Regardless of the need, and given an academy full of geniuses, a discovery remains impossible as long as any of the fundamental background is missing. Once the last bit of background has been accumulated, the discovery becomes possible. It may not be made at once, but its ultimate solution is sure to come.

Let us consider the atomic theory of John Dalton in this connection. Ideas about atoms were part of the philosophical repertoire of some of the early Greeks, but such a theory could not gain precedence over a theory of the continuity of matter until it could explain experimental observations better than a theory of continuity was able to explain such observations. Such an opportunity did not present itself until chemistry became a well-established science about 1800. By then the modern concept of elements was established, the analytical balance was in common use, the law of conservation of matter was generally accepted, the law of definite proportions was being debated, the law of partial pressures of gases was being discovered, and the law of multiple proportions was becoming apparent. The time was ripe for one steeped in the corpuscular philosophy of Newton to arrive at the idea of chemical atoms and attribute characteristic weights and properties to them. In fact, enough background had already been laid fifteen years before for William Higgins to arrive at essentially the same conclusion.

Although the chemical background of the first half of the nineteenth century led to a belief in atoms, it was not yet ready for an atom composed of parts. Such an atom had to wait until the atomic concept of Dalton had first become firmly established and then proved inadequate to explain new chemical and physical phenomena. In the last half of the century such inadequacies became glaring enough to bring about revision of atomic concepts leading to a particle composed of protons and electrons.

Discovery of the elements gives us another example of the need for background before discovery becomes possible. Seven metals and two nonmetals were known to the ancient world. The last gaps in the periodic table of some ninety elements have been filled only recently. The discovery of the elements has followed very closely the increased understanding of new chemical principles. Those elements known to the ancients are inactive enough that they either occur in nature in the elemental form or are easily reduced to the element in a hot charcoal fire. New elements were not added to the list until recent centuries. The alchemists, using chiefly heat and charcoal, were successful in the preparation of elemental arsenic and antimony. The more difficult reduction of phosphorus was also finally achieved by these methods. Isolation of the remainder of the fairly abundant metals had to await the development of mining and metallurgy to the point where marginal ores were becoming of interest, and the development of chemical operations to the point where fairly refined separations were possible. The gaseous elements were not discovered until satisfactory methods of collecting gases were devised. The very active metals, such as potassium and sodium, could not be discovered until the work of Galvani and Volta in the field of electricity brought the chemist a new tool in the form of electrolysis. With the isolation of sodium and potassium, the discovery of calcium, magnesium, strontium, and barium naturally followed in short order. The availability of potassium made it possible for Oersted and Wöhler to prepare metallic aluminum. Preparation of this metal by an alternative procedure was not possible for half a century, until knowledge of electrolytic phenomena and the solubility behavior of aluminum compounds had advanced to the point that Hall in America and Héroult in France independently discovered the basis for the present commercial process.

The development of the spectroscope in the hands of Bunsen and Kirchhoff made possible the discovery of the elements rubidium and cesium, which are so closely related in properties to sodium and potassium. Their preparation by the electrolytic method of Davy would have been simple. Their scarcity, however, made necessary the invention of a new analytical tool before their discovery was possible. The spectroscope also made possible the discovery of a number of additional elements, including the rare gases, argon, helium, and neon. These gases had come to the attention of Cavendish more than one hundred years before. Cavendish, careful and exhaustive worker that he was, lacked both the mental and instrumental background available to Ramsay and Rayleigh a century later.

Most of the radioactive elements, likewise, had to await methods for their detection. Once the existence of radioactivity phenomena was apparent and methods for the detection of radioactivity were worked out, progress was dramatically rapid. This rapid progress once again resulted in a number of discoveries made at almost the same time in independent laboratories.

Finally, the discovery of the transuranium elements became possible only with the discovery of accelerating devices, atomic transmutation, the neutron, and artificial radioactivity. No one in his right mind would have embarked, fifty years ago, on a deliberate search for an element beyond uranium in the periodic table. Science does not progress in that fashion. It moves forward along unpredictable paths for the most part. True, a nation can set forth upon a project to develop an atomic explosive, but only after a sufficient amount of background has been laid to point to the possibility of such a development. The need for such an explosive only speeded up the search. The development, nevertheless, was inevitable, as is all scientific discovery.

Thus, we are led to conclude, as the result of an all-too-short survey, that the primary factor in bringing about scientific discovery is not necessity, or individual genius, but the relentless pressure of accumulating knowledge. Seldom is it possible to foresee all those factors that are essential to the solution of a certain problem until they have accumulated and become a part of man's scientific heritage. Once the accumulation is complete the next step becomes inevitable.

SOUND WAVES AND RHYTHMS

VERN O. KNUDSEN

Winner of the Annual Thousand Dollar AAAS Prize in 1934 for his paper on "The Absorption of Sound in Gases," Dr. Knudsen (Ph.D., Chicago, 1922) is professor of physics and dean of the Graduate Division, University of California, Los Angeles. His article is from an address presented in the symposium on "Waves and Rhythms" at the AAAS Centennial in Washington, September 13–17, 1948. Almost immediately after that meeting, Dr. Knudsen sailed for Europe to make a study of the acoustics of such famous concert halls and opera houses as those in Stockholm, Gothenburg, Oslo, Copenhagen, Amsterdam, and Milan. He will also visit the Roman theatres in southern France.

THE rhythmical patterns of sound waves in air—music, speech, and noise—surround us eternally, and we hear them from the cradle to the grave. Sometimes these rhythmical waves flow gently and sweetly over us, and gladden our hearts; sometimes they impinge harshly on our ears, and disturb our sleep or raise our blood pressure. Most of what we learn and much of what we enjoy are conveyed to us on waves of sound. It is natural, therefore, that man should be curious about these waves and rhythms, as indeed inquisitive man always has been.

Although the art of music was developed by the Chinese, Egyptians, and Hindus at least six thousand years ago, the systematic study of the rhythmical waves of music began with Pythagoras, some twenty-five hundred years ago. By means of the monochord he established the basic relations between consonant musical intervals and the ratios of simple numbers. He discovered that two segments of the monochord gave the perfect consonance of the *octave* when the ratio of the lengths of the two segments was 1 : 2, the consonant *fifth* when this ratio was 2 : 3, the *fourth* when 3 : 4. Pythagoras and his disciples maintained, with characteristic Greek logic, that "the intervals of music are rather to be judged intellectually, through numbers, than sensibly through the ear." But Aristoxenus, some two hundred years later, espoused the view that the ear is the sole and final authority for determining consonance or dissonance. The battle between these two schools continued for many centuries (it flares up even in the twentieth century); each school held the other in contempt, and, as in too many later controversies in art and science, each was more concerned with its own triumph and the bowling over of its opponent than with the finding of the truth. Although each had discovered a part of an eternal truth, modern acoustics favors the views of Aristoxenus.

We have come a long way during these past twenty-five centuries in developing and exploiting our accumulated knowledge of the vibrations, waves, and rhythms that characterize sound. Until recently, this knowledge was concerned almost exclusively with music. Bacon recognized this limitation when he said, "The nature of sound hath in some sort been enquired as far as concerneth music."

The science of sound really began with Galileo, who is much better known for his epochal discoveries in mechanics and astronomy, and for his cherished recalcitrance in heeding the edicts of cardinals and the verdict of the Inquisition. In Galileo's *Two New Sciences* (1638), he deduces the laws of vibrating strings, showing how the frequency of vibration depends upon the length, diameter, density, and tension of the string. He confirmed the findings of Pythagoras, discovered the well-known relation between pitch and frequency (which, like so many other discovered laws in nature, is only an approximation), explained resonance, and described the causes of consonance and dissonance.

Although Vitruvius (about 50 B.C.) described certain acoustical properties of theatres, such as echoes, reverberations, and interference, Kircher (1650) was probably the first to apply the principles of geometrical acoustics to the design of the shape of rooms. He anticipated the focusing effect of sound in rooms having cylindrical or elliptical ceilings, as occurs, for example, in the Salt Lake Tabernacle.

Many of the rhythmical properties of sound were discovered by Chladni's researches on the nature of the vibration of strings, rods, and plates, published in *Die Akustik,* in 1802. Almost every freshman student of physics has seen the beauty of Chladni's sand figures, which reveal the infinitely many modes of vibration of a bowed plate.

Many advances in the acoustics of the eighteenth century were made possible by the newly developed calculus. The vibrating string, especially, but also vibrating pipes and elastic fluids, became the proving ground on which such distinguished scientists as Euler, Lagrange, and D'Alembert tested new differential equations that have proved so fruitful in many branches of classical and modern physics besides acoustics.

The epochal advances in the nineteenth century are too numerous even to catalogue in this brief backward glance at the evolution of acoustics, but I cannot refrain from calling the roll of honor of such great contributors as Biot, Fourier, Savart, Ohm, Henry, Tyndall, Stokes, Kirchhoff, Lissajous, Helmholtz, and Rayleigh. Rayleigh's *Theory of Sound,* first published in 1877, and revised in 1894, is a compendium of the important acoustical works of the nineteenth century and before, but it is vastly more. It is a record of his own monumental contributions, which touched and elevated almost every branch of acoustics. It is no less than the "Principia of Acoustics." It continues to be, even in 1948, the standard book in its field. Most acoustical investigations of the twentieth century may well begin with references to what Rayleigh contributed. He treated most phases of the subject so thoroughly that many physicists of two or three decades ago felt that nothing of importance remained to be explored in the field of acoustics. There was indeed a period of low interest and activity from Helmholtz' *Tonemfindungen* (1863) and Rayleigh's *magnum opus* until the advent of the condenser microphone and the thermionic vacuum tube (about 1915) ushered in the modern era of acoustics. During this era telephony, radio, architectural acoustics, psychoacoustics, ultrasonics, and many other fundamental and technical aspects of acoustics have developed in magnitude and complexity to the extent that no one today would attempt to become an expert in all branches of acoustics. There are more than 1,400 members in the Acoustical Society of America, and during the past year the *Journal of the Acoustical Society* published six issues, numbering 875 pages.

I shall here review briefly, and in a nontechnical manner, some of the progress that has been made in recent years by certain investigations of (*a*) the propagation of sound waves through the atmosphere, and (*b*) the behavior of sound waves in rooms.

Sound waves and rhythms in the atmosphere. A few exceptional sounds, such as that due to the violent eruption of Krakatao in 1883, or to the Great Siberian Meteor of 1908, excite the entire atmosphere surrounding the earth into its resonant or normal modes of vibration. The theory of these atmospheric vibrations was first developed by Laplace (1845), and was extended by Lamb, Solberg, Taylor, and Pekeris. The period of the waves set up in the atmosphere by the explosion of Krakatao was of the order of one hour; the period of those excited by the Siberian Meteor was of an order of two or three minutes. Such slow oscillations are propagated with almost no attenuation—the subsonic aerial oscillations of Krakatao, as revealed by microbarographs in London, Berlin, Palermo, and Valencia, traveled around the globe four times. This blast continued on its eastward and westward stratospheric journeys for ten days before it spent its force.

But most familiar sounds—the whistle of a train, the peal of a church bell, or the song of a bird—are propagated at most a few miles, and soon become inaudible, too feeble to be detected by the most sensitive acoustical instruments. Such sounds are confined to small regions near their sources, and thus do not sensibly excite the normal modes of vibration of the entire atmosphere. The paths along which these sounds travel in the air are determined largely by the distribution of temperature and wind in the atmosphere. The distances they travel along these refracted paths before they are dissipated into heat depend upon the frequency of the sound, upon the temperature, humidity, viscosity, and heat conductivity of the air, and upon the nature and amount of fog and smoke in the air. Stokes and Kirchhoff calculated the attenuation of sound waves owing to viscosity and heat conductivity. Their findings indicated that such losses were negligible for most audible sounds, but that the attenuation increased with the square of the sound frequency. There are three other types of losses, however, that were not anticipated by Stokes and Kirchhoff, and these losses often are the predominant ones, especially for sounds in the audible frequency range. The most important of these three types of losses is one due to a lag of pressure, with respect to the condensation of the sound wave, which results from the transfer of energy during collisions between translating and vibrating molecules. At temperatures above about 0°C, oxygen molecules are capable of being excited into vibration as a result of molecular collisions, and collisions between oxygen and water vapor molecules are especially effective in "exciting" or "de-exciting" the vibration of the oxygen molecule. The number of vibrating oxygen molecules waxes and wanes as successive condensations and rarefactions of the sound wave pass through the air, but the process is not a reversible one, and thus a part of the energy of the sound wave is converted into heat. The attenuation is negligible at very low frequencies because the loss of sound energy during each cycle of the sound wave is very small; the attenuation is negligible at very high frequencies because the entire cycle of the sound wave occurs in a time that is short compared with the time required to establish equilibrium between the vibrating and translating oxygen molecules; but the attenuation becomes a maximum, and may be a hundred times as great as that due to viscosity, when the period of the sound wave is of the same magnitude as the time required to establish equilibrium. This source of the attenuation of sound waves was scarcely suspected, and certainly not comprehended, only twenty years ago. At that time, it was taken for granted that the velocity of sound was independent of the frequency of the sound wave, and that the degradation of the energy of the sound wave into heat depended primarily on the viscosity and heat conductivity of the air.

The large effect of temperature and humidity on the attenuation of sound in air is indicated by a comparison of the audibility of sounds in the Arctic and in a desert. The cold winter air of the Arctic is acoustically transparent; the attenuation of sound is primarily that attributable to viscosity and heat conductivity, which is very small at the lower audible frequencies. In terms of the collision theory, the vibrational heat capacity of oxygen at such low temperatures is insufficient to contribute any attenuation from energy exchanges during molecular collisions. Under such conditions, barking dogs and crowing roosters have been heard at distances of ten or more miles. In contrast, in the hot summer air of the desert, such as at Greenland Ranch, Inyo County, California, when the temperature reaches 134° F., and the relative humidity declines to 2.4 percent, the attenuation at 6,000 cycles per second is 30 db per 100 yards. Thus, a sound wave of this frequency, in such desert air, has 99.9 percent of its energy degraded into heat for each 100 yards the sound wave advances; the same sound wave in the cold Arctic air would travel at least 3,000 yards before it lost this same fraction of its energy.

The presence of fog or smoke in the air contributes further to the attenuation of sound. When the water droplets or the smoke particles are relatively large, they remain at rest with respect to the much smaller molecules of air that are forced into vibration by the sound wave, and thus sound energy is lost by the viscous drag between the suspended particles and the air. When the particles are very small (or when the frequency of the sound is sufficiently low) the particles participate in the forced vibratory movement of the medium, and thus the viscous force is diminished, or even vanishes, for sufficiently small particles. When the particles are water droplets, as in fog or clouds, there is another source of attenuation of sound that is attributed to the periodic evaporation of water from, and its recondensation on, the water droplets. This periodic evaporation and condensation follows the periodic heating and cooling of the pressure variations of the sound wave. This is a "relaxation" type of attenuation, which results from the lag of the evaporation and recondensation behind the periodic changes of temperature induced by the sound wave. It is responsible for most of the observed loss of energy of a sound wave in fog for frequencies below about 500 cycles and, according to recent measurements in our reverberation chamber, may amount to as much as 1 db per 100 yards in a dense fog. No such loss is observed in a fog of oil droplets, although at higher frequencies where the viscous forces are predominant, fogs of oil are as effective as are fogs of water for attenuating sound waves. For example, at a frequency of 6,000 cycles, a water fog having a density of 2×10^{-6} grams per cubic centimeter (visibility of about 100 feet), the attenuation was about 3 db per 100 yards, and for an oil fog of about two thirds the same density and slightly smaller particle size, the attenuation was about 4 db per 100 yards.

Thus are the waves and rhythms of sound in the free atmosphere degraded into heat. The viscosity and heat conductivity of the air, the delays involved in the exchanges of vibrational and translational energy between the colliding molecules of oxygen and water vapor, the viscous drag between suspended particles and the vibrating air surrounding these particles, and the evaporation from, and recondensation on, the water droplets of fog and clouds, all exact their toll of energy from sound waves propagated through the atmosphere.

Sound waves and rhythms in rooms. Although many of the dissipative forces just considered are operative on the waves and rhythms of sound in rooms, and in large rooms contribute significantly to the absorption of high-frequency sounds, the boundaries of these rooms are largely responsible for the character of the sounds we hear in them. The shape and size of a room, and the materials that comprise its boundaries, determine the acoustical properties of the room. Every enclosed space is an acoustical resonator, in which there is a triply infinite number of resonant frequencies. In a rectangular room, 55′ × 100′ × 20′, the lowest or gravest resonant frequency is one that corresponds to the fundamental of an organ pipe 100 feet long, closed (or open, neglecting end effects) at both ends, namely, about 5.6 cycles per second. Other resonant frequencies for this room are, in ascending order, 10.3, 11.3, 11.7, 16.9, etc. cycles per second. There are some 440 resonant modes in this room having frequencies below 110 cycles, about 440,000 below 1,100 cycles, and countless others at higher frequencies. All these resonant frequencies are excited when sounds are produced in such a room. Sometimes one or several of them may be more prominent than the others, in which case the room may have a peculiar resonance. A tiled rectangular bathroom is an example familiar to most of us. We often are deceived about the resonant quality and bigness of our voice as we test it in the bath. The room rather than our voice is the musical instrument we hear—our voice may be little more than the bellows that makes the room "speak" or "sing" at its resonant frequencies.

When a radio, violin, or any musical instrument is played in a room, that room is, in effect, a part of the instrument; we say that the instrument is *coupled* to the room. The instrument excites the normal modes of vibration of the room, and if some of these are too prominent they mar the tonal quality of the instrument as heard in that room. A high-quality radio or a world-famous Stradivarius cannot produce high-quality music in a room that has faulty resonance, or in any room that suffers from other acoustical defects.

One of the prime requirements for good acoustics in a room is the proper control of these resonant frequencies. The shape of the room should be designed so that its resonant modes of vibration are as uniformly spaced as possible; that is, so that several modes do not have the same, or even very nearly the same, frequency. A cube, for example, has the same fundamental frequency for its length, breadth, and height, and the harmonics for each of these three fundamental modes are respectively the same. In such a cubical room, especially if it is small with highly reflective boundaries, there will be prominent resonances at audible frequencies that coincide with the frequencies of these fundamental and harmonic modes. Similarly, rectangular rooms whose dimensions form ratios of 1 : 2 : 3, 2 : 3 : 4, or other ratios of small whole numbers, are more likely to have peculiar resonances than are rooms whose dimensions have incommensurate ratios.

More important than these ratios, for the control of resonance and other relevant acoustical properties of a room, are the selection and location of absorptive materials for the boundaries of the room. A small room with its floor carpeted and its ceiling lined with acoustical tile may, contrary to popular opinion, suffer from resonances. If the walls are smooth, unbroken, and highly reflective, certain low-frequency modes that vibrate tangentially to the floor and ceiling will be only slightly damped, and the room therefore may be excessively resonant and reverberant at the frequencies of these tangential modes. A room so treated may contain more absorption than is required for good acoustics; but, by reason of the improper placement of the absorptive materials, the room will have poor acoustics unless the walls have been treated so as to contribute a high degree of diffusion of sound. Absorptive materials should be distributed over the boundaries of a room in such a manner that all modes of vibration are damped approximately the same amount. This usually requires the careful selection and placement of reflective and absorptive materials for the walls as well as for the floor and ceiling of a room. In general, the materials should absorb all audible frequencies—and not merely high ones, as is characteristic of thin carpets and many commercial materials—and they will be more effective if distributed in patches, strips, or panels than if concentrated in a few large areas. When distributed in patches over the walls and ceiling, such absorptive materials not only ensure uniform damping of all the resonant frequencies of the room, but they also promote a desirable diffusion of sound. In such a room steady-state sounds are uniformly distributed, and transient sounds build up and die away in a smooth, pleasing manner, thus contributing to good acoustics.

There is reason to believe that the hearing of speech in large auditoriums may be improved by a combination of (1) acoustical treatment in which the low frequencies are absorbed more than the high ones, and (2) sound amplification in which the high frequencies are amplified more than the low ones. Most absorptive materials used for the reduction of reverberation in rooms absorb the high-frequency consonants more than they do the relatively low-frequency vowels. In such rooms the vowels die away more slowly than do the consonants, and therefore they often mask the much feebler consonants that follow the vowels.

Measurements of the hearing of speech in a reverberant auditorium revealed that 93.3 percent of the vowels were heard correctly but only 76.4 percent of the consonants were so heard. Of the total number of 955 consonant errors in these tests, 529 were final consonants, 224 were initial consonants, 117 were omissions of the called consonants, and 85 were erroneous additions of consonants that were not called. Thus, final consonants were mistaken more than twice as frequently as were initial ones. This is convincing evidence that the masking of a final consonant by the reverberation of the preceding vowel is responsible for many errors in the recognition of the sounds of speech in rooms.

It seems worth while, at any rate, to investigate the effect on the hearing of speech in a room of (1) treating the room with absorptive material that is at least two or three times as absorptive at low frequencies as it is at high ones, and (2) reproducing the speech by means of an amplifier whose gain, or amplification factor, increases about 3 or 4 db per octave throughout the frequency range from about 125 to 8,000 cycles.

There are other means for improving the hearing of speech in rooms that are suggested by the results of speech articulation tests: (1) a radical means, namely, to devise a system of speech that is free from final consonants and from other frequent sources of error; or (2) a more moderate and sensible means, namely, to train all speakers to emphasize those sounds of speech that give the greatest difficulty, especially such consonant endings as *ng*, *d*, *v*, *f*, and *th*, which are responsible for a large portion of the complaints concerning poor acoustics in rooms.

Returning to the problem of the acoustical design of rooms, the mathematical difficulties of analysis of the growth, decay, and steady-state of the triply infinite modes of vibration in a typical room are well-nigh insuperable. The methods of physical or wave acoustics are the only rigorous ones, and these have been used to calculate the behavior of a few low-frequency modes in rectangular rooms, and a few other regular shapes, when the reflective and absorptive materials are simply distributed. The formulas of physical acoustics become so complicated, however, for actual rooms in which the boundaries are nonuniform, and in which it is necessary to deal with a large number of modes, that it is not yet feasible to expect architects and engineers to use design formulas based on physical acoustics. Even those who are familiar with the mathematical complexities of physical acoustics are confronted with the most formidable tasks of computation; the calculations for a relatively simple treatment of a rectangular room would require weeks of routine labor even with the help of computing machines.

The methods of geometrical, *or ray*, acoustics avoid these complications. Although only approximate for dealing with most problems in architectural acoustics, and utterly misleading for some others, these methods, when one is guided by an understanding of their limitations and by the general principles of physical acoustics, are extremely useful. Fortunately, these methods are adequate for the acoustical designing of most rooms. Strictly, however, geometrical acoustics is applicable only when the wave lengths of the sound are small compared to the dimensions of the room; when the wave lengths are sufficiently short, the simple formulas of geometrical acoustics become good approximations to the more precise formulas of physical acoustics. For the design of rooms of moderate size, geometrical acoustics is satisfactory for frequencies above about 250 cycles, and for large auditoriums it is a good approximation to physical acoustics at all frequencies of practical interest. But for the design of small rooms, especially those that are to be used for music or for broadcasting and recording purposes, recourse to physical acoustics should be made, at least qualitatively, to the fullest possible extent. When this is done, acoustical designing becomes much more than "acoustical correction" by the application of absorptive materials to the ceilings of rooms, important though this may be in many cases for the control of reverberation and the suppression of noise.

Good acoustical designing begins with the first preliminary sketches for a building. It involves such considerations as the selection of a suitably quiet site, or, if the site is a noisy one, the use of sound-insulative constructions that will eliminate all undesirable noises; it requires the design of a room shape that, on the one hand, will avoid echoes, prominent reflections, sound foci, and room flutter, and, on the other hand, will ensure proper diffusion and the most advantageous flow of sound to all auditors in the room; it involves the selection and distribution of reflective and absorptive materials and constructions that will provide the optimum conditions for the growth, decay, and the steady-state of sound in the room; it requires, if the room is a large auditorium or theatre, the installation of a high-quality sound-amplification system; it requires inspections during construction, and tests of the completed structure, to make sure that the specified sound insulation and sound absorption have been realized; and, finally, it requires even that maintenance instructions be left with the building manager, indicating how the acoustical materials can be cleaned or redecorated, which furnishings in the building must be retained in order to maintain good acoustics, how the humidity should be controlled (in large rooms where the highest standards are desired) in order to avoid excessive absorption of high-pitched sounds, and how the sound-amplification system should be operated and maintained. If the foregoing steps are intelligently and skillfully executed in the design and construction of an auditorium, there need be no anxiety about the acoustical outcome—the acoustics of the room will be good. The waves and rhythms of sound in such a room will be undisturbed and undistorted; they will be properly enhanced and will fall gently on the ears of the auditors.

WINTER WIND

Save for a white asepsis
His unattended birth
Is marked less by the crash
Of mountains' ice,
Than silences that swell
Surround and drown
The artifacts of air and sound

An alien in an alien land,
He roams—
As restless as the mind—
The tundra flats and prairies,
Then pinnacles,
To test his might on boulders
Stranded after snow.

Soon he will call;
First, begging at your windowpane,
Then slinking to the floor.
He'll whine;
He'll roar like surf against your door.
[illegible]*y timber turned,*
He'll flee into the night.

He'll sweep the waters of the sea
And land, the sand.
He'll fracture saplings, crash down oaks
And in his frenzy spend himself
Against his sibling foe,
The glacial boulder,
Whose thrust he cannot turn.

Joseph Hirsh

THE NEANDERTHAL CONTROVERSY: NINETEENTH-CENTURY VERSION

JACOB W. GRUBER

Mr. Gruber (M.A., Oberlin, 1947) is teaching in the Department of Sociology at Temple University while working for his doctorate in anthropology at the University of Pennsylvania. He has also taught at New York University.

IN 1848, just a century ago, a human cranium of unusual visage was unearthed during quarrying operations on the Rock of Gibraltar. Until then, no recent man had ever looked upon the face of his Ice Age predecessors—the Neanderthaloids—that massive-browed and flat-headed race of men who roamed the continent of Europe as its only inhabitants, when the last of the glaciers first poked its icy lobes toward the Mediterranean. In the long span of geologic history, a century is not a very long time; but measured in terms of scientific accomplishment the past hundred years encompass almost the entire history of human paleontology.

In that hundred years, the Neanderthaloids have not only provided the most numerous remains of Pleistocene man, but they have, in addition, presented the searchers for human origins with some of their knottiest problems. Having as their habitat the area in which modern Western civilization developed, they have been the subject of some of the most violent controversies in human paleontology to determine whether such a brutish form could be ancestral to today's rulers of the universe. Their true place in the evolution of man has never been completely established. Do they represent the direct ancestor of modern man, or merely a distant cousin who dropped off the line of human evolution at some far-off time in the past? Do they represent specimens of normal individuals, or are they only pathological deviations from the normal stock? Are they a race, a species, or genus of man, or are they man at all? These are the questions inherent in the Neanderthal form and his distribution. As newer discoveries have been made, as more and more information has come to light concerning Pleistocene man, the problem of the Neanderthaloids becomes more and more confused. Today, particularly, with the apparent substantiation of a *sapiens* type in Europe prior to the Neanderthaloids, the problem of the place of Neanderthal man in human evolution requires a re-examination, a re-examination unclouded by strongly held tenets of another day. Almost lost in the present confusion regarding the true nature of this form is the bitter scientific controversy that raged over the first specimens. Perhaps it is not amiss, under such circumstances, to re-create certain aspects of that debate and the scientific atmosphere under which it was waged.

With imaginations and intellects chained by too-literal interpretations of the Scriptural record, which maintained a recency of 6,000 years for the creation of man, the participants in the burgeoning sciences of the eighteenth and early nineteenth centuries were unable even to conceive of earlier forms of man in either a chronological or a morphological context. Certainly, the brilliant though cautious Cuvier, acknowledged master of the natural scientists a century and a half ago, contributed greatly to the limitations, intellectual and otherwise, imposed upon those who sought for the origin of man in a remote antiquity. Revolutionary and radical in the fields of comparative anatomy and tertiary paleontology, but extremely conservative regarding questions of man's origin, he reiterated time and time again the religio-scientific dogma of man's recent creation and the improbability, if not impossibility, of ever finding his remains earlier than the most recent times. Cuvier's fame and unquestioned skill in his own field strengthened the doubts and skepticism that permeated the intellectual atmosphere of the first half of the nineteenth century, an atmosphere so clouded with a priori judgments as to condemn, almost without hearing, conclusions based on scientific investigations of undoubted merit that tended to imply that man coexisted with extinct glacial fauna.

As early as 1797, John Frere addressed a short communication to the Society of Antiquaries of London, in which, after summarizing his find of some flints, obviously fashioned by man in a stratum underlying the bones of extinct fauna, he very cautiously suggested that "the situation in which these weapons were found may tempt us to refer them to a very remote period indeed; even beyond that of the present world." Frere's suggestion, however, was heard only to be forgotten. It was not until almost half a century later that increasing evidence, supplied during the intervening years primarily by the unrecognized labors of such pioneers as MacEnery in England, Schmerling in Belgium, and, above all, Boucher de Perthes in France, made it obvious and intellectually necessary to accept, though in some quarters reluctantly, Frere's views as to the existence of man during glacial times. So overwhelming had the evidence become that in 1859, only twelve years after Boucher de Perthes had set forth the results of his investigations at Abbeville in his celebrated, but scathingly reviewed, *Antiquites Celtiques,* Sir Charles Lyell in a major scientific address could say:

> The facts, however, recently brought to light during the systematic investigations, as reported on by Falconer, of the Brixham Caves, must, I think, have prepared you to admit that scepticism in regard to the cave evidence in favor of the antiquity of man had previously been pushed to an extreme. To escape from what I now consider was a legitimate deduction from the facts already accumulated, we were obliged to resort to hypotheses requiring great changes in the relative levels and drainage of valleys, and, in short, the whole physical geography of the respective regions where the caves are situated—changes that alone would imply a remote antiquity for the human fossil remains, and make it probable that man was old enough to have co-existed, at least, with the Siberian mammoth.

The dramatic change in scientific attitudes respecting the age of man is more vividly expressed in an editorial in the *Geologist* of August 1862, in which S. J. Mackie very candidly remarks:

> When the antiquity of man was first proclaimed from the discovery of the Abbeville flints by Boucher de Perthes, no one believed it. Everybody thought him like the mad man who swore all the world was mad; and so it seemed, then, as if all the world had mental obliquity of vision, which made them declare our *savant* of Abbeville to be labouring under a delusion. When, however, Rigollet, Prestwich, Flower, Lyell, Evans, and others of the goodly company of geologists,—as unbelieving, however, as so many St. Thomases,—went, saw, and returned believing, the fame of Boucher de Perthes' discoveries gained ground. Some there were who hardened themselves in their unbelief, and hazarded wild theories of ocean-waves chipping out artificial forms, and of recent objects sinking down in the ground, and burying themselves, and other equally untenable notions, but these waxed fewer and fewer, not by dying out, but by becoming converts to the novel truths.

Lyell and Mackie, however, represented the "radical" converts to the conclusions of Boucher de Perthes. More in keeping with the thought of the times was the opinion expressed by Prestwich before the Royal Society of London in 1859, an opinion which is curiously akin to those expressed not too long ago by Americanists convinced of the recency of man in the New World. Prestwich

> does not, however, consider that the facts, as they at present stand, of necessity carry back man in past time more than they bring forward the great extinct mammals towards our own times, the evidence having reference only to relative and not to absolute time; and he is of the opinion that many of the later geological changes may have been sudden or of shorter duration than generally considered. In fact, from the evidence here exhibited, and from all that he knows regarding drift phenomena generally, the author sees no reason against the conclusion that the period of man and the extinct mammals—supposing their contemporaneity to be proved—was brought to a sudden end by a temporary inundation of the land; on the contrary, he sees much to support such a view on purely geological considerations.

Shades of Cuvier and his catastrophism!

It was in such a period of doubt and skepticism, of biblical injunctions and science, that the first evidence other than cultural remains of Pleistocene, or glacial, man, the Gibraltar skull, was brought to light. Understandably enough, the notions prevailing a decade before the *Origin of Species*, concerning man's place in nature, served to deprive the find of its proper importance and to hide its true meaning for the development of man. Little is known, therefore, of the early history of the Gibraltar skull, and much of that is due to the later investigations of Sir Arthur Keith.

The find is documented by an entry in the minutes of the Gibraltar Scientific Society, dated March 3, 1848, which merely reads "Presented a Human Skull from Forbes Quarry, North Front, by the Secretary." Its history from 1848 until it was brought to England sixteen years later is unknown. By the time the Gibraltar skull was brought to the notice of English paleontologists in the mid-1860s, a peculiar skull from the Neander Valley of Germany had already been the center of a raging controversy for half a dozen years and, overshadowed by what was later to be proved a fellow, the Gibraltar skull was forgotten again.

Recovered in August 1856 by Fuhlrott, the Neanderthal skull was first brought to the attention of a scientific body by Professor D. Schaaffhausen in a report read to a meeting of the Lower Rhine Medical and Natural History Society at Bonn on February 4, 1857. It was in the course of this paper, published in 1858, that Schaaffhausen gave the first, and one of the few, complete anatomical descriptions of the skeleton, along with the circumstances of its accidental discovery and preservation.

Workers, digging through unstratified loam deposits in a cave opening on the cliff face of the Neander Valley near Dusseldorf, unearthed a group of bones which they thought to be nonhuman. Through the fortunate appearance of Dr. Fuhlrott, who recognized the true nature of the remains before they had been completely discarded, a large portion of the skeleton was preserved. The parts extant included primarily the skullcap, which had apparently been broken from the base of the skull at the time of disinterment, and several of the long bones. The left humerus was much slenderer than the right, and the coronoid process of the left ulna, or that portion which articulated with the humerus, was so much enlarged by bony growth as to preclude the possibility of the flexure of the elbow beyond a right angle; the anterior fossa of the humerus was filled with the same bony growth. Despite the fact that the remains were some four or five feet below the surface of the cave, no faunal remains or artifacts were found by which an approximate dating could be made.

In his detailed report on the form, Schaaffhausen concluded:

> (1) That the extraordinary form of the skull was due to natural conformation hitherto not known to exist, even in the most barbarous races; (2) That these remarkable human remains belonged to a period antecedent to the time of the Celts and the Germans, and were in all probability derived from one of the wild races of Northwestern Europe, spoken of by the Latin writers; and which were encountered as autocthones by the German immigrants; (3) That it was beyond doubt that these human relics were traceable to a period at which the latest animals of the diluvium still existed; but that no proof in support of this assumption, nor consequently of their *fossil* condition, was afforded by the circumstances under which the bones were discovered.

Schaaffhausen in his anatomical description of the remains remarked on the enlarged eyebrow ridges, the low, narrow forehead, and the excessive bony growth on the occiput or the occipital torus. He compared the skullcap with Neolithic and Roman remains that had been unearthed in Europe and concluded that, although in its particular characters it surpassed any specimen then known, generalized resemblances were so great that the Neanderthal man must be considered human and normal. He reasoned:

> Sufficient grounds exist for the assumption that man coexisted with the animals found in the *diluvium*; and many a barbarous race may, before all historical time, have disappeared, together with the animals of the ancient world, whilst the races whose organization is improved have continued the genus. . . .
>
> There is no reason whatever for regarding the unusual development of the frontal sinuses in the remarkable skull from the Neanderthal as an individual or pathological deformity; it is unquestionably a typical race character, and is physiologically connected with the uncommon thickness of the outer bones of the skeleton, which exceeds by about one-half the usual proportions.

The article of Schaaffhausen as it was originally published evoked little interest outside Germany. By the time of its translation by George Busk and republication in the *Natural History Review* in 1861, however, Darwin's *Origin of Species* was already two years old, and some members of the scientific world were ready to seize upon any specimen that by virtue of its morphological characters would tend to support the principle of transmutation or evolution. As the advocates of Darwin's doctrine were determined to support it by evidence of more ancient and morphologically more primitive forms of mankind, so were the opponents of the principle of evolution determined to discredit any such evidence. The appearance of the Neanderthal specimen gave both proponents and opponents the opportunity for which they had been waiting. It gave them the opportunity of shifting the argument from the relatively static realm of the lower animals to the stage of man himself. Almost every anatomist and paleontologist of any consequence, and several of no consequence whatsoever, felt constrained to add something to the discussion. A review of the writers on the Neanderthal specimen in the decade after Schaaffhausen's publication appeared reads like a *Who's Who* in the anatomical sciences of the middle nineteenth century. The controversy over the real nature of the skull raged through the pages of the scientific journals and on the rostrums of the scientific meetings for many years, concluding only when new discoveries settled the question.

The arguments ranged themselves between two conflicting extremes: (1) the specimen, despite its peculiarities, was that of a normal human; and (2) the specimen, because of its peculiarities, was that of a pathological individual. Generally the followers of the new doctrine of Darwin championed the former view and the opponents of the evolutionary thesis, the latter.

Huxley, the defender and protector of the shy and unassuming Darwin, after a study of a cast of the skullcap, stated emphatically:

> The Neanderthal skull exhibits the lowest type of human cranium at present known, so far as it presents certain pithecoid characters in a more exaggerated form than any other; but . . . inasmuch as a complete series of gradations can be found, among recent human skulls, between it and the best developed forms, there is no ground for separating its possessor specifically, still less generically, from *Homo sapiens*. At present we have no sufficient warranty for declaring it to be either the type of a distinct race, or a member of an existing one; nor do the anatomical characters of the skull justify any conclusion as to the age to which it belongs.

William King, anatomist at Ireland's Queen's University, however, though agreeing with Huxley that the skull was that of a normal individual, could not agree with its putative affinity to modern man. He saw, rather, a much greater resemblance to the apes, so great in fact that the form deserved a separate specific designation—*Homo neanderthalensis,* the first recorded use of that term for this form. (King later recanted and stated that the name is not applicable since the fossil is not only specifically but also generically distinct from man.)

King's disagreement with Huxley was a minor one compared with that of the proponents of the pathological view. C. Carter Blake, Honorary Secretary of the Anthropological Society of London, was one of the first to express this view when, writing in the *Geologist* in 1862, he maintained:

> All these characters [pathology of the left arm, barrel type of rib box, etc.] are compatible with the Neanderthal skeleton having belonged to some poor idiot or hermit, who died in the cave where his remains have been found. They are incompatible with the evidences which might be left in a Westphalian bone-cave of the remains of a normal healthy uninjured being of the *Homo sapiens* of Linnaeus.

Ten years later, after finally receiving the opportunity of examining the original specimen, Virchow, the great German anatomist, emphatically reaffirmed the pathological nature of the specimen. The most imaginative of the "idiotic" interpretations, however, was that put forth by the German anatomist Mayer in 1864. Decrying the emphasis of the English disputants on the supraorbital torus, he pointed out that this is not an unusual feature in modern man, that, in fact, he "had a classmate, who had a brilliant talent for theosophic studies, in whom the eyebrows sprang out far over the pious face." His interpretation was that the skeleton represents an individual who was afflicted with rickets, evidenced by the pathology of the left arm, and that this condition inflicted so much pain that from childhood on the forehead was puckered in a perpetual frown, resulting in the large supraorbital torus. Furthermore, he insisted, the bowed femurs indicated a horseman, and the skull was more like that of a Mongol than that of an ape, gorilla, or New Zealander. Assembling these deductions, Mayer concluded that the skeleton was that of a rickety Mongolian Cossack belonging to one of the hordes driven by Russia, through Germany, into France in 1814. If not meeting the specifications of scientific objectivity, Mayer's explanation was at least a good story.

And so the argument raged until with the discovery of other specimens with the same physical peculiarities, particularly the two skeletons found in the Spy Caverns of Belgium in 1886, directly associated with glacial fauna, both the antiquity and distinctness of Neanderthal man were settled to virtually everyone's satisfaction.

As for the Gibraltar skull, which not until 1906 was definitely recognized as a Neanderthaloid, it made one brief appearance at the meeting of the British Association at Bath in 1864, after which it was consigned to almost half a century of anonymity in the Museum of the Royal College of Surgeons.

George Busk reported in the *Reader* newspaper of July 23, 1864, that he had received the skull from Captain Brome, of Gibraltar. He noted resemblances to the Neanderthal skull and prepared, with Hugh Falconer, an English paleontologist, to display the cranium before the British Association. Falconer was adamant in his opinion that the skull was human, writing to a relative:

> If you hear any remarks made, you may say from me, that I do not regard this priscan pithecoid man as the "missing link," so to speak. It is a case of a very low type of humanity—very low and savage, and of extreme antiquity—but still man, and not a half way step between man and monkey.

He recognized, however, its distinctness from modern man and in a letter to Busk regarding the display of the specimen suggested the name *Homo Calpicus,* from "Calpe," the ancient name for Gibraltar. The skull was duly displayed, and Busk made a few remarks concerning it. There seemed, however, little tendency to link the form to the controversial Neanderthal skull.

The fate of the Gibraltar discovery and the controversy surrounding its more widely known fossil colleague from the Neanderthal, though small episodes in only one current of the science of a century ago, represent a recurrent phenomenon—and problem—in the development of science. The pat answers of Cuvier and the religio-scientific dogmas surrounding the search for man's origins are almost forgotten in our own enlightenment. But it is sometimes disconcerting to find the Cuviers, the Mayers, and the Virchows—all as equally devoted to the methods and objectives of modern science as any of today's scientists—with intellects imprisoned and imaginations shackled by hypotheses of their own making, hypotheses purportedly based on fact and uninfluenced by metaphysical considerations. It is disconcerting to realize that as their intellects were shaped and limited by the dogmas—often scientific—of their day, so may the intellect of the modern investigator be shaped by the a priori judgments of his time, the unproved hypotheses and overgeneralizations, the result either of the nonscientific environment in which he lives and works or of the sometimes equally nonscientific traditions he follows. One sometimes wonders how many "rickety Mongolian Cossacks" exist in the controversies and conclusions, the debates and deductions, of today.

SCIENCE ON THE MARCH

EFFECT OF 2,4-D ON YIELDS OF OLEORESIN FROM SLASH AND LONGLEAF PINES

FOR over three hundred years the standard practice in the gum naval stores industry in the United States has been to cut wounds on pine trees at frequent intervals—usually weekly—to maintain a flow of oleoresin. Recent work with chemical reagents, however, has demonstrated that the flow of oleoresin from one wound can be increased and prolonged by treatment with suitable chemicals. In 1933, Solodki and Vasskouskaya[1] first reported increased yields of oleoresin resulting from the application of various chemicals to wounds on Scots Pine (*Pinus sylvestris* L.) in Russia. Later work by the Forest Service, U. S. Department of Agriculture, in Florida,[2,3,4] resulted in practical methods of treating wounds on slash pine (*P. caribaea* Mor.) using a 40 percent solution of sulfuric acid and on longleaf pine (*P. palustris* Mill.) using a 60 percent solution.

The sulfuric acid treatment has been used to advantage on commercial operations in two different ways. The treatment can be used at *weekly* intervals for one or two years, with an increase of about two thirds in the total yield of oleoresin from a given number of trees. The treatment also can be used at *biweekly* intervals on the same number of trees for at least five years, with resulting yields equal to those obtained with weekly wounding and no chemical treatment. The biweekly schedule for

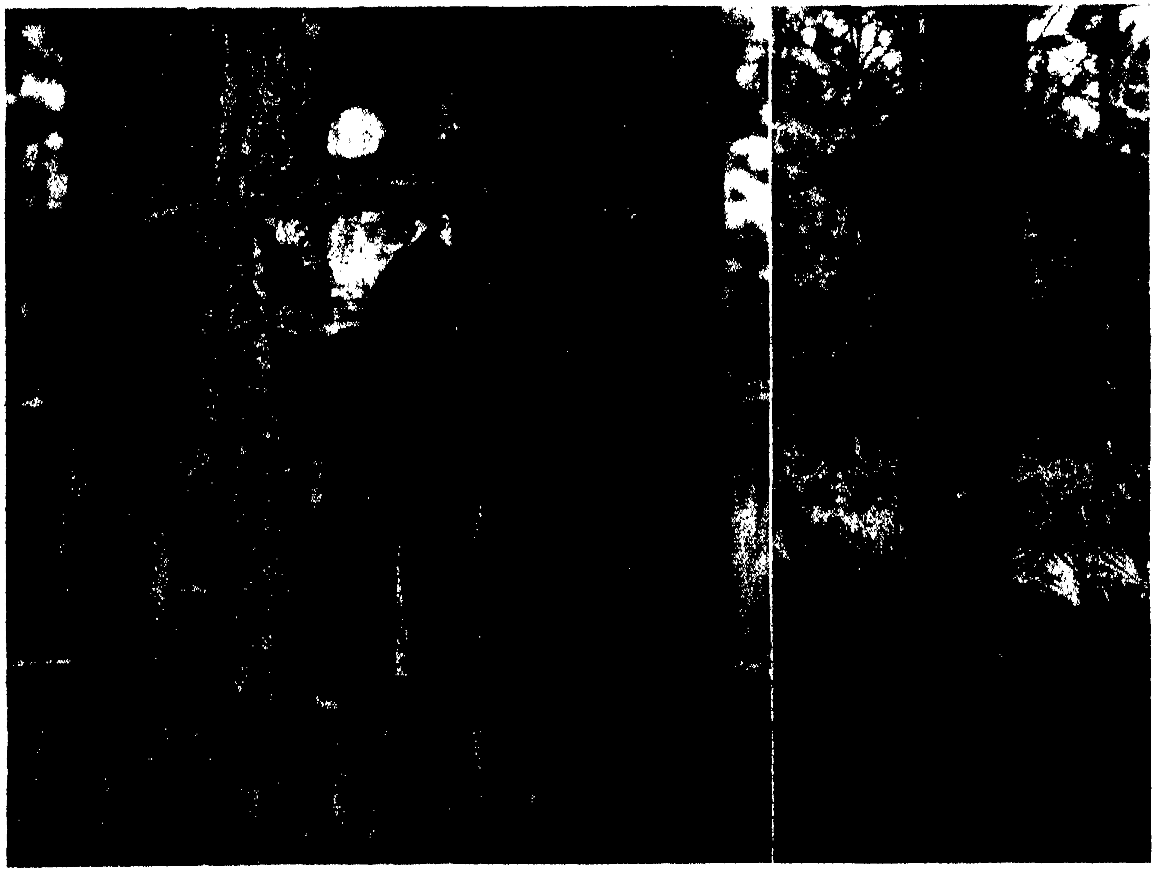


Fig. 1. Punch-wound method used in experimental work for screening various chemical solutions for their effectiveness in increasing and prolonging gum flow from Southern pine trees. Final tests of promising chemicals are made using the new commercial method of chipping shown in Figure 3. *A*, close-up of punch wound; *B*, tree with 24 punch wounds.

wounding and treating makes possible an increase of about 60 percent in production of oleoresin per man-day of wounding or "chipping" labor over that obtained by weekly chipping without treatment.

Although sulfuric acid is the best of the chemicals that have been fully tested for naval stores use, it has the disadvantage of being corrosive. Numerous chemicals have been tested in the search for a noncorrosive and harmless substance to replace sulfuric acid. One of the more promising is 2,4 dichlorophenoxyacetic acid.[5]

In a preliminary test made in 1947 on slash pine in northern Florida, several commercial formulations of 2,4-D were compared with respect to their ability to increase the flow of oleoresin when applied to fresh wounds. The procedure used in this test[6] was to make a series of 16 untreated, cambium-depth, circular wounds 0.75 inch in diameter in the bark of each of 4 trees. Oleoresin was collected and measured volumetrically in glass vials suspended on the tree below each wound. Wounds were divided into four yield groups of 16 wounds each on the basis of oleoresin yields from these untreated woundings. The 16 treatments as listed in Table 1 were assigned at random to the wound locations within each yield group. Each treatment was replicated four times and the check sixteen times. Rewounding and treating was done at biweekly intervals. Treatment consisted of wetting the wound completely with a solution applied as a spray and allowing the excess to drain off. Results in Table 1 show that the 8 percent solution of 2,4-D applied as the morpholine salt was the best treatment in this comparison. The punch-wound technique used in this test greatly magnifies the differences in yields between treated and untreated wounds (Figs. 1, 2, 3), and hence the results are not comparable to those that would be obtained on commercial-sized wounds.

TABLE 1

Mean Yields of Oleoresin from Punch Wounds on Slash Pine Rewounded and Treated with a Formulation of 2,4-D at Biweekly Intervals for 6 Weeks

2,4-D Formulation	Concentration (Percentage of Free 2,4-D Acid)	Mean Yields[1] for Three 2-Week Periods	
		ml	Percentage of Check
Morpholine salt . .	16	39.9	603
	8	39.9	603
	4	27.3	412
Sodium salt	4	26.3	398
	2	22.2	335
	1	15.4	233
Methyl ester	16	17.2	259
	8	21.2	321
	4	12.5	189
Amyl ester	16	18.7	283
	8	14.5	219
	4	13.4	203
Check	0	6.6	100

[1] Least significant difference between treatments: 5.8 ml at 5% level.

A second test was made on commercial-sized wounds, or streaks, to compare yields of oleoresin from the best treatments of the previous test with yields from the biweekly sulfuric acid treatment and from the conventional untreated streaks.

TABLE 2

Mean Biweekly Yields of Oleoresin from Commercial-sized Wounds on Slash Pine and Longleaf Pine

Treatment[1]	Mean Biweekly Yield per Tree[2]			
	Slash Pine		Longleaf Pine	
	gm	Percentage of Check	gm	Percentage of Check
Check	190	100	151	100
40% H_2SO_4	168	88	. . .	. . .
60% H_2SO_4	. . .	. .	236	151
8% 2,4-D as morpholine salt	187	98	161	107
4% 2,4-D as morpholine salt	136	72	130	86

[1] Check trees were wounded at *weekly* intervals. All others were wounded and treated at *biweekly* intervals.

[2] Slash pine yields are means from four 2-week periods, and longleaf pine yields are means from three 2-week periods. Least significant differences are: slash pine, 8.0 gm at 5% level; longleaf pine, 14.6 gm at 5% level.

Trees were divided into 16 yield groups of 4 trees each on the basis of oleoresin yields from untreated, cambium-depth streaks, 0.5 inch high, and equal in width to about one third the circumference of the tree. Treatments listed in Table 2 were randomized within each yield group.

Results in Table 2 show that on slash pine the 8 percent solution of 2,4-D applied to four successive streaks at biweekly intervals resulted in a yield of oleoresin 10 percent greater than a corresponding number of sulfuric acid treatments. On longleaf pine, however, yields from three biweekly streaks treated with sulfuric acid were about 40 percent greater than with the 2,4-D treatment. When yields from the biweekly 2,4-D treatment are compared with those from the weekly untreated streaks, no significant differences are observed with either species. Hence, during the test period, one

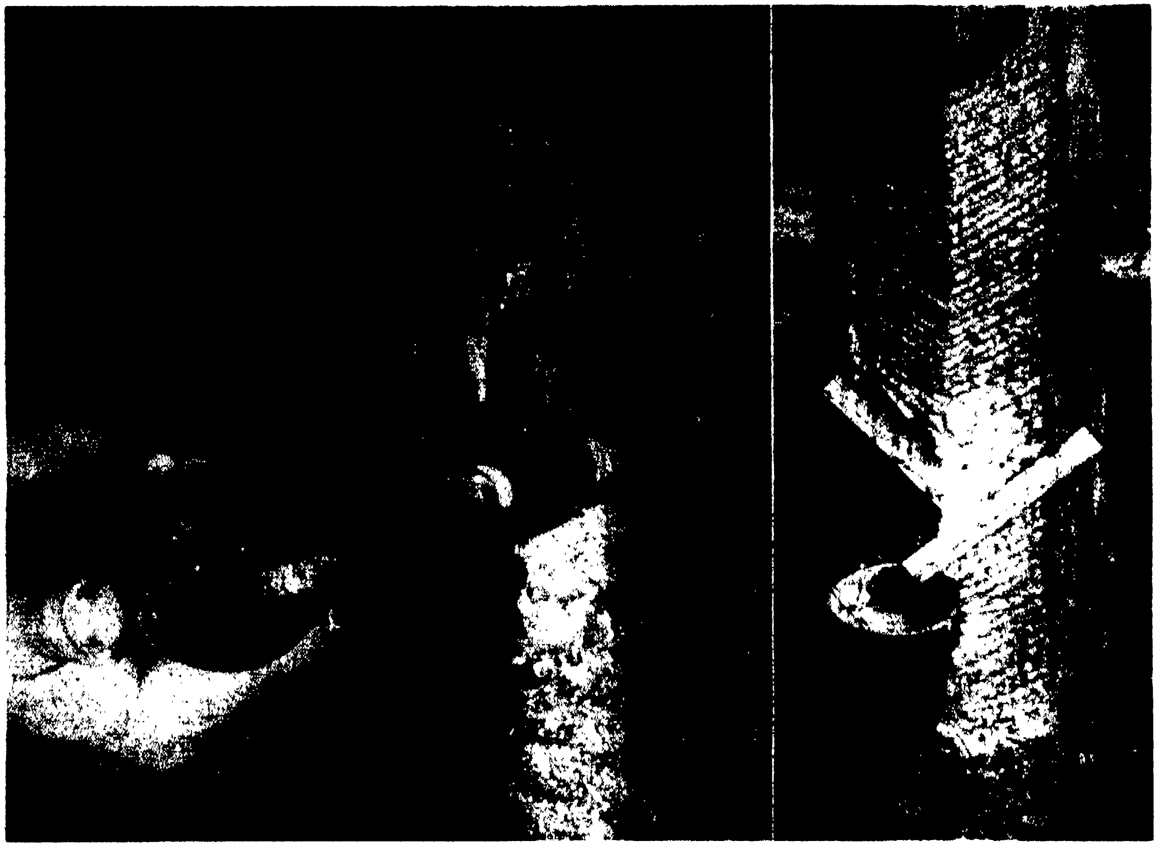


Fig. 2. The old commercial method of chipping a streak. Streaks are made weekly and extend 1/2 inch high and 1/2 inch radially into the wood. No chemical treatment is used. *A*, close-up of old-style streak; *B*, face of tree after being worked for two years.

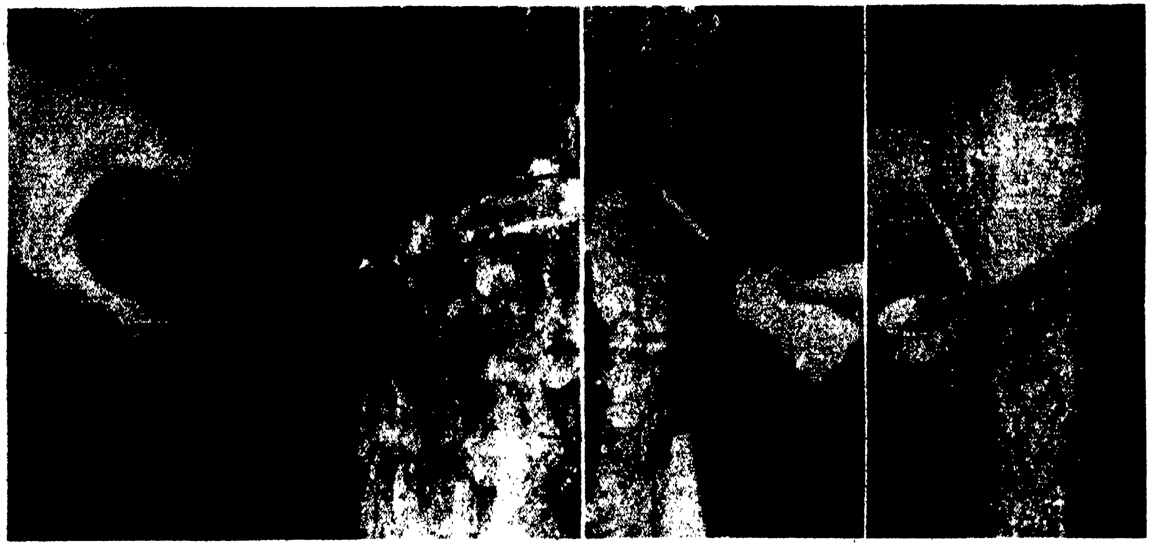


Fig. 3. With the new commercial method, only the bark is removed when chipping a streak, as in *A*. *B*, chemical solutions (plastic spray bottle) sprayed on the streak cause an increase and prolongation of gum flow over that obtained with the old wood-chipping method; *C*, face of a bark-chipped tree is smooth, and the tree is not weakened by cutting into the wood.

streak treated with 2,4-D yielded as much oleoresin as two untreated streaks. This increase in yield with 2,4-D is as great as that usually obtained with sulfuric acid on commercial operations. Inconsistencies in this experiment are that the sulfuric acid treatment on longleaf pine resulted in a yield considerably higher, and on slash pine somewhat lower, than the average yields obtained on commercial operations with this reagent.

Application of an 8 percent solution of 2,4-D as the morpholine salt to cambium-depth wounds on pine trees resulted in a certain amount of killing in the phloem and cambium tissues above the wound. The killing was localized, but the phloem tissues in the vicinity of the cork cambium appeared to be much more susceptible to the lethal effects of 2,4-D than the phloem tissues near the vascular cambium. In the cork cambium, killing extended to a mean height of 60 mm above the last streaks in slash pine and 90 mm in longleaf pine. Killing in the vascular cambium was only 27 mm in slash pine and 34 mm in longleaf pine. The relationship between this killing and the increased gum yields is not yet known.

The research is being continued to determine commercial applicability. A full-season test running 32 weeks will be necessary to show whether or not 2,4-D will be effective with repeated applications, and whether it will seriously damage the trees.

REFERENCES

1. Solodki, T. H., and Vasskouskaya, T. *Forest Chemical Industry*. 1933. **2**, 36–43.
2. Liefeld, T. A. *Naval Stores Rev.* 1942, **52**, 8.
3. Ostrom, C. E. *Chemurgic Digest*. 1945, **4**, 240–3.
4. Snow, A. G., Jr. Southern Forest Expt. Sta., Occasional Paper No. 106. New Orleans, La. Sept. 1944.
5. Ostrom, C. E., and Schopmeyer, C. S. U. S. Patent No. 2,435,724. Feb. 10, 1948.
6. Ostrom, C. E., and True, R. P. *Naval Stores Rev* 1946, **56**, 16, 26.

C. S. Schopmeyer

Southeastern Experiment Station
USDA
Lake City, Florida

MODERN CLAY INVESTIGATIONS

CLAY is a universal earth material. Few other geologic products have caused as much annoyance—even disaster—or given as much comfort and pleasure throughout human history as clay in its numerous varieties and products. It is an indispensable raw material in many types of modern industry. Ancient races, also, found it most useful for various arts and crafts; its importance in the preservation of invaluable archeologic records of remote antiquity can scarcely be overemphasized.

To many intelligent and educated people, even in this age of pure and applied science, clay is only a form of common "dirt." They know little or nothing about its usefulness to our modern way of life. Others understand somewhat vaguely that clay is an ingredient of brick, tile, pottery, and chinaware, but very few realize that it has extremely valuable uses in other fields, such as the oil, rubber, paper, and metal industries.

Aside from water, and possibly stone and flint for tools and weapons, clay may have been the geologic resource most widely used by ancient peoples. Bricks, tablets, and vessels made of clay are among the relics of prehistoric tribes and nations. With the advance of modern civilization, involving steadily increasing dependence upon geologic raw materials, clay of various qualities came to be more extensively used. Much of the progress in the use of clay in the ceramic arts was made with only a relatively slight knowledge of its fundamental properties. Empirical testing had long been the rule in attempting to use new clays or in making new uses of familiar clays. It is still important.

Physical and chemical sciences have for some decades been concerned with attempts to discover the microscopic and ultramicroscopic characteristics of raw materials. Such precise and painstaking studies in several laboratories throughout the nation have given us a true knowledge of the real composition and structure of clays and, therefore, of their characteristic properties. We are now beginning to understand why clays "behave" as they do—some in one manner, others in another. These recent investigations have been made with the precision tools of modern microscopic and X-ray research now at the command of investigators of ultrafine materials. Combined with new insights into the applicable principles of modern chemistry and physics, new explorable frontiers have come rapidly into view.

Many scientists, outside the particular fields in which the data are studied or used, are more or less unaware of the march of science in the expanding field of clay research. Some of the discoveries during the past two decades have been revolutionary. These notable advances have been made chiefly through the coordinated work of a relatively few petrologists, mineralogists, chemists, and physicists. Soil scientists have contributed much to present knowledge, as have ceramists. The scientific and economic importance of these discoveries in the realms of geology, soil science, engineering, and specialized industries has been discussed chiefly in technical periodicals.

The word "clay" once denoted any extremely fine-grained plastic natural material, without giving any clear idea of its composition and structure. "Clay material" has recently been introduced as the general term, inasmuch as certain geologic materials having many of the basic characteristics of "clay" are not now generally considered to be clay in an accurate sense. The newer term includes clay, shale, certain soils, and similar geologic materials. All are composed of very tiny grains of mineral particles which, in the aggregate, have plastic properties when mixed with water. Clay is now known to be not amorphous but composed of crystalline particles. The mineral grains are dominantly hydrous aluminum silicates; some are hydrous magnesium or iron silicates. It was a momentous scientific discovery, made by means of X-ray diffraction analysis, in 1923–24, to determine that clays are composed essentially of crystalline grains. This basic fact not only altered the philosophic consideration of clays in their manifold natural relations but gave new insight into their practical utilization.

This significant step far beyond the previous horizon of known facts, or interpretations based on assumed facts, was soon implemented further and supplemented by the discoveries of a few optical mineralogists and petrographers. These investigators have become indefatigable students of the fundamental composition and structure of varieties of clay, or of distinct clay minerals. The few clay minerals that are now generally recognized have as much significance in the understanding and use of clays as do other minerals that have long been more widely known and studied. Each clay mineral group has been given an appropriate name, such as kaolinite, montmorillonite, and illite. The clays of each group have definite, specific properties, a knowledge of which is essential in understanding soils and in determining engineering and industrial procedures.

The modern method of studying clay usually begins with a determination of the kind of clay minerals that compose it by using X-ray, thermal, or optical microscopic methods. Sometimes one of these methods is adequate, but numerous clays are so complex that all methods are required to identify their components. Following this procedure, the character of the nonclay mineral components, including water-soluble salts and exchangeable bases, is determined. Finally, the particle size of the individual mineral grains is studied. All these data permit the investigator to determine the reason why a given clay possesses its own peculiar properties: for example, why one clay is resistant to heat, whereas another is not; why one clay is satisfactory for compounding with rubber, and another is not, and so on.

The major objective of modern clay investigations is to determine the factors controlling the properties of clays. This is of great value in selecting the best clays for particular industrial uses and in devising the best and most economical means of processing clays in manufacturing operations. In some instances such knowledge is of paramount importance, as is shown by this illustration: engineers frequently find it necessary to build a structure on foundation material that may be largely clay whose properties are very detrimental. The clay, for example, may be too plastic, and may tend to flow under the weight of the structure that is to be put on it. If the engineer understands the causes of these detrimental properties, he may be able to treat the clay so that the characteristics are changed. The precise investigation of raw clays for diverse and manifold uses in modern industry is a long story in itself.

Investigations in soil science have shown also that many of the properties of soils, including physical characteristics and the availability of plant nutrients, are intimately related to the properties of the constituent clay minerals. Soils consist essentially of minute crystalline particles of one or more clay minerals plus variable amounts of quartz and other nonclay minerals of sand or silt size. Permeability, water adsorption, plasticity, and compaction are related to the clay constituents of soils. Recently the outstanding discovery has been made that the exchangeable bases in soils have an important control upon the physical properties. In brief, whether the base is sodium or calcium is significant. The essentiality of water absorbed by clay particles is obvious. The water hull that surrounds the tiny clay flakes also plays an important part.

The geologic setting of any soil, especially in terms of the soil profile developed on surficial materials of widely diverse geologic origins, is of critical importance, both for soils as nourishers of plants and as engineering materials. From the standpoint of plant growth, clay controls the general physical and chemical properties of soils, as well as their tilth, acidity, moisture, and supply of water and bases.

ARTHUR BEVAN

State Geological Survey
Urbana, Illinois

BOOK REVIEWS

PLANT SCIENCE

Botanik der Gegenwart und Vorzeit in Culturhistorische Entwickelung. Karl F. W. Jessen (1864). xx + 495 pp. $6.00. Chronica Botanica. Waltham, Mass.

JESSEN dedicated his *Botanik der Gegenwart und Vorzeit* to two men: G. Gervinus, student of the evolution of mankind; and Ernst Meyer, student of the history of botany. In his preface Jessen pays high tribute to Gervinus as the man who first gave him an insight into the meaning of history as a record of human achievement and of the factors affecting the evolution of the human race; and to Meyer as the man who so successfully applied the methods of the research historian to the field of plant science: a historian with the insight of the biologist and a biologist with the insight of the historian.

The book is the first in a series of offset reprints of out-of-print and classic scientific works, edited by Frans Verdoorn and published by the Chronica Botanica Company. Although written in very readable German, it is to be hoped that it may eventually be translated into English in order to make it easily comprehensible to all students and teachers of the plant sciences in English-speaking countries. In fact, so valuable and stimulating is it that one might wish that it be translated into other languages also. But all serious students of the plant sciences will welcome this replica of the original German, and no botanical library will be complete without it.

It is difficult to review Jessen's book because the reader is likely to become so deeply engrossed in it that he forgets to be critical. There probably will be general agreement with Verdoorn's statement, opposite the title page of the reprint, that "From the point of view of the historian of science, Jessen's book is unique for its cultural approach." Dr. Verdoorn writes also: "I hope that the result of his labors embodied in this reprint, the first volume of our new serial *Pallas*, will be useful and stimulating, not only to the few professional biological historians but to many biologists with historical and humanistic interests." This hope will certainly be realized by all those readers who have a deep interest in plant science and its evolution. The book really is unique in the richness of the cultural background: it is not merely a history of botany; it is a part of history itself. And yet it is a very useful reference book also, thanks to the detailed table of contents and the index.

The period covered is from earliest times to about 1860, the preface to the original being dated November 15, 1863. The book is divided into twenty chapters, including one of about 9 pages dealing with plant knowledge of earliest antiquity, one of about 45 pages on Greece, a shorter chapter on Rome, one on the Eastern Christian world, and another on the Western Christian world. There follow chapters on the Arabs; The Beginnings of Western Science; Universities in the West; The Founding of Western Natural Science; Popular Cultural Development; Special Botanical Studies after Dioscorides; The Beginnings of Systematic Botany under the Influence of the Classicists; The Reformation of Natural Science; The Discoveries of the Seventeenth Century; Agriculture and Horticulture into the Eighteenth Century; Taxonomy from Jung to Linné; followed by chapters on Taxonomy under the influence of Linné and Jussieu; Anatomy and Physiology in the Eighteenth Century; Nature Study and Botany in the Eighteenth and Nineteenth Centuries; and a final chapter on Scientific Botany in the Nineteenth Century.

It is evident from these subjects that Jessen concerned himself not only with esoteric botany but also with the part that knowledge of plants played in the lives of peoples, intellectually, economically, and esthetically. This history of botany is not only a story of the evolution of plant science but also of plant utilization and appreciation: it is a record and evaluation of the impact of civilizations on the development of plant science and of the vital part that plants have played in the evolution of civilization; it is humanized and humanizing history.

E. C. STAKMAN

Department of Agriculture
University of Minnesota
St. Paul

RELIGIOUS EVOLUTION

The Heathens. William Howells. 306 pp. Illus. $3.75. Doubleday. New York.

RELIGION "is an expansion of the whole world of reality . . . an imaginative improvement . . . a set of earnest policies which a group of people adopts under an unconscious compulsion in order to tidy up their distraught relationships with one another and with the universe as they perceive it" (pp. 22–23). To describe and analyze this universal phenomenon among nonliterate peoples, the author has selected an array of topics, including mana and tabu, magic, divination, medicine and witchcraft, life after death, shamanism, and totemism, and has drawn his data from all five continents. Throughout the three hundred pages the author has written with authority, lucidity, and a handsome balance between details and perspective.

Some critical comments can always be made on any work, especially one that is popular and wide in scope. In the present instance, one may, for example, observe (1) that the author's division between primitive and civilized religions is not particularly valid (from the title of the book, the reader may easily misunderstand that all non-Christian religions are primitive); (2) that the four characteristic traits attributed to all "great faiths" (p. 5) chiefly apply to Christianity and Mohammedanism; (3) that although elsewhere cursory intrusion into Chinese beliefs is common, in the treatment of ancestor worship no reference is made to China, where the cult has been universal and is different in character from that found in Africa; (4) that little attention has been given to the mingled state of science (that is, "the understanding of the correct explanation or principle for anything at all" [p. 19]) and religion or magic (that is, belief or practice which is an "imaginative" expansion of the whole world of reality" [p. 23]) in human behavior not only among nonliterate but among all peoples; and (5) that the author's conclusion that religion cannot be classified because there is no boundary to human imagination (pp. 23, 215–16) would seem to be due to misunderstanding of the reality of the human mind, which is everywhere shaped by cultures.

These and a few other academic questions do not, however, impair in the least the great value of the present volume, which will prove to be fascinating and instructive to all readers. Professor Howells' book must rate among the best popular treatises on the subject.

Francis L. K. Hsu

Department of Anthropology
Northwestern University

SOUND MIND IN SOUND BODY

Mental Health in Modern Society. Thomas A. C. Rennie and Luther E. Woodward. 431 pp. $4.00. Commonwealth Fund. New York.

THE authors outline the purpose of this book as an attempt to add up wartime experiences with mental health, to take a look at the needs and treatments in the postwar world, to consider what education, planning, and reorganization are needed to provide proper and effective treatment to those who need it, and to build positive mental health. Both authors have had wide experience in the mental hygiene field, as well as in mental rehabilitation work, for the National Committee for Mental Hygiene.

The book, which is divided into three major divisions, is excellently edited. In Part One, entitled Lessons From the War Period, the authors discuss such subjects as mental health services in the armed forces, emergency measures for aiding veteran-civilian adjustment, the psychiatric disabilities of war and principles, methods, and results of treatment. This section is well documented with references.

In Part Two, postemergency problems in mental health, including the psychiatric residuals of war, are interestingly though briefly discussed.

In Part Three the authors attempt to integrate the various sources of help in treatment and prevention of mental illness. The contributions of the practicing physician, the social worker, the clinical psychologist, and the minister are ably presented. There follow a discussion of mental hygiene in industry, practical considerations in interviewing and counseling, mental hygiene of family living, and mental health in education. In the final chapter the authors give a summary and forecast entitled The Individual and Society, in which is developed a broad plan of building for positive mental health in modern society.

This Commonwealth Fund book will appeal not only to all workers in the mental hygiene field but to informed laymen who recognize the importance of an intelligent and positive approach to health and happiness.

Addison M. Duval, M.D.

Saint Elizabeths Hospital
Washington, D. C.

NATURE'S CROONERS

Voices of the Night. Recorded by the Albert R. Brand Bird Song Foundation. 4 vinylite records, 8 sides. $6.50. Comstock Pub. Ithaca, N. Y.

THE veteran herpetologist will be startled to hear the most familiar of the frog voices without being wet to the knees in a swamp or pond and searching for the musician in the circle of light from his headlight; he may even regret the degeneration of the times in which his younger colleagues may, with their feet dry, hear the sound medley of the swamp. We may be certain, however, that the remarkably fine album of frog voices of Eastern North America, now made available through the efforts of the Albert R. Brand Bird Song Foundation, of Cornell University, will in the long run stimulate more interest in the nocturnal world of the swamps and shores than all the accounts of frog hunts from the older generation.

The album consists of recordings of the voices of seven species of tree frogs (*Hyla*), four toads (*Bufo*), one spadefoot (*Scaphiopus*), one cricket frog (*Acris*), three swamp tree frogs (*Pseudacris*), and seven true frogs (*Rana*)—twenty-six species and subspecies in all. The reproduction of the voices of these forms is almost startlingly exact. It is only necessary to play them in the dark to recall field experiences to match them. The frog voices in the background, occasional bird voices, and other night sounds give particularly pleasing effects.

The voices of frogs have biological significance as examples of a "species character" of an essentially psychological nature that is frequently far more distinctive and more readily recognizable than any morphological characteristic. A few instances are known, also, of populations of well-known species in which the voice is distinctly different from the norm of the species, as has been reported for the common tree toad (*Hyla versicolor*) of the middle-Atlantic states. There are indications, also, in the little Solomon Island frog *Batrachyla* that there are distinct differences in voice from island to island, though the frogs themselves are otherwise quite indistinguishable. These matters bear on questions of speciation, since it has often been objected that incipient species differences would be too small to make natural selection effective. That the modifiability of voice without necessity of initial structural change affords an isolating mechanism of extraordinary significance seems to be attested vocally by the frog species of the world. Evolution in other physiological and structural respects may follow in due course.

These records should be in every school, and above all in the home of every naturalist, from nature lover or camper to biologist. There should be an immediate demand for a supplementary album of the distinctive voices of the frogs and toads of the West and Southwest, another of the voices of additional species of the South. We might then hope for samples of the nocturnal chorus of the tropics, which affords so immediate a challenge to every visiting naturalist. A most desirable addition to the series would be a set of nocturnal and diurnal insect sounds.

Over a period of some twenty years, when chance has condemned me to attend a class B motion picture in which the perfectly manicured and marcelled blond heroine was subjected to the terrors of the jungle, I have noted with some amusement that the jungle sound effects, whether intended for Malaya or Africa, have been the same—apparently even the very same record—instantly recognizable as the voice and chorus of the swamp tree frog of temperate North America, perhaps the most familiar frog voice in our continent. The motion-picture industry may now be referred to the Albert R. Brand Song Foundation for a new set of jungle sound records.

KARL P. SCHMIDT
Chicago Natural History Museum

BIOLOGY AND ETHICS

Life and Morals. S. J. Holmes. x + 232 pp. $3.00. Macmillan. New York.

IN THIS stimulating little volume Dr. Holmes, professor emeritus of zoology at the University of California, has set himself the task of revaluing the ethical content of Western culture in the light of the findings of the biological sciences. The enterprise is as formidably difficult as it is urgently necessary. Professor Holmes has made a provocative contribution toward a naturalistic morality as distinguished from value systems based on supernatural revelations and sanctions. His thesis is that ethical standards in our time must proceed not from the concept of man as a divine creation, but from that of man as a living organism and as a member of a society of living organisms, all of whom have the duty of preserving and perpetuating the species as their highest obligation. Professor Holmes' orientation is well put in a passage from his preface:

> Moral conduct in human society is viewed as having much the same functional significance as the cooperative behavior of the parts of the body in maintaining the life of the whole. In the adjustment of their egoistic and altruistic activities, these parts have to do quite precisely the right things, else they and their associates will quickly come to grief. Much the same relations obtain in a hive of bees and to a less degree in other social groups. The life of a human society also depends on the proper conduct of individuals in their several roles; at least, the behavior of individuals cannot be too antisocial or chaos, if not destruction, will soon result.

For all scientists—physical, biological, and social—this book is well worth reading. It is full of new insights and suggestive reformulations of age-old issues. It suffers, however, from a major defect. Dr. Holmes has not taken cognizance of the contributions of Sigmund Freud and his successors to an understanding of the structure and dynamics of human personality. An essay on ethics in pre-Freudian terms, even from the pen of an able biologist, is almost as quaint as an essay on biology in pre-Darwinian terms or an essay on economics in pre-Keynesian terms. This "blind spot" is stark and startling in these pages. It vitiates much of the value of what is here said, although the residue is still worth serious attention. It is to be hoped that Professor Holmes may someday find time and occasion to amplify and perfect his trenchant argument in the light of contemporary psychoanalysis and psychiatry.

FREDERICK L. SCHUMAN
Department of Political Science
Williams College

Ethics for the Atomic Age. Ana María O'Neill. 411 pp. $3.00. Meador Pub. Boston.

UNDER the shattering impact of events unleashed by the discovery and artificial liberation of atomic forces, scientists asked themselves whether a changed physical universe does not require a total reorientation in our moral universe. In a lively and readable book, the author tries to answer this perplexing question and thus to give a blueprint for an ethical society for our time. In a penetrating analysis of human nature she resolves the complex personality of man into an affinity for things and an affinity for correct solutions. Inasmuch as he, as a physical being, cannot escape the physical and biological laws of nature, as an intellectual entity he cannot disengage himself from the urge to find correct solutions for problems caused by the impact of his physical environment or contact with other human beings. In the most elucidating part of the work, "The Voice of Moral Engineers" is recorded in their endeaver to coordinate physical and intellectual urges:—through justice, the message of the Greek philosophers; through intelligence, the doctrine of American pragmatism; through duty, the essence of Kant's categorical imperative; through love as preached in the Sermon on the Mount. All these ethical precepts are rooted in the concept of man as an essentially moral being endowed with inalienable rights that presuppose a moral order above the political and economical organization of society. However, although the author succeeds in analyzing the interaction of physical and intellectual forces which determine our own moral attitude and the character of our society, she does not attempt to find an ethical solution for national conflicts so much more threatening in this atomic age. But it is exactly here where the lack of a unifying principle of sufficient moral power has become ever more apparent. Inasmuch as man will never reconcile himself to the coexistence of two worlds in a political sense, so he will never be satisfied with the concept of the separation of his universe into a material and a spiritual world. For his reason tells him there can exist but "One World," and he will never cease to integrate his scientific and moral experience into a physical and at the same time an ethical universe.

FRANCIS JOSEPH WEISS
Washington, D. C.

THOSE CLEVER PEOPLE

Farmers of 40 Centuries. (2nd ed.) F. H. King. 379 pp. $5.00. Rodale Press. Emmaus, Pa.

THIS book, published first in 1911, contains the observations of Dr. King during a trip of several months made for the purpose of studying agricultural practices in the Orient. Dr. King was an eminent agricultural scientist and a well-qualified observer. He presents a thorough account of the cropping practices, crop rotations, drainage and irrigation methods, and soil management procedures followed in parts of China and, to a lesser extent, in Korea and Japan. There are, in addition, numerous observations on living conditions, housing, and other phases of life in these countries.

The central theme of the book is the painstaking care of the Oriental farmers to maintain or enhance the fertility of their soils by saving and returning to the soil everything that might serve for that purpose. This includes farm manures, straw, ashes from the fires, soil dredged from ditches and canals, grass and other vegetation harvested in the hills, and even night soil from the cities. Dr. King stresses also the density of the population, limitations on arable land, and the need for producing more and more food. Now, almost forty years after the first edition of this book, these same practices are followed and the same problems remain. Agriculturalists who have been in Japan in recent years will be struck by the similarity of conditions today with those described by Dr. King nearly four decades ago.

The text is illustrated by 209 figures—photographs of scenes in China, Japan, and Korea. Some of the pictures are excellent. It is unfortunate that the editor or publishers did not use some selection, for many are too blurred to be useful.

This book should be a valuable reference for agriculturalists seeking knowledge of conditions in the Orient. It is doubtful if it will find a wide audience among lay readers. The style is ponderous in places, and there are too many statistics on production, fertilizer application, and monetary returns per acre. One might wish also that the book had been dated. There is no date of this edition and no reference, except on the dust jacket, to the fact that this is a second edition of a book published in 1911. Statistics on population, prices, acreages, production, etc. are for the early years of the twentieth century and do not apply today. The casual reader might easily be misled into considering them currently valid.

W. M. MYERS
U. S. Regional Pasture Research Laboratory
State College, Pennsylvania

PRACTICAL AESTHETICS

The Unfolding of Artistic Activity. Henry Schaefer-Simmern. 201 pp. Illus. $5.00. Univ. of California Press. Berkeley and Los Angeles.

IN SO FAR as aesthetics is a legitimate branch of inquiry, it seeks the answers to such questions as What are the psychological processes behind artistic production? What are the sociological factors conditioning taste? What is the cultural function of art forms in a given culture? This is not to say that writers in aesthetics must themselves be psychologists or anthropologists, any more than a philosopher of science need be a creative physicist. It is only to say that the task of aesthetics is similar to the task of the philosophy of science: the philosophy of science examines the writings of scientists to separate empirical and analytical elements, to arrive at what Carnap has called the rational reconstruction of science. Similarly, the philosophy of art has the assignment of examining the language of art for the purpose of determining which sentences of criticism are empirical, that is, derived from psychology and the social sciences; which statements are analytic, that is, definitions, derivations, and the like; and which statements are pragmatic, that is, requirements of purpose and intention. In short, the philosophy of art should attempt a reconstruction for the language of criticism.

It should do so. All this, then, is programmatic for aesthetics. This program is not well understood by some writers in this field (just as the program for the philosophy of science is not well understood by some philosophers), and this confusion leads to the disappointment of those who appeal to aesthetics for a rational interpretation of art.

This explanation makes it clear why Professor Schaefer-Simmern's book deserves the notice of scientists, philosophers, and all those who may have looked in vain to aesthetics for information of the fundamental sort about the artistic process. We wish to know from the aesthetical philosopher, for example, which alleged principles of form and "composition" are programmatic—whether "traditional" (program adopted from the art of the past) or "modern" (program urged by artists in the present)—and which alleged principles have a psychological function in what Schaefer-Simmern calls "visual conceiving," that is, a process growing out of the actual making of a work of art.

If there is such a psychological function of artistic activity, as apart from the conventions of representation of making surface structures that are only externally coherent, then it should be determinable by experiment and observation. Schaefer-Simmern describes four such experiments with untrained groups: one with mentally defective girls and boys, one with delinquent boys, one with refugees, and one with business and professional men and women. The process of learning to make a work of art is a very complicated one, and the experiments reflect this complication: we are shown, in actual visual facts, the problems met and solved by the would-be artists, from the first scribbled drawings to (months and years later) finished works of art on a high level of expression. The stages in these solutions are clearly distinguished and fully described. Each problem presents the novice artist with the need of finding his own solution in terms of the job actually before him, and the evidence seems clear that he finds it in his own stage of development in visual conceiving. That these stages are similar in respect to similar problems for all the persons involved is proved. Moreover, Schaefer-Simmern suggests that they are similar for all artists in all times and cultures—a most important law resulting from his thesis that these processes are psychologically determined by the problems themselves. It is this part of *The Unfolding of Artistic Activity* that is somewhat sketchy; but a man cannot say everything at once, and we can hope for a later definitive study from the author to amplify this part of his demonstration.

The demonstration in the present study is limited to a detailed description of the four experiments mentioned. Without such experimental data no amount of comparative aesthetics would be conclusive. If those in art academies who have a vested interest in teaching "composition" will suspect that Schaefer-Simmern has got out of his experiment a sort of common denominator primitivism because he instilled it there in the form of subtle precept to members of his groups, these critics have only to repeat the experiment and find out for themselves. That is the advantage of scientific work: it is self-correcting. Incidentally, the educational and other social advantages of duplicating this experiment on a wide scale seem so promising as to make one wish for the greatest possible exploitation of all opportunities. As John Dewey says, in his foreword to this book, the demonstration here "provides the pattern and model of the full and free growth of personality and of full life activity, wherever it occurs, bringing refreshment and, when needed, restoration."

William B. Holther

University of California
Berkeley

THE WORLD WE LIVE IN

Understanding Our Environment. Franklin B. Carroll. vii + 313 pp. Illus. Winston. Philadelphia.

Understanding Our World. Franklin B. Carroll. vii + 412 pp. Illus. Winston. Philadelphia.

Understanding Our Universe. Franklin B. Carroll. vii + 313 pp. Illus. Winston. Philadelphia.

IN THESE new, vivid books prepared for the seventh, eighth, and ninth grades, the principles and methods of science are thoroughly and fascinatingly described. The author has realized the problems connected with the preparation of such a series and has made an exceptionally successful presentation. The need for outstanding science instruction in the junior high school is very great, for it is at this time that the student must form a critical, "scientific" attitude in regard to the problems of his environment.

The science text has three main functions. Two of them are to stimulate the student's interest and participation in science and to impart in a flexible mind the merits and methods of scientific thinking. A scientific attitude is not a strange, impractical abstraction; it is merely logical thinking—something which can well be applied in many fields other than that of science. The science text should emphasize the practical, everyday aspect of "scientific method." The third function of the science text is to furnish a broad, yet concrete, background in the biological and physical sciences and to relate these facts to the panorama of modern life. These fundamentals can be presented only in a form familiar to the student. Abstract principles alone are not proper subjects for a junior high course. The ABCs of modern science, therefore, must be developed from things the student knows of and is curious about. The radio, the airplane, the flower, the skyscraper—these are the things that students think about. All of them must be used to develop the abstractions of scientific theory, principles which are, of course, quite necessary for advanced science instruction.

The "Interpreting Science" series seems to be above average on all three counts. The approach to subject matter is clear and practical. The text is filled with stimulating examples and illustrations, and students will be attracted by the numerous clear diagrams and pictures. Black-and-white and color photography are put to good use in promoting interest and clarifying subject matter.

From the first chapter to the last, the series emphasizes the methods we call "scientific thinking." The development of a scientific attitude is prompted in the student with the aid of many examples from everyday life. Thought-provoking questions at the end of each chapter provide opportunity for putting scientific thinking into practice. The ninth-grade text, especially, is highly informative, and the presentation of subjects is well planned. Above all, the books develop scientific method as a useful attitude. Says the author:

> The method of science is kept constantly before the

pupil's attention. Establishment of facts through observation is presented in experimental studies as well as through text and narrative. The organization of facts into a body of knowledge, their interpretation in a generalization at the pupil's level, the use of such generalizations in the interpretation of facts in a new situation, the framing and testing of hypotheses, receive attention at strategic positions. Solution of problems by scientists illustrates the method in use, and guidance in solution of problems provides opportunity for its exercise.

In view of the merits of this new series, it seems correct to appraise these books as outstanding examples of modern scientific texts.

ANDREW KENDE

Evanston, Illinois

NONTECHNICAL SURVEY

Handbook of Psychiatry. Winfred Overholser and Winifred V. Richmond. xii + 252 pp. $4.00. Lippincott. Philadelphia.

THE product of the collaborative efforts of a psychiatrist and a clinical psychologist, this volume presents a nontechnical survey of the various types of mental disease, their causes, symptoms, and prognosis.

The mental diseases discussed include the psychoses (the so-called insanities) and the relation of these severe behavior deviations to such organic factors as paresis, encephalitis, infantile paralysis, essential hypertension, brain tumors and injuries, alcohol, drugs, and the organic changes accompanying middle and old age. The functional psychoses (those without detectable organic damage) are then considered, followed by a discussion of the psychoneuroses—anxiety states, neurasthenia, hysteria, and obsessive-compulsive states—and their prevalence and importance in our daily affairs, including delinquency, crime, and war. A special section is devoted to a discussion of psychiatric conditions in children.

Related to the discussion of the symptomatology of each type of mental disorder is a section on causes, with special emphasis on a debunking of popular beliefs concerning the origins of mental disease.

Of greatest interest to the layman, however, will probably be the discussion concerning prognosis for each disorder and the description and evaluation of various psychotherapies. Here again, emphasis is placed on what cannot as well as what can be done, cautioning the lay reader against credulous acceptance of quick cures frequently promised by "psychological" quacks.

Since this volume is intended primarily for the nontechnical reader, it is perhaps unfortunate that the title, through its use of the word "handbook," implies an authoritative, technical summary of the subject matter for the specialist in mental disorders. For the specialist the book offers nothing new in either content or organization; for the layman it provides a sound survey of major behavior deviations. The book might have proved more valuable to the lay reader, however, had it included, besides the psychoses and psychoneuroses, a consideration of the psychological mechanisms employed by normal persons in their struggles with everyday problems.

This volume is one of the Non-Technical Scientific Series of the American Association for the Advancement of Science.

GEORGE F. J. LEHNER

Department of Psychology
University of California
Los Angeles

A MAJOR SOCIAL SCIENCE STUDY BY A BIOLOGIST

Sexual Behavior in the Human Male. Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin. xv + 804 pp. Illus. $6.50. Saunders. Philadelphia and London.

UNLIKE most scientific books, the so-called Kinsey report is by now familiar to millions of people. An unusually large number of scholars and laymen alike have bought the book; others have read reviews of it in scientific and popular journals; and many others are reading articles and pamphlets based on it. Before the cycle is completed, we can expect to see many books about the Kinsey report and many public discussions pro and con. It has had a tremendous impact on Americans of all walks of life. Why?

For one thing, it is about sex, and sex is always interesting. But there have been hundreds of books about sex that have received nothing like the attention given this one. There is more involved, therefore, than just the topic. The big difference, in fact, between this book and others in the field is that it tries to tell us much more about sex, and about sex in all sorts of men, than any previous book.

It tells us, for example, that there is great variation in the sexual outlet of male adults and that variant sexual patterns are much more common than has been supposed. It describes the rise and wane of sexual power in the course of a man's life. It gives us new insights into the types of outlet in different cultural, educational, and religious groups.

The discussion of these facts, and of many more like them, in this report, alters radically our conception of what is statistically "normal" in American male sex life. The many graphs and tables in the book bring out other details. Altogether, they paint a rather different picture of male sex life than most clinicians, scientists, and laymen have given us before. In fact, what has often been depicted as abnormal and delinquent sex behavior now seems, according to Kinsey, to be rather normal in the population as a whole.

But no one reads the book without asking the all-important question: Can we believe what we read? Did the Kinsey workers get a fair sample of the American male population, or did they get only the more boastful and exhibitionistic members? Can their interviewing methods be trusted? Are the methods capable of detecting the deception and outright lying inevitable in such a private matter as sex? On the answers to these questions hinges the real value of the book.

How fair is the sample? That is the first big question. The sample is large: it includes 5,300 cases. They represent all ages (five to ninety years), rural and urban societies, all educational levels (grade school through college), and all kinds of people, including prison inmates, male prostitutes, clerks, businessmen, college professors, and clergymen. If we look closely at the number of people of each kind and class, we see that they are not representative of the American population at large, however. There are too many special classes, such as the professions and prison inmates. There are too many in the Midwestern states and too few in other areas of the country. Is that bad? It probably is not, for Kinsey, well aware of the skews in his sample, consulted the U. S. Census statistics and, in making up his final figures and charts, usually weighted his groups according to their incidence in the general population. That is a fair correction to make and cannot be seriously criticized.

Once satisfied that Kinsey's statistics have been adequately corrected for disproportions in the classes of people studied, one can still ask whether the people he interviewed were representative of the groups from which they came. Did he get too many boasters, too many people preoccupied with their sex problems and only too willing to talk about them? That is a more difficult question to answer. Certainly Kinsey did his best. He tried to secure a fair cross section of people of various types. More than that, as a control against bias in his sample, he ran a good many so-called 100 percent tests; he worked until he had interviewed 100 percent of the members of a fraternity or an institution. When we compare the results of these complete studies with those in which only a small percentage of the group was interviewed, differences are noticeable, though not large. In fact, if we read such comparisons carefully, we must conclude that, if there is any bias toward boasters or unrepresentative individuals, it cannot be great. It cannot, in the main, change the general results very much.

The final question is: Can the interviewing method be trusted? Kinsey went to a great deal of pains to work out his interviewing method and then to put it to every possible test. Certainly the interviewing was comprehensive: it included a battery of 531 items, and it was so constructed that it contained many cross checks against inaccuracy and deceit. It is clear, too, that Kinsey and his interviewers used many clinical devices to guard against false information. Finally, many repeat interviews were given to the same individuals, separated by weeks, months, or years, and the agreement between repeat interviews, given by the same or different interviewers, was practically perfect. It is true that people have imperfect memories of their past behavior, especially when many years have gone by, and it is difficult to assess how much bad memory may have affected the results. When all is said and done, however, Kinsey's interviewing method must be considered fairly complete and the best and most reliable that has ever been used in this field. It is hard to see how the final statistical statement of his results could be very wide off the mark.

To this reviewer, then, the Kinsey report is an outstanding achievement. The methods were basically sound; they led to trustworthy results—more so than the vast majority of biological experiments. If they are in error, it is not by far. And they certainly give us the truest, most accurate picture that we have to date of sex behavior in the American male.

The implications of Kinsey's results for scientists, legislators, physicians, and public officers are tremendous. To the scientist, they make clear that man is a typical mammal, exhibiting the many variations in sexual behavior that we see in other mammals. To the physician, they point a warning not to construe many kinds of behavior as abnormal, nor as unduly important in the patient's mental or physical health. To the legislator, they say clearly that our current laws do not comply with the biological facts or normal sex behavior. And to our judges and public officers, they point the way to a much more intelligent handling of the sex offender and sexual delinquency.

Outstanding as it is, the Kinsey report is not finished. Kinsey and his collaborators have thousands of interviews of female sexual behavior that will appear before too long in another book. And they are at work collecting more data to make a much more careful analysis of the social and biological factors in sexual behavior. Eventually they plan to present a series of volumes on sex behavior. These will certainly modify and extend the conclusions of the first volume. The final result should be one of the most outstanding contributions of social and biological science to the welfare of millions of people in all walks of life.

C. T. MORGAN

Department of Psychology
The Johns Hopkins University

TECHNOLOGICAL NOTES

Centralization vs. decentralization. "Paying the freight" to make manufactured products the same price all over the nation seems threatened by the Supreme Court's recent decisions. Social planners may believe the result will be a scattering of factories. Just the opposite may be the case. Uniform prices have benefited the unindustrialized parts of the country. If Mississippi, not a heavy producer of either books or steel, must pay more for them than eastern Pennsylvania, she has small chance of building up her own publishing or metal-fabricating industries. Her people, already poor, must pay still more for the products of the East. Many factors must be considered. As just one example, the extra weight of glass in fluorescent tubes as compared with incandescent electric bulbs has bearing on the dispersion of lamp factories. New plants are going up near local markets. Another reason for their distribution is that they can tap the abundant supply of labor in small towns instead of adding to the congestion of cities.

Electrapane becomes warming. On windshields, various methods have been used to melt ice and dispel fog. One of the newest, used widely in the war and apparently proving useful in peace, is the deposit of a thin film of conductive oxide on the glass. Alternating or direct current bucking the resistance of the coating produces heat. The film is on the inside, next to the plastic filling of safety glass, so the conductor is not exposed. The casting is durable and practically invisible. Reflection is cut down by keeping the film thickness a multiple of one quarter of the wave length of yellow-green light. Libby-Owens-Ford, owner of the invention, calls the glass Electrapane.

Jet helicopters. Research on jet propulsion for helicopters is a new undertaking of the General Electric Company. The idea of jets on the ends of the blades of the helicopter seems novel, but it is a logical extension of the theory.

Fragile vitamins. Quick cooking with "radar" waves and other uses of high-frequency radiation in preparing foods may have some drawbacks. If the rays used in preserving or preparing the food destroy vitamins, the result may be a palatable shadow without much substance. The subject is being investigated at MIT.

Special glass. Big tanks of molten glass, constantly supplied with fresh batches of ingredients, make glass production for many purposes a continuous operation. Not so the special glasses used in electrodes for delicate chemical analyses. That kind of glass-making is entirely different. Leeds & Northrup electrodes take about a pound of glass a month. This important batch is especially compounded and melted in a small electric furnace by L & N workmen.

Tomorrow's train. Announcements of anything as belonging to "tomorrow" are dangerous. The term is exceedingly elastic. "Life in 1960" in *Kiplinger Magazine* relegated 1947's "Train of Tomorrow" to a Georgia branch line. A lighter, sleeker ultramodern train is suggested by the Chesapeake & Ohio Railway. It would do away with the independence of the individual cars. Each car would really be a trailer, 30 feet shorter than present cars, with one wheel truck at the rear. The low center of gravity, the trailing wheels, and the light weight would make 150 miles an hour possible, the designers think, and even on curves high speed and smooth riding could be obtained. Instead of each car's having its own generators and batteries, all such services would be concentrated next to the locomotive.

Magnesium Magazine. A few years ago the Dow Chemical Company was practically the only producer and promotor of magnesium to lighten metal products. It was made from Michigan brines or from sea water. Then came the war and the widespread use of magnesium alloys and production of the metal from dolomite in Ohio and from brucite at Las Vegas. Not so much magnesium is needed now, but magnesium continues its "march," according to the *Magazine of Magnesium* published by Brooks & Perkins of Detroit. A light mask for the baseball catcher may be of magnesium. Cabinets for delivering bread and cakes weigh an easily handled 68 pounds, against 270 pounds for similar designs in steel.

Strong and light. Crated freight increases shipping costs. Strong crates of "Versalite," a light thermosetting plastic made by the U. S. Rubber Co., and insulated with "Flotofoam," an expanded plastic that doesn't weigh a pound per cubic foot, are available as freight savers. Because the insulation is so good, quick shipments of frozen foods may dispense with ice, either dry or wet. Longer distances and times require inclusion of ice in the container.

Warm hearth. When the fireplace doesn't draw properly, some experienced fire builders spread a newspaper across the opening, concentrating air at the bottom. That procedure is a trifle dangerous and tiring. Moreover, it doesn't prevent the great loss of heat as warmed air is drawn out of the room, causing chilly drafts. A maximum of cheer and radiated heat, with a minimum of smoking and loss through drafts, is the claim of the makers of a glass fireplace-control screen. The louvres of plate glass can be turned down as desired to permit the fire to be seen and felt and still confine the departing air to a small space at the bottom of the fireplace opening. The makers, Dollinger Corporation, of Rochester, say that the glass screen will make a fireplace damper unnecessary.

Vernier slide rule readings. In the century and a quarter since the slide rule was invented, practically everything, presumably, has been discovered about this graphic application of logarithms. Nevertheless, a Miami University senior recently reported to the Ohio Academy of Science a possibility that seems not to be mentioned in the instructions for using the slide rule. A result on the *D* scale may be read to one more place if the corresponding part of the *C* scale—so adjusted that 10 of its divisions cover 9 of the divisions on the *D* scale—is brought up to the *D*-scale ruling in question. This student made the interesting discovery while he was still in high school. He finds it particularly useful with cheap and poorly graduated slide rules.

Light. Intensity, safety, and coolness are claimed for a miniature fluorescent microscope lamp by Burrell Technical Supply Co., of Pittsburgh. The highest heat is 120°, and delicate specimens are safe without dimming the light.

"The greatest invention since sunlight" is the mysterious slogan on a picture of the portable "Strobo-Lite" of Wilcox Photo Sales, Los Angeles. The output is compared favorably to that of a photoflash bulb. The weight of this two-lamp, battery-operated light source is 70 pounds.

Light on the school budget is a Bausch & Lomb microscope, stated to be standard in practically every way but priced at less than $100.

Thin film. Emulsions only one tenth as thick as a human hair are helpful in autoradiography—the subject "takes its own picture" by effect of its radiation on film. Radiation comes off in all directions, so extra thinness helps "pinpoint" the location; there is little room for travel in the film. Eastman makes it.

Closely related is Eastman's announcement that its emulsions have recorded the "track" of electrons, the streak made in the film as an electron travels about two thousandths of an inch, affecting 6–28 silver grains.

Magnetic clutch and iron powders. Making a smooth connection between a moving motor and running gear is a clutch problem with which every motorist is familiar. A Bureau of Standards development seems to be something new and promising in clutches. The space between the plate that will drive and the plate that will be driven is filled with oil containing a thick suspension of iron powder. When an electrical connection makes that space also a magnetic field, the iron particles line up and hold to each other and to the plates, making a smooth clutching action. A magnetic clutch idea would be worth little without suitable iron powders. Tiny granules of iron, made by General Aniline & Film Corporation for use in radio cores, have proved useful for the new clutch. The granules are made by reaction of carbon-monoxide gas and iron oxide.

Night guide. As many of us who have groped for a bedside flashlight know, the darkest of moments may seem an eternity before the article is found. Life is made a little brighter by availability of a flashlight with a case that, properly exposed to light during the day, glows at night. It is a Winchester product.

As *Paint Progress*, published by the New Jersey Zinc Company, reminds us, luminescence may be a valuable property of many other items in our environment. Faintly glowing guides contributed greatly to safety on blacked-out ships and in darkened towns during the war, and even in peacetime there are many places where luminescent paint may afford protection to more than the surface.

Protective protein. For mildly corrosive atmospheres, the National Bureau of Standards has developed a combination of protein and a chrome salt to protect metals. The two-step process has most possibilities. The article is dipped in a water solution of gelatin, albumin, or other protein and allowed to dry; then there is another dip in chromic acid or in dichromate of zinc, iron, or nickel. The film is slightly yellowish and transparent. It is hard, adheres well, is almost insoluble in water, and will stand heating up to 150° F. It is not as durable as paint, but it has advantages over greases and waxes.

Even on Pikes Peak. Raucous horns from trains on main-line railroads are vivid reminders of what some word coiners probably call "Dieselization." Almost everywhere the steam locomotive, with its panting exhaust and musical whistle, seems on the way out, although the C & O—a great coal hauler—is sponsor of a coal-burning steam-electric engine. Changes of this kind are not easy. Diesel fuel comes from oil, and oil has a much more uncertain future than coal—except that coal will probably be a source of oil in the future, although the expense will be greater. Locomotive factories must redesign and retool. Railroad roundhouses and workmen must be made over to fit the new locomotive. Diesel power has even come to the famous cog railway up Pikes Peak, the National Geographic Society reminds us. Last year two Diesel-electric locomotives took over from the old tilt-boiler puffers, now kept as stand-bys for extra-heavy traffic. The railroad was opened for business in 1891 and has carried more than a million passengers.

Business in building. Residence construction for personal use, known as "building a home," is often undertaken in something of an emotional state. But house construction is a business. Items of contract, and realism in dealing with architects and contractors, are discussed in a new circular of the Small Homes Council of the University of Illinois. This *Business Dealings* is the twentieth in the series of free pamphlets issued by the Council.

M. W.

COMMENTS AND CRITICISMS

WHAT IS TIME?

Hermann Lotze, in his *System der Philosophie* (part 2, 1879, "Metaphysik"), rejects an "abstracting comparison" which leads to the concept of a kind of general laws of nature "which first exist as valid and to which then a world would come submitting itself to them. . . . In this sense, we can find better justification in the often heard sayings according to which not time is the condition for acting [*Wirken*], but acting produces time." This, however, is not "a permanently remaining product, an actively real time," it is a subjective form of intuition.

The "often heard" argument contains one of those Copernican reversions which Kant claimed to have performed by his system and which are quite frequent in the development of science (see, e.g., my article in *Osiris*, 5, 1938). Thomson King ("On Time as a Product of Motion," SM, October 1948) describes as his discovery that "time is the product, not the cause, of events." Time, he says, is not "elemental," not "a primary reality, a something in itself, its definition as a product of motion gives it a reality that is consistent with the universal as we know it. . . ." The author does not explain how we can know motion without bringing to it the concept of time. How can motion produce time? The labors of centuries led to the concept of heat as a product of motion. In 1825, Berzelius still lists as "dynamides"—namely, something like force—substances without matter, heat, together with light, electricity, and magnetism. All four of them were found to be based on motion. Is time the fifth "product"?

In one of his tragedies, Strindberg introduces a young girl who says: "Time is that which flees. I flee; I am time!" King's argument is of about the same consistency.

Does the pendulum while swinging produce time? Is the amount of time increasing with every moving ship, train, plane? The author cannot mean this, although he writes: ". . . the time of any interval, a minute or a century, is the result of the summation, the integral of the motions that took place during the period." He seems to mean the universal Motion, the motion of the "billions of molecules," etc., which in totality are comprised under the laws of energy. Such an extrapolation belongs to the group of generalizations which, according to Ernst Mach and many others, are devoid of meaning when extended to the whole universe.

As a final proof, King lists a series of identities between time and motion without realizing that this means the contrary of his "discovery." The two are connected but not by one producing the other as an object of either "primary" or derived reality. The process does not take place without the human mind. The attempt to omit this intermediary was made long ago, at the "time" of the violent discussions between realists and nominalists in the eleventh and following centuries. Discussions of this rank were extended to the "reality" of forms and their "production" by matter, of the relationship between individuum and community, and finally of microcosmos and macrocosmos. Since then, we may have made progress in distinguishing observations and abstractions, between event and observer. Motion produces change of position and composition, it does not produce time as, say, yeast produces enzymes. Time is the mental, and sentimental, addition we bring into events. That this is done through objectivation and standardization, in short, through comparison, prevents this addition from being either arbitrary or a mere identity with motion.

EDUARD FARBER

Washington, D. C.

The article by Thomson King in the October issue is about as bad as his earlier one, "On Life as a Separate Entity" (February 1947). It would be better to solicit papers of this kind from philosophers rather than scientists—if Mr. King is one. For these are matters of abstraction and thought and not open to scientific study. . . .

Scientists study matters of fact or events, not categories of abstractions. I challenge Mr. King to show me a matter of fact or event called "life" or "time." The absence of clear philosophic thinking in Mr. King's paper emerges from such statements as ". . . time is a product of motion . . ." (p. 290); or ". . . a universe that contains matter, but has never had motion . . ." (p. 291); or ". . . time is the product, not the cause, of events" (p. 292).

The products of motion and events can only be again motion and events, but not time. Time is an aspect of motion and events, just as is space; in other words, an abstraction of our minds to which no real object corresponds. That this is so is proved by the results of Minkowski (whom Mr. King misunderstands in his quotation on page 292) and Einstein, for instance. I wonder whether Mr. King is going to present us with similarly learned papers on such abstractions as "mind," "color," "soul," or "taste." Perhaps he had first better read Plato, who treated of these things much more logically and poetically.

HANS TISCHLER

Roosevelt College of Chicago

NOT MERELY FOUR SQUARE WALLS

Even though there seems to be a vast difference of opinion between us, I thoroughly enjoyed Lindsay Lord's letter in your February issue. I can't accept his cost figures as typical of the industry, however.

Since Mr. Lord has raised the question, I do want to say that I have wandered through Williamsburg and Mount Vernon many times and that I have a very deep admiration for these old houses. My favorite pilgrimage is to Charlottesville and Monticello. Thomas Jefferson! There we have a true nonpareil, a designer whose aesthetic sensibilities are matched by his mechanical ingenuity, a gadgeteer with a genius for the beautiful!

The only drawback to such visits is that I always wind up with the deflating thought that after a century and a half we are still trying to imitate these architectural masterpieces of the past for their surface characteristics—and not doing too good a job at that. We completely miss the point that if these early designers were still alive they would be trying to do now what they were doing then—seeking to do the utmost with the materials at hand. . . .

If I understand Mr. Lord correctly, his standard of perfection in housing must always be at least twenty-five years old. This does not seem to me to be in the best American tradition. Certainly it cannot be called scientific.

C. THEODORE LARSON

Arlington, Virginia

INDEX VOLUME LXVII (JULY-DECEMBER 1948)

Title Index of Articles

AAAS Enters its Second Century, The, by H. Shapley, E. W. Sinnott, and E. C. Stakman, 242.
Ahimsa and Cattle Breeding in India, by B. H. Schneider, 87.
Are our Wars Good Times? by W. F. Ogburn and J. L. Adams, 23.

Babbage, Charles: A Scientific Gadfly, by C. F. Mullett, 361.
Bacterial Viruses (Bacteriophages), by W. H. Price, 124.
Basic Postulates of Psychology, The, by W. E. Vinacke, 110.
Bioluminescence: A Reaction Rate Tool, by F. H. Johnson, 225.
British Institutions for Soil Research, by Sir E. John Russell, 130.

Columbia University Research in Contemporary Cultures, by W. La Barre, 239.

Definitions and Principles of Dynamics, by J. H. Keenan, 406.
Descriptions of Rock Colors, by R. K. DeFord, 133.

Electrons and Computation, by W. J. Eckert, 315.
Effect of 2,4-D on Yields of Oleoresin from Slash and Longleaf Pines, by C. S. Schopmeyer, 440.
Epidemics and Bacteriological Warfare, by R. L. Mayer, 331.
Evolution of Terrestrial Life, The, by A. Gulick, 267.
Experimentally Induced Abnormal Behavior, by N. R. F. Maier, 210.

Famous Wilderness Highway, A, by K. Ver Steeg, 39.

Genes, Cytoplasm, and Environment in Paramecium, by T. M. Sonneborn, 154.

Hg^{198} as a Wave-Length Standard, by F. Benford, 66.
History of Population Growth in the United States, A, by P. K. Whelpton, 277.
Human Brain and its Development, The, by F. Weidenreich, 103.

Ideal Partnership, An, by L. R. Cleveland, 173.
Identification of the Best Southern Negro High-School Seniors, by J. M. Stalnaker, 237.
Importance of the Individual in Human Evolution, The, by H. F. Blum, 115.
Impotence Principles in Modern Physics, by R. B. Lindsay, 50.
Inevitability of Scientific Discovery, The, by A. J. Ihde, 427.
Is Mathematics an Exact Science? by N. A. Court, 119.

Livingston, Burton E., 1875–1948, by W. B. Mack, 34.

Magnificent Rodent, The, by H. R. Gregg, 73.
Malarial Parasites, by Q. M. Geiman and R. W. McKee, 217.
Meteorites, Relative Abundances, and Planet Structures, by H. Brown, 383.
Modern Clay Investigations, by A. Bevan, 443.

Naming the Gardenia, by M. Denny, 17.
Neanderthal Controversy, The: Nineteenth-Century Version, by J. W. Gruber, 436.
Nerves in Vivo, by C. C. Speidel, 178.
Nuclear Physics and High-Voltage Accelerators, by M. E. Tuve, L. R. Hafstad, and O. Dahl, 161.

On Living in the Biosphere, by G. E. Hutchinson, 393.
On Time as a Product of Motion, by T. King, 289.
Our Forests and Watersheds, by E. N. Munns, 346.
Outlet of Lake Bonneville, The, by R. L. Ives, 415.

Paradoxical Rivers of the Great Plains, The, by O. F. Evans, 54.
Petroleum and Natural Gas as Source Materials for Chemicals, by G. Egloff, 304.
Photography-in-Science Salon, 273.
Pineapples in Ancient America, by J. L. Collins, 372.
Plant Physiologist Looks at the Cancer Problem, A, by P. R. White, 187.
Plant Physiology and Recent Progress in Agriculture, by J. van Overbeek, 236.
Population Phenomena and Common Knowledge, by L. C. Cole, 338.
Probability, Rarity, Interest, and Surprise, by W. Weaver, 390.
Problem of the Helicopter, The, by A. Klemin, 127.
Protoplasmic Contractility, by D. Marsland, 193.

Science of Sanitation, The, by W. A. Simanton, 378.
Scientific and Technical Enlisted Men in the Army, by M. W. White, 47.
Seven Degrees of Adventure, The, by M. Bates, 254.
Sheffield Scientific School, by C. H. Warren, 58.
Significant Decade in Science, A, by J. W. Oliver, 83.
Social Aspects of Housing, by P. C. Glick, 355.
Some Accomplishments and Limitations of Reaction Rate Theory, by H. Eyring, 183.
Some Problems of Plant Nutrition, by D. R. Hoagland and D. I. Arnon, 201.
Sound Waves and Rhythms, by V. O. Knudsen, 430.

Teamwork in the Social Sciences, by M. J. Williams, 64.
Technology and the Changing Socioeconomic Structure, by A. J. Jaffe, 93.
Thirty Years of Mass Spectroscopy, by A. J. Dempster, 145.
Three Methods of Voting, by W. S. Taylor, 297.
Time and Change in the Metagalaxy, by H. Shapley, 243.
Tissues in Infection and Immunity, The, by R. L. Kahn, 162.
Trees, Shoes, and Peron, by W. M. Harlow, 240.
Tropical Medicine—A Partial Report, by E. H. Payne, 302.
Twentieth-Century Navigation, by N. C. Gerson, 257.
Two Telescopes and the New Universe, by R. S. Underwood, 5.

What is Natural Selection? by S. J. Holmes, 324.
Wood in an Industrial World, by J. A. Hall, 398.

Subject Index

AAAS, 242.
Abnormal behavior, 210.
Adventure, 254.
Agriculture, 201, 236.
Anthropology, 103, 239, 436.
Antigens, 154, 162.
Astronomy, 5, 243.

Bacteriological warfare, 331.
Bacteriology, 124, 331.
Bacteriophages, 124.
Beavers, 73.
Biochemistry, 217, 267.
Biology, 154, 193, 225.
Bioluminescence, 225.

Botany, 372.
Brain, human, 103.
British research, 130.

Calculating machines, 315.
Cancer, 187.
Cattle breeding in India, 87.
Cepheids, 5, 243.
Chemistry, 183, 225, 304.
Clay, 443.
Computing machines, 315.
Conservation, 346.
Crown gall, 187.
Crytocercus punctulatus, 173.

Dynamics, 406.

Ecology, 338.
Education, 237.
Electronic devices, 315.
Epidemics, 331.
Erosion, 346.
Evolution, 115, 267, 324.

Genes, 154.
Geography, 415.
Geology, 39, 133, 415, 443.
Grand Portage, 39.

Harvard College Observatory, 243.
Helicopter, the, 127.
History of science, 17, 39, 58, 83, 145, 267, 331, 361, 372, 406, 415, 427, 436.
Housing, 355.

Induced abnormal behavior in rats, 210.
Isotopes, 145, 201.

Linnaeus, 17.
Livingstone, Burton E., 34.

Magellanic Clouds, 5, 243.
Malarial parasites, 217.
Mass spectroscopy, 145.
Mathematics, 119, 315, 390.
Medicine, 162, 217.
Metagalaxy, the, 243.
Meteoritics, 383.
Milky Way, 5, 243.
Mount Wilson Observatory, 5.
Mutations, 124, 154.

Natural history, 73.
Navigation, 257.
Nebulae, 5, 243.
Negro, the Southern, 237.
Nerve fibers, tadpole, 178.
Neuroses in animals, 210.

Palomar Observatory, 5.
Paramecia, 154.
Peking man, 103.
Pepsi-Cola Scholarship Board, 237.
Petrochemicals, 304.
Philosophy, 289.
Photography in science, 273.
Photosynthesis, 201.
Physics, 50, 145, 161, 430.
Pineapples, 372.
Plant hormones, 236.
Plant nutrition, 201.
Plant physiology, 187, 236.
Population, 277, 338.
Pressure experiments, 193.
Protozoa, 173.
Psychology, 110, 210.

Quebracho, 240.

Radiolocation, 257.
Reaction rates, 183, 225.
Rivers, behavior of, 54.
Rivers, Great Plains, 54.
Rock colors, 133.
Rothamsted Experimental Station, 130.
Royal Society, 17, 361.

Sanitation, 378.
Science, history of, 17, 39, 58, 83, 145, 267, 331, 361, 372, 406, 415, 427, 436.
Science, Indian, 87.
Science in the Army, 47.
Selective Sequence Electronic Calculator, 315.
Silliman, Benjamin, 58.
Sociology, 64, 93, 237, 239, 277, 355.
Soils, 130.
Symbiosis, 173.
Synthetic chemicals, 304.

Tadpole nerve fibers, 178.
Tannin, 240.
Technology, 93.
Thermodynamics, 50, 289, 406.
Time, 289.
Tissue immunity, 162.
Tropical medicine, 217, 302.
2,4-D, 236, 440.

Voting Procedures, 297.
Voyageurs' Highway, 39.

War, 23, 47, 331.
Watersheds, 346.
Wave-length standards, 66.
Wood, 398.
World's Natural Resources, 393, 398.

Yale College, 58.

Contributors

ADAMS, J. L.: *See* OGBURN, W. F.

BATES, M.: The Seven Degrees of Adventure, 254.
BENFORD, F.: Hg^{198} as a Wave-Length Standard, 66.
BEVAN, A.: Modern Clay Investigations, 443.
BLUM, H. F.: The Importance of the Individual in Human Evolution, 115.
BROWN H.: Meteorites, Relative Abundances, and Planet Structures, 383.

CLEVELAND, L. R.: An Ideal Partnership, 173.
COLE, L. C.: Population Phenomena and Common Knowledge, 338.
COLLINS, J. L.: Pineapples in Ancient America, 372.
COURT, N. A.: Is Mathematics an Exact Science? 119.

DAHL, O.: *See* TUVE, M. A.
DEFORD, R. K.: Description of Rock Colors, 133.
DEMPSTER, A. J.: Thirty Years of Mass Spectroscopy, 145.
DENNY, M.: Naming the Gardenia, 17.

ECKERT, W. J.: Electrons and Computation, 315.
EGLOFF, G.: Petroleum and Natural Gas as Source Materials for Chemicals, 304.
EVANS, O. F.: The Paradoxical Rivers of the Great Plains, 54.
EYRING, H.: Some Accomplishments and Limitations of Reaction Rate Theory, 183.

GEIMAN, Q. M.: Malarial Parasites, 217.
GERSON, N. C.: Twentieth-Century Navigation, 257.
GLICK, P. C.: Social Aspects of Housing, 355.
GREGG, H. R.: The Magnificent Rodent, 73.
GRUBER, J. W.: The Neanderthal Controversy: Nineteenth-Century Version, 436.
GULICK, A.: The Evolution of Terrestrial Life, 267.

HAFSTAD, L. R.: *See* TUVE, M. A.
HALL, J. A.: Wood in an Industrial World, 398.

Harlow, W. M.: Trees, Shoes, and Peron, 240.
Hoagland, D. R.: Some Problems of Plant Nutrition, 201.
Holmes, S. J.: What is Natural Selection? 324.
Hutchinson, G. E.: On Living in the Biosphere, 393.

Ihde, A. J.: The Inevitability of Scientific Discovery, 427.
Ives, R. L.: The Outlet of Lake Bonneville, 415.

Jaffe, A. J.: Technology and the Changing Socioeconomic Structure, 93.
Johnson, F. H.: Bioluminescence: A Reaction Rate Tool, 225.

Kahn, R. L.: The Tissues in Infection and Immunity, 162.
Keenan, J. H.: Definitions and Principles of Dynamics, 406.
King, T.: On Time as a Product of Motion, 289.
Klemin, A.: The Problem of the Helicopter, 127.
Knudsen, V. O.: Sound Waves and Rhythms, 430.

La Barre, W.: Columbia University Research in Contemporary Cultures, 239.
Lindsay, R. B.: Impotence Principles in Modern Physics, 50.

Mack, W. B.: Burton E. Livingstone, 1875–1948, 34.
McKee, R. W.: *See* Geiman, Q. M.
Maier, N. R. F.: Experimentally Induced Abnormal Behavior, 210.
Marsland, D.: Protoplasmic Contractility, 193.
Mayer, R. L.: Epidemics and Bacteriological Warfare, 331.
Mullett, C. F.: Charles Babbage, 361.
Munns, E. N.: Our Forests and Watersheds, 346.

Ogburn, W. F.: Are our Wars Good Times? 23.
Oliver, J. W.: A Significant Decade in Science, 83.

Payne, E. H.: Tropical Medicine—A Partial Report, 302.
Price, W. H.: Bacterial Viruses (Bacteriophages), 124.

Russell, E. J.: British Institutions for Research in Soils and Crop Production, 130.

Schneider, B. H.: *Ahimsa* and Cattle Breeding in India, 87.
Schopmeyer, C. S.: Effect of 2,4-D on Yields of Oleoresin from Slash and Longleaf Pines, 440.
Shapley, H.: Time and Change in the Metagalaxy, 243.
Sonneborn, T. M.: Genes, Cytoplasm, and Environment in Paramecium, 154.
Speidel, C. C.: Nerves in Vivo, 178.
Stalnaker, J. M.: Identification of the Best Southern Negro High-School Seniors, 237.

Taylor, W. S.: Three Methods of Voting, 297.
Tuve, M. A.: Nuclear Physics and High-Voltage Accelerators, 161.

Underwood, R. S.: Two Telescopes and the New Universe, 5.

Van Overbeek, J.: Plant Physiology and Recent Progress in Agriculture, 236.
Ver Steeg, K.: A Famous Wilderness Highway, 39.
Vinacke, W. E.: The Basic Postulates of Psychology, 110.

Warren, C. H.: Sheffield Scientific School, 58.
Weaver, W.: Probability, Rarity, Interest, and Surprise, 390.
Weidenreich, F.: The Human Brain and its Development, 103.
Whelpton, P. K.: A History of Population Growth in the United States, 277.
White, M. W.: Scientific and Technical Enlisted Men in the Army, 47.
White, P. R.: A Plant Physiologist Looks at the Cancer Problem, 187.
Williams, M. J.: Teamwork in the Social Sciences, 64.

Books Reviewed

Alexander, Jerome: *Life, Its Nature and Origin*, 138.
American People, The, by Geoffrey Gorer, 137.
Australian Bird Life, by Charles Barrett, 139.

Barrett, Charles: *Australian Bird Life*, 139.
Beginner's Guide to Wild Flowers, by Ethel Hinckley Hausman, 380.
Beginnings of Modern Science, The. Holmes Boynton, Ed., 68.
Bell, A. E.: *Christian Huygens and the Development of Science in the Seventeenth Century*, 69.
Birds of Prey of Northeastern North America, by Leon August Hausman, 135.
Botanik der Gegenwart und Vorzeit in Culturhistorische Entwickelung, by Karl F. W. Jessen, 445.
Boynton, Holmes: *The Beginnings of Modern Science*, 68.
Business of Farming, The, by Herrell DeGraff and Ladd Haystead, 136.

Carroll, Franklin B.: *Understanding our Environment*, 449; *Understanding our World*, 449; *Understanding our Universe*, 449.
Chemical Russian Self-Taught, by James W. Perry, 380.
Conklin, Groff: *A Treasury of Science Fiction*, 140.

Darwin, Charles, by Jean Rostand, 309.
DeGraff, Herrell, and Ladd Haystead: *The Business of Farming*, 136.

Economics of the Mount Hagen Tribes, New Guinea, by Abraham L. Gitlow, 381.
Ethics for the Atomic Age, by Ana María O'Neill, 447.

Farmers of 40 Centuries, by F. H. King, 448.
Frontiers of Flight, by George W. Gray, 308.

Genius of Industrial Research, The, by D. H. Killeffer, 308.
Gershevsky, Noah D.: *Scientific Russian Reader*, 380.
Gitlow, Abraham L.: *Economics of the Mount Hagen Tribes, New Guinea*, 381.
Gorer, Geoffrey: *The American People*, 137.
Gray, George W.: *Frontiers of Flight*, 308.
Green World of the Naturalists, The. Victor Wolfgang von Hagen, Ed., 70.
Growth of Physical Science, The, by Sir James Jeans, 70.

Halliday, James L.: *Psychosocial Medicine*, 139.
Handbook of Psychiatry, by Winfred Overholser and Winifred V. Richmond, 450.
Hausman, Ethel Hinckley: *Beginner's Guide to Wild Flowers*, 380.
Hausman, Leon August: *Birds of Prey of Northeastern North America*, 135.
Heathens, The, by William Howells, 445.
Holmes, S. J.: *Life and Morals*, 447.
Howells, William: *The Heathens*, 445.
Huygens, Christian, and the Development of Science in the Seventeenth Century, by A. E. Bell, 69.

Jeans, Sir James: *The Growth of Physical Science*, 70.
Jessen, Karl F. W.: *Botanik der Gegenwart und Vorzeit in Culturhistorische Entwickelung*, 445.

Killeffer, D. H.: *The Genius of Industrial Research*, 308.
King, F. H. *Farmers of 40 Centuries*, 448.
Kinsey, Alfred C.: *Sexual Behavior in the Human Male*, 450.

Life and Morals, by S. J. Holmes, 447.
Life, Its Nature and Origin, by Jerome Alexander, 138.

Martin, Clyde E.: *Sexual Behavior in the Human Male,* 451.
Mathematics: Our Great Heritage. William L. Schaaf, Ed., 381.
Mental Health in Modern Society, by Thomas A. C. Rennie and Luther E. Woodward, 446.
Miller, Benjamin F.: *You and Your Doctor,* 140.
Mineral Resources in the United States, Bureau of Mines and Geological Survey, 135.

O'Neill, Ana María: *Ethics for the Atomic Age,* 447.
Organic Form and Related Biological Problems, by S. J. Holmes, 309.
Osborn, Fairfield: *Our Plundered Planet,* 310.
Our Plundered Planet, by Fairfield Osborn, 310.
Overholser, Winfred: *Handbook of Psychiatry,* 450.

Perry, James W.: *Chemical Russian Self-Taught,* 380.
Pomeroy, Wardell B.: *Sexual Behavior in the Human Male,* 450.
Psychosocial Medicine, by James L. Halliday, 139.

Rennie, Thomas A. C.: *Mental Health in Modern Society,* 446.
Richmond, Winifred V.: *Handbook of Psychiatry,* 450.
Rostand, Jean: *Charles Darwin,* 309.
Royal Society, The: Newton Tercentenary Celebration, 71.

Schaaf, William L.: *Mathematics: Our Great Heritage,* 381.
Schaefer-Simmern, Henry: *The Unfolding of Artistic Activity,* 448.
Scientific Russian Reader, by Noah D. Gershevsky, 380.
Schwab, George: *Tribes of the Liberian Hinterland,* 138.
Sexual Behavior in the Human Male, by Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin, 450.
Swedenborg, Emanuel, Scientist and Mystic, by Signe Toksvig, 68.

Toksvig, Signe: *Emanuel Swedenborg, Scientist and Mystic,* 68.
Treasury of Science Fiction, A. Groff Conklin, Ed., 140.
Tribes of the Liberian Hinterland, by George Schwab, 138.

Understanding our Environment, by Franklin B. Carroll, 449.
Understanding our Universe, by Franklin B. Carroll, 449.
Understanding our World, by Franklin B. Carroll, 449.
Unfolding of Artistic Activity, The, by Henry Schaefer-Simmern, 448.

Voices of the Night, recorded by the Albert R. Brand Bird Song Foundation, 446.

Von Hagen, Victor Wolfgang: *The Green World of the Naturalists,* 70.

Woodward, Luther E.: *Mental Health in Modern Society,* 446.

You and Your Doctor, by Benjamin F. Miller, 140.

Reviewers

Bascom, W. R.: 138.
Bell, J. H.: 308.

Cunningham, T. H.: 135.

Deming, H. G.: 138.
Dobzhansky, T.: 309.
Duval, Addison M.: 446.

Goldstein, M. S.: 140.

Hall, A. G.: 310.
Hedgpeth, J. W.: 309.
Henney, H. J.: 136.
Heyl, P. R.: 68, 381.
Holther, W. B.: 448.
Hsu, F. L. K.: 445.

Ipatieff, V.: 380.

Kende, A.: 449.

Lehner, G. F. J.: 450.
Lewis, O.: 137.
Luyten, W. J.: 69.

Meyerhoff, H. A.: 135.
Morgan, C. T.: 450.
Myers, W. M.: 448.

Perry, J. W.: 380.
Powers, P. N.: 70.

Ricker, P. L.: 380.

Schmidt, K. P.: 446.
Schramm, R. M.: 140.
Schuman, F. L.: 447.
Snyder, M. B.: 70.
Spoehr, A.: 381.
Stakman, E. C.: 445.
Stetson, H. T.: 71.

Weiss, F. J.: 447.
Wenger, P.: 139.
Wittke, C.: 68.
Wolf, R. F.: 308.
Woolfolk, E. J.: 139.

Verse

Barry, Francis: Time, 266.

Cook, H. L.: The Scientist, 109.

Goodman, Mae Winkler: Daylight Saving, 102; Cowbird, 177.

Grimm, G. W.: The Great Event, 414.

Hirsh, Joseph: Symphony at Sea, 160; Winter Wind, 435.

Hoffman, Henry A.: Physicist's Winter, 323.

Kenny, Carmen: Crystallization, 200.

Moore, Robert Thomas: Remnants, 33.

Ryder-Smith, Roland: Astronomer, 253.

Wylie, Clarence R., Jr.: Paradox, 63; At the Museum, 86.

Comments and Criticisms

Davis, W. A.: *See* Havighurst, R. J.

Farber, Eduard: What is Time? 454.

Gulick, A.: The Scientist as Philosopher, 314.

Havigurst, R. J., and Davis, W. A.: Can Intelligence be Measured? 312.

Larson, T. Theodore: Not Merely Four Square Walls, 454.

Luckiesh, M.: Of Error, a Trace, 314.

Otis, A. S.: Can Intelligence be Measured? 312.

Tischler, Hans: What is Time? 454.

Weed, Merrill: Technological Notes, 311, 452.

I. A. R. I. 75.

IMPERIAL AGRICUL[illegible]SEARCH
INSTITUTE [illegible]ARY
NEW DEL[illegible]

Date of issue.	Date of issue.	Date of issue.